VAN NOSTRAND'S

ENGINEERING MAGAZINE.

VOLUME XXIX.

JULY–DECEMBER.

1883.

NEW YORK:
D. VAN NOSTRAND, PUBLISHER
23 MURRAY STREET AND 27 WARREN STREET (UP STAIRS).
1883.

CONTENTS.

VOL. XXIX.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXV.—JULY, 1883.—VOL. XXIX.

CALIBRATION OF THERMOMETER GREEN 5280, BY HANSEN'S METHOD.

CHARLES C. BROWN, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

1. DESCRIPTION.—Thermometer Green 5280 is 20 in. long, with bulb 1.5 in. long, and 0.25 in. in diameter; stem, 18.5 in. long and 0.25 in. in diameter, and bore 17.75 in. long, graduated on the glass to 5ths of a degree F., from 17° to 214°, the length of a graduated degree being about 0.07 in. The distance from the middle of the bulb to the 32° mark is 4.1. in. The stem has a pear-shaped reservoir at the end opposite the bulb, capable of containing a column of mercury about 200° long.

2. CALIBRATION.—*Methods of Observation.*—Calibration corrections were desired for points differing by 5° from 32° to 212°. As required by Hansen's method, and as most convenient for Neumann's method, all possible readings at both ends of all lengths of columns differing by 5°, from 5° to 175°, were made at or near all graduation-marks differing by 5° from 32° to 212°. The column lengths were read running the column of mercury from 32° to 212°, and immediately after running in the opposite direction. The mean of the two column-lengths obtained between any two graduations was taken, thus eliminating any slight effect of change of temperature during the time of making the readings. The resulting observation-equations are of equal weight. The bore is assumed to be uniform for the short distances, generally not exceeding 0°.2 between the end of the columns as read and the nearest degree graduation mark. Readings were made with a microsope with thermometer horizontal. No difficulty was experienced in obtaining any desired length of column within 0°.1 to 0°.3 except in case of columns less than 20° or more than 150° to 160° in length, by the following method: A column of mercury 200° to 250° in length was easily obtained by making that amount of mercury run into the stem, a few slight jars being sufficient to start the column moving; a little manipulation then brought the empty space in the bulb to the junction of the bulb and the stem, when a sudden turn of the thermometer upright broke off the column, and almost as sudden an inversion preserved it. When too large a space was left in the bulb to be filled by heating the thermometer to 140° or 150°. a few drops of mercury were allowed to drop off the end of the column in the stem held upright, good care being taken to stop the operation before the column joined the mercury in the bulb. Then,

the thermometer being heated until the the mercury from the bulb began to appear in the stem, the column already in the stem was run down carefully, and partially joined to the mercury in the bulb, leaving a small bubble on one side of the column; the thermometer being allowed to cool slowly until the desired length of column above this bubble (which remains very nearly stationary) was obtained. The column was broken off at the bubble by a slight twitch or jar. If there are objections to heating the thermometer above a certain temperature column lengths above 10° to 20° or 30° longer than the number of degrees of that temperature, depending on the distance of the 32° point from the bulb, can be obtained by jarring off small drops of mercury from a long column into the reservoir at the top of the stem. It requires much more time, care and patience to obtain a column in this way than in the other. Columns more than about 160° in length were so obtained in this case. It is rather difficult to break off short columns, 5° to 15° in length in the manner first described, the weight of mercury in the short column not giving momentum enough to move it away from the rest of the column readily. A little patience is all that is necessary, however. To be able to read the shorter columns at 32°, a column 10° to 50° long, depending on the temperature at the time, must be broken off and put into the reservoir at the upper end of the tube, out of the way.

Hansen's Method.—Hansen's first method of determination of the graduation-errors of a standard of length, as worked out in his paper "Von der Bestimmung der Theilungsfehler eines gradlinigen Maassstabes," is the one here applied to the calibration of thermometers. The other methods there described may be applied in a similar manner. We wish to obtain the corrections to the thermometer readings. But, for the moment, let (0), (1), (2), &c., to (n) represent the corrections to the graduation-marks at the 32°, &c., &c., to 212°, points respectively (being the corrections at 32°, 52°, 72°, &c., 212° degrees for the short example following, and at 32°, 37°, 42°, &c., 212° for the full work on the thermometer Green 5280). Let w represent the volume of a column of mercury used, being a constant for that column, and i the true (corrected) volume of the bore for the corresponding graduated spaces, also a constant; or, in linear measure, let w_0, w_1, w_2, &c., represent the lengths of the column of mercury used (of constant volume) in the several positions along the tube, these lengths being unequal, owing to the *differences in bore* of the tube, and let i_0, i_1, i_2, &c., represent the lengths of the corresponding graduated spaces, directed to make the included volumes of bore (i) equal, (which lengths will also be unequal, owing to the *same differences in bore* of the tube); and let a_0, a_1, a_2, &c., represent the observed differences of length between the column-length and the graduated spaces, taken as positive when the graduated space is the longer. We shall have from each column-length read in each position, an observation-equation of the form

$$-(0)+(1)+w_0+a_0=i_0$$
$$-(1)+(2)+w_1+a_1=i_1$$
&c., &c.,

in which $w_0-i_0=w_1-i_1=$&c.$=$a constant throughout a set of observation-equations obtained for one column, the differences of the respective w's and i's from a constant length being due to the same causes respectively. But we wish the corrections to *thermometer-readings*, which will be equal in amount but opposite in sign to the corrections to graduation-marks. To transform the above equations into the proper form for obtaining the corrections to thermometer-readings represent the corrections to *thermometer-readings* by (0), (1), (2), &c., to (n), and change the signs of the remaining terms. Now represent the constant ($=i_0-w_0=i_1-w_1=$&c.) by m, and represent the a's (which are now positive when the column-length is the longer) by $[0]_0$, $[1]_0$, &c., $[0]_1$, $[1]_1$, &c., when the quantity in brackets represents the number of the graduation at the lower end of the column, and the subscript represents the number of the column. $[0]_0$, in the shorter example given, is the a corresponding to the reading of the lower end of the 20° column near the 32° mark, $[1]_1$ that corresponding to the reading of the lower end of the 40° column near the 52° mark, &c., $[0]_0$ in the

longer example corresponds to the reading of the 5° column on the 32° mark, $[1]_1$ of the 10° column on the 37° mark, &c.) We shall then have series of observation-equations as follows:

For the first column,

$$1.\ -(0)+(1)+m_0+[0]_0=0$$
$$2.\ -(1)+(2)+m_0+[1]_0=0$$

&c., &c., to

$$n-1.\ -(n-2)+(n-1)+m_0+[n-2]_0=0$$
$$n.\ -(n-1)+\ n\ +m_0+[n-1]_0=0$$

For the second column,

$$1.\ -(0)+(2)+m_1+[0]_1=0$$
$$2.\ -(1)+(3)+m_1+[1]_1=0$$

&c., &c., to

$$n-2.\ -(n-3)+(n-1)+m_1+[n-3]_1=0$$
$$n-1.\ -(n-2)+\ (n)\ +m_1+[n-2]_1=0$$

and so on, till

For the $(n-1)$th column,

$$1.\ -(0)+(n-1)+m_{n-2}+[0]_{n-2}=0$$
$$2.\ -(1)+\ (n)\ +m_{n-2}+[1]_{n-2}=0$$

And for the nth column,

$$1.\ -(0)+\ (n)\ +m_{n-1}+[0]_{n-1}=0$$

in which n is the number of columns used in the determination. The last equation is of no use in practice, and its $[0]_{n-1}$ is made equal to zero, and retained for symmetry in the following tables.

Forming the normal equations, Hansen obtains by his method of solution the following general formulæ, from which, by substitution of the observed quantities, the corrections to the thermometer-readings result.

(1). Let

$$\{0\}'=[0]_0+[1]_0+[2]_0+\ldots\ldots+[n-2]_0+[n-1]_0$$

$$\{1\}'=[0]_1+[1]_1+[2]_1+\ldots\ldots+[n-2]_1$$

&c., &c., to

$$\{n-2\}'=[0]_{n-2}+[1]_{n-2}$$
$$\{n-1\}'=[0]_{n-1}$$

(2). and

$$\{0\}=-[0]_0-[0]_1-[0]_2\ldots\ldots-[0]_{n-3}]-[0]_{n-2}-[0]_{n-1}$$

$$\{1\}=\ [0]_0-[1]_0-[1]_1\ldots\ldots-[1]_{n-4}-[1]_{n-3}-[1]_{u-2}$$

$$\{2\}=\ [0]_1+[1]_9-[2]_0\ldots\ldots-[2]_{n-5}-[2]_{n-4}-[2]_{n-3}$$

&c., &c., to

$$\{n-2\}=[0]_{n-3}+[1]_{n-4}+[2]_{n-5}\ldots\ldots+[n-3]_0-[n-2]_0-[n-2]_1$$

$$\{n-1\}=[0]_{n-2}+[1]_{n-3}+[2]_{n-4}\ldots\ldots+[n-3]_1+[n-2]_0-[n-1]_1$$

$$\{n\}=[0]_{n-1}+[1]_{n-2}+[2]_{n-3}\ldots\ldots+[n-3]_2+[n-2]_1+[n-1]_0$$

Let k be the symbol for the number of any graduation-mark, p $(=\frac{1}{2}n)$ for the middle graduation-mark if n is even, and q $(=\frac{1}{2}(n-1))$ for the graduation next below the middle if n is odd. Then let $(s,\ k)$ represent the sum of two corrections at points equi-distant from the end-points, and $(d,\ k)$ their difference, k taking its value from the lower graduation, thus:

$$(s,\ 0)=(0)+(n) \qquad (d,\ 0)=(0)-(n)$$
$$(s,\ 1)=(1)+(n-1) \qquad (d,\ 1)=(1)-(n-1)$$

&c., to &c., to

$$(s,p-1)=(p-1)+(p+1) \qquad (d,p-1)=(p-1)-(p+1)$$
$$(s,p)=2(p) \qquad (d,p)=0$$

or to or to

$$(s,q)=(q)+(q+1) \qquad (d,q)=(q)-(q+1)$$

(3). Let $S_k=\{k\}+\{n-k\}$
to $S_p=2\{p\}$
or to $S_q=\{q\}+\{q+1\}$

(4). $D_k=\{k\}-\{n-k\}$
to $D_{p-1}=\{p-1\}-\{p+1\}$
or to $D_q=\{q\}-\{q+1\}$

(5). and

$$K_k=(k+1)\{k\}'+(n-k)\{n-k-1\}'$$

to

$$K_{p-1}=(p)\{p-1\}'+(n-p+1)\{n-p\}'$$

or to

$$_{q-1}=(q)\{q-1\}'+(q+2)\{n-q\}'$$

and $K_q=2(q+1)\{q\}'$

Then, making the assumption that the calibration-corrections at 32° and 212° are zero, the formulæ for the determination of the corrections to the thermometer-readings are as follows:

(6). $(s, k = \frac{S_0 - S_k}{n+1}$, the values of k running from 1 to p or q.

(7). $L_0 = D_0$

$$L_1 = L_0 + \frac{2}{n} K_0$$

$$L_2 = L_1 + \frac{2}{\qquad 1)} \left\{ K_1 + D_1 - L_1 \right\},$$

or, in general,

$$L_k = L_{k-1} + \frac{2}{k(n-k+1)} \left\{ K_{k-1} + \Sigma_1^{k-1} D - \Sigma_1^{k-1} L \right\}$$

in which the values of k run from 2 to p or to $q+1$, Σ^{k-1} D = the sum of D_1, D_2, &c., to D_{k-1}, and Σ^{k-1} L = the sum of L_1, L_2, &c., to L_{k-1}.

(8). $$(d, k) = \frac{L_k - D_k}{n+1}.$$

(9). $$(k) = \tfrac{1}{2} \{ (s, k) + (d, k) \}$$
$$(n-k) = \tfrac{1}{2} \{ (s, k) - (d, k) \}$$

The probable errors of the corrections are found as follows: The sum of the squares of the residuals =

(10). $$\Omega = (ll) - \frac{1}{n+1} \Sigma \{ k \}^2 - \frac{1}{n+1} \Sigma \triangle^2{}_k + \frac{1}{2(n+1)} \Sigma L^2{}_k - \frac{1}{4(n+1)} \Sigma (k+1)(n-k) \triangle_k'^2$$

in which (ll) is the sum of the squares of the absolute terms in the observation-equations; the limits of k in the second term are 0 and n; $\triangle_k = \{ k \}' - \{ n-k-1 \}'$, k having the limits 0 and $p-1$ or $q-1$; the limits of k in the fourth term are 0 and $p-1$ or q; and $\triangle'_k = L_k - L_{k+1}$, except that $\triangle'_{p-1} = L_{p-1}$, or $\triangle'_q = L_q$, the limits being 0 and

$$p(p+1) \triangle'^2_{p-1}, \text{ or } 2(q+1)^2 \triangle'^2_q.$$

The weights of the respective corrections increase from the ends towards the middle, corrections equi-distant from the middle having equal weights. The expressions for the weights are generally quite complicated, the values lying between

$$P(1) = P(n-1) = \frac{(n+1)n}{2n-1},$$

and

$$P(p) = \frac{2(n+1)}{3},$$

where n is even. The value of $P(q)$ is quite complicated and of different form for different values of n, and slightly less than $\frac{2}{3}(n+1)$. The probable error of a correction is then:

(11). $$r = 0.6745 \sqrt{\frac{\Omega}{P(l-n)}},$$

in which l is the number of observation-equations, being the sum of the series of natural numbers from 2 to n, and u is the number of unknowns, being the number of corrections determined plus the number of m's (or column-lengths used), or $2(n-1)$.

Formulæ for the values of m can also be readily deduced if desired, but in this work they are not necessary.

Example.—The tables for the application of the method to the whole work on the thermometer Green 5280 being rather large, the following short example is selected from the work, and only the results of the full work are given. In the shorter example, the 20° column being the shortest used, the corrections are obtained for points 20° apart from 32° to 212°. In this case $n=9$ and $q=4$. The quantities in the following numbered tables are obtained by substitution of the proper quantities in the formulæ of the same numbers respectively. The unit in the numbered tables is the hundredth of a degree. The formulæ (1) and (2) are arranged in vertical lines in tables 1 and 2 for convenience in addition. The manner of forming table 2 from table 1 will be readily seen. The readings and resulting column-lengths are as follows:

Table A.

1st reading.		2d reading.		Resulting mean column-length.
°	°	°	°	°
32.01	52.08	31.98	52.04	20.06
52.03	72.18	51.84	72.00	.16
72.00	92.06	72.03	92.08	.06
92.04	112.18	91.90	112.00	.12
112.16	132.20	112.10	132.18	.06
132.12	152.22	131.80	151.87	.08
152.00	172.14	151.85	172.01	.15
172.00	192.04	172.18	192.22	.04
191.96	211.98	191.98	211.99	.02
31.99	72.21	31.68	71.98	40.28*
51.95	92.15	51.82	92.04	.21
71.98	112.18	71.82	112.03	.20
92.00	132.20	91.81	132.00	.20
112 07	152.23	111.83	152.02	.18
131.93	172.16	131.78	172.02	.24
151.92	192.16	151.88	192.16	.26
171.95	212.04	171.98	212.08	.09
32.00	91.98	32.00	91.99	59.98
52.08	112.12	51.94	111.97	60.04
72.05	132.00	72.04	131.99	59.95
91 94	151 86	92.19	152.18	.96
111.98	171.88	112.04	171.99	.93
131.99	191.96	132.14	192 10	.96
152.07	212.00	152.00	211.94	.94
32.03	112.25	31.76	112 00	80.23
51.94	132.12	51.83	132.08	.21
72.03	152.18	71.90	152.03	.14
91 96	172.17	91.85	172.10	.23
112.00	192.18	111.81	191.98	.18
132.00	212.15	131.83	212.00	.16
31.94	132.17	31.80	132.00	100.22
51.94	152 12	51.83	152.06	.20
72.14	172.34	71.85	172.08	.22
91.96	192.16	91.81	192.01	.21
111.97	212.03	111 97	212 02	.06
31.98	151.80	32.19	152.00	119.82
51.99	171.82	52.18	172.02	.84
71.98	191.76	72.16	191.96	.79
92.00	211.77	92.22	212.00	.78
32.06	171.97	32.11	171.98	139.89
51.98	191.79	52.03	191.83	.80
71.98	211.64	72.30	212.01	.68
32.18	192.22	31.97	192.00	160.04
51.98	211.94	52.08	212.02	159.95

* 3d reading, 32°.03, 72°.34.

Table 1.

(0)′	(1)′	(2)′	(3)′	(4)′	(5)′	(6)′	(7)′	(8)′
+06	+28	−02	+23	+22	−18	−11	+04	00
+16	+21	+04	+21	+20	−16	−20	−05	
+06	+20	−05	+14	+22	−21	−32		
+12	+20	−04	+23	+21	−22			
+06	+18	−07	+18	+06				
+08	+24	−04	+16					
+15	+26	−06						
+04	+09							
+02								
+75	+166	−24	+115	+91	−77	−63	−01	00
1	2	3	4	5	6	7	8	9
+75	+332	−72	+460	+455	+462	−441	−08	00

Table 2.

(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
−06	+06	+28	−02	+23	+22	−18	−11	+04	00
−28	−16	+16	+21	+04	+21	+20	−16	−20	−05
+02	−21	−06	+06	+20	−05	+14	+22	−21	−32
−23	−04	−20	−12	+12	+20	−04	+23	+21	−22
−22	−21	+05	−20	−06	+06	+18	−07	+18	+06
+18	−20	−14	+04	−18	−08	+08	+24	−04	+16
+11	+16	−22	−23	+07	−24	−15	+15	+26	−06
−04	+20	+21	−21	−18	+04	−26	−04	+04	+09
00	+05	+32	+22	−06	−16	+06	−09	−02	+02
−52	−35	+40	−25	+18	+20	+03	+37	+26	−32

Check on computation of $\{ k \}$:

$$\Sigma \{ k \} = 0.$$

Table 3.

$$S_0 = -52 \quad -32 \quad = -84$$
$$S_1 = -35 \quad +26 \quad = -09$$
$$S_2 = +40 \quad +37 \quad = +77$$
$$S_3 = -25 \quad +03 \quad = -22$$
$$S_4 = +18 \quad +20 \quad = +38$$

Check: $\Sigma S \begin{cases} = 0 \text{ if } n \text{ is odd.} \\ = \{ p \} \text{ if } n \text{ is even.} \end{cases}$

Table 4.

	Multiplier.	Product.
$D_0 = -52 + 32 = -20$	n or 9	−180
$D_1 = -35 - 26 = -61$	$n-2$ or 7	−427
$D_2 = +40 - 37 = +03$	$n-4$ or 5	+ 15
$D_3 = -25 - 03 = -28$	$n-6$ or 3	− 84
$D_4 = +18 - 20 = -02$	$n-8$ or 1	− 2

$-\frac{1}{2}$ sum $= +339$

Table 5.

$$K_0 = +75 \quad + 00 \quad = +75$$
$$K_1 = +332 \quad - 08 \quad = +324$$
$$K_2 = -72 \quad -441 \quad = -513$$
$$K_3 = +460 \quad -462 \quad = -02$$
$$K_4 = 2 \times 455 \quad = +910$$

Check on computation of $\{ k \}'$, D, and K: $-\frac{1}{2}$ the sum of the multiplied D's (last column of table 4)

$$\begin{cases} = \Sigma K - \tfrac{1}{2} K q \text{ if } n \text{ is odd.} \\ = \Sigma K \text{ if } n \text{ is even.} \end{cases}$$

TABLE 6.

$$(s,1) = \tfrac{1}{10}(-84+9) = -07.5$$
$$(s,2) = \tfrac{1}{10}(-84-77) = -16.1$$
$$(s,3) = \tfrac{1}{10}(-84+22) = -06.2$$
$$(s,4) = \tfrac{1}{10}(-84-38) = -12.2$$

Check: $\Sigma(s,k)$

$$\begin{cases} =\dfrac{1}{n+1}(q+1)S_0 \text{ if } n \text{ is odd.} \\ =\dfrac{1}{n+1}\Big((p+1)S_0-\{p\}\Big) \text{ if } n \text{ is even.} \end{cases}$$

TABLE 7.

$$L_0=D_0 \quad =-20$$
$$L_1=-20 \quad +\frac{2}{9}(75) \quad =-\ 3.33$$
$$L_2=-\ 3.33+\frac{2}{2\times 8}(+324-61+\ 3.33) =+29.96$$
$$L_3=+29.96+\frac{2}{3\times 7}(-513-58-26.63) =-26.96$$
$$L_4=-26.96+\frac{2}{4\times 6}(-\ 2-86+00.33) =-34.27$$
$$L_5=-34.27+\frac{2}{5\times 5}(+910-88+34.60) =+34.26$$

Check: $\begin{cases} L_q=-L_{q+1} \text{ if } n \text{ is odd.} \\ L_p=0 \text{ if } n \text{ is even.} \end{cases}$

TABLE 8.

$$(d,1)=\tfrac{1}{10}(-\ 3.3+61)=+05.77$$
$$(d,2)=\tfrac{1}{10}(+30.0-03)=+02.70$$
$$(d,3)=\tfrac{1}{10}(-27.0+28)=+00.10$$
$$(d,4)=\tfrac{1}{10}(-34.3+02)=-03.23$$

Check: $\dfrac{1}{n+1}\Big(\overset{p \text{ or } q}{\underset{1}{\Sigma}} L-\overset{p \text{ or } q}{\underset{1}{\Sigma}} D\Big)=\Sigma(d,k).$

TABLE 9.

$$(0)=\ (32) \quad =\ 00$$
$$(1)=\ (52)=\tfrac{1}{2}(-07.5+05.8)=-01$$
$$(2)=\ (72)=\tfrac{1}{2}(-16.1+02.7)=-07$$
$$(3)=\ (92)=\tfrac{1}{2}(-06.2+00.1)=-03$$
$$(4)=(112)=\tfrac{1}{2}(-12.2-03.2)=-08$$
$$(5)=(132)=\tfrac{1}{2}(-12.2+03.2)=-04$$
$$(6)=(152)=\tfrac{1}{2}(-06.2-00.1)=-03$$
$$(7)=(172)=\tfrac{1}{2}(-16.1-02.7)=-09$$
$$(8)=(192)=\tfrac{1}{2}(-07.5-05.8)=-07$$
$$(9)=(212) \quad =\ 00$$

Check: $\Sigma\,(k)\begin{cases} =\Sigma(s,k) \text{ if } n \text{ is odd.} \\ =\Sigma(s,k)-(p) \text{ if } n \text{ is even.} \end{cases}$

TABLE 10.

	Squares.
$\Delta_0=+\ 75+00=+\ 75$	5625
$\Delta_1=+166+01=+167$	27889
$\Delta_2=-\ 24+63=+\ 39$	1521
$\Delta_3=+115+77=+192$	36864
	71899

	Squares.	Multipliers.	Products.
$\Delta'_0=-20+\ 3=-17$	289	1×9	2601
$\Delta'_1=-\ 3-30=-33$	1089	2×8	17424
$\Delta'_2=+30+27=+57$	3249	3×7	68229
$\Delta'_3=-27+34=+\ 7$	49	4×6	1176
$\Delta'_4=-34 \quad =-34$	1156	2×5^2	57800
			147230

$$(ll) \quad =+11872$$
$$-\tfrac{1}{10}\Sigma\{k\}^2 \quad =-\ 996$$
$$-\tfrac{1}{10}\Sigma\Delta^2{}_k \quad =-\ 7190$$
$$+\tfrac{1}{20}\Sigma L^2{}_k \quad =+\ 160$$
$$-\tfrac{1}{40}\Sigma(k+1)(n-k)\Delta'^2{}_k=-\ 3681$$
$$\Omega \quad =+\ 265$$

TABLE 11.

$$P\ (1) = P\ (8) = \frac{10\times 9}{18-1} = 5.29$$

$$P\ (4) = P\ (5) = \frac{1120}{171} = 6.55$$

$$r(1)=r(8)=0.6745\sqrt{\frac{265}{5.29(44-16)}}=\pm 0^\circ.009$$

$$r(4)=r(5)=0.6745\sqrt{\frac{265}{6.55(44-16)}}=\pm 0^\circ.007$$

The calibration-corrections as obtained by Hansen's method for every 5° point from 32° to 212° are given in the first column of table B following. n in this case is 36 and p 18. The calibration-corrections were also obtained by Neumann's method, (see *Am. Jour. Sci.*, Vol. XXI., May, 1881, Art. XLVII., for method,) and the same values were obtained throughout except for the 82°, 87°, 92°, 117°, 122°, 127°, points, when to the nearst hundredth they are 0°.01 larger than those obtained by Hansen's

method. The probable errors of corrections obtained by Hansen's method, carried to thousandths of a degree, range from 0°.0026 at 122° to 0°.0030 at 37° and 207°; and by Thiessen's method of obtaining the probable errors of corrections obtained by Neumann's method, they are 0°.003.

3. Other Corrections. — The boiling point was determined three times within two hours, alternating with determinations of the freezing point. The readings with the thermometer horizontal were in order, July 12, 1882, 6 p. m., 211°.00, 31°.38, 210°.80, 31°.30, 210°.78, 31°.30, 7:40 p. m. The readings at boiling point with thermometer vertical were, in the same order, 210°.60, 210°.50, 210°.58. The barometer-reading at 6:36 p. m. was $29^{in.}.195$, with corrections of −0.126 for temperature, and +0.017 for instrumental error, to be applied. The thermometer was 46.80 feet below the barometer, and was approximately 600 feet above sea-level Taking the mean of the boiling-points observed (horizontal), 210°.86, and the boiling-point for observed barometer, with the proper corrections applied, 210°.67, the correction −0°.19 to thermometer-readings in vicinity of 212° results. The correction to thermometer-readings near 32° is, from the last reading above, +0°.70. The length of column between freezing and boiling points is then 180°.89, and the "scale-corrections," or corrections to thermometer-readings on account of this excess of length over the graduated length, result as given in table B, second column.

The constant corrections to the thermometer-readings, given in the third column are the sums of the corrections in columns 1 and 2. Application of the freezing-point correction at any date to these corrections will give the total corrections to the thermometer-readings on that date.

The thermometer was compared on January 1, 1882, with thermometer Casella 21472, (now in the office of the Chief Signal Officer, U. S. A.), whose corrections to an air-thermometer have been well determined, and the corrections to Green 5280 to reduce to an air thermometer (with the freezing-point correction for that date subtracted), resulted as given in the fourth column.

Table B.—Corrections to Thermometer Green 5280.

No. of graduation.	Calibration corrections.	Scale corrections.	Total *constant* correction.	Constant correction to reduce to air-thermometer from Casella 21472.
32°	0°.00	0°.00	0°.00	
37	— .02	— .02	— .04	
42	— .04	— .05	— .09	— 0°.17
47	— .02	— .07	— .09	
52	— .01	— .10	— .11	— 0°.21
57	— .07	— .12	— .19	
62	— .04	— .15	— .19	— 0°.36
67	— .05	— .17	— .22	
72	— .07	— .20	— .27	— 0°.39
77	— .05	— .22	— .27	
82	— .05	— .25	— .30	— 0°.40
87	— .05	— .27	— .32	
92	— .03	— .30	— .33	— 0°.45
97	— .00	— .32	— .32	
102	— .02	— .35	— .37	
107	— .04	— .37	— .41	
112	— .08	— .40	— .48	
117	— .06	— .42	— .48	
122	— .04	— .44	— .48	
127	— .04	— .47	— .51	
132	— .04	— .50	— .54	
137	— .04	— .52	— .56	
142	— .04	— .54	— .58	
147	— .01	— .57	— .58	
152	— .03	— .59	— .62	
157	— .09	— .62	— .71	
162	— .07	— .64	— .71	
167	— .06	— .67	— .73	
172	— .09	— .69	— .78	
177	— .08	— .72	— .80	
182	— .05	— .74	— .79	
187	— .06	— .77	— .83	
192	— .07	— .79	— .86	
197	— .03	— .82	— .85	
202	— .04	— .84	— .88	
207	— .02	— .87	— .89	
212	— .00	— .89	— .89	

Natural oil, though possessing several advantages as a lubricant over vegetable or mineral oils, has the disadvantage of wanting the necessary viscosity and solidity to resist great pressures. M. Boulfroy has remedied this defect by concentrating natural oil in a special apparatus until it acquires a density of from 0.91 to 0.912. On continuing the process of concentration, a mineral tallow is obtained, which takes the place of animal tallow.

CANAL PROJECTS OF THE DAY.

By F. N. NEWCOME.

From the "Nautical Magazine."

Two engineering projects of surpassing interest have riveted the world's attention during the past few sessions. Through the pre-eminent attention paid to these, and the varied feelings they have called into action both at home and abroad, other propositions of almost equal audacity and merit have been purely eclipsed. Projects that a generation ago would have literally astounded the world, have scarcely evoked a moment's controversy, barely even a partisan championship; while volumes, rather let us say libraries full, have been written about the Panama Canal and the Channel Tunnel. Yet many of the schemes so ignominiously dismissed in a few short sentences are more realizable, more useful, and more certain to bear fruit. When completed, as many of these less imposing enterprises will be in time—and this is more than can be confidently predicted of the Channel Tunnel—they will exercise an influence over politics and commerce, not much inferior to that exerted by the Suez waterway. Unquestionably, however, the Panama venture must be allowed to take first rank among all marine enterprises of the day. It stands *facile princeps*, and must remain so, perhaps, till the end of time. Besides, its consummation appears to be within measurable distance. The capital required to construct it is fully subscribed; serious operations have been commenced; an army of excavators are toiling day and night, while M. de Lesseps, whose name as regards canals is one to conjure with, has distinctly promised its fulfilment by the year "eighteen-eighty-eight." Should his prophecy come true then, or even should his splendid undertaking be achieved at some later date, the mercantile world will undergo a revolution, besides which, all previous transitions will fade and grow dim. England, at any rate, will lose her natural Oriental supremacy—the favored merchants of the Eastern American States competing with other on more than even terms in the rich markets of Japan, China, and the Indies. This unequal emulation will doubtless lead to another serious diminution in the profits upon our trade with Oriental nations, if not to a positive falling away in its volume, while, as regards America, we must expect to see her Mercantile Marine assuming an importance to which it has been a stranger since the iron age dawned across the shipbuilding horizon. What that mutation destroyed, the Panama Canal may re-create. Given a seaway between the Atlantic and Pacific Oceans, almost within her own territories, and the youthful Titan is certain to set her feet astride the ocean. Destiny is evidently "shaping the ends" of the giantess Republic, decreeing for her offspring a first place among maritime people.

But that ocean passage is not likely to be an unmixed blessing to the United States. Its imminence is already raising jealousy and disquietude among the nations; it is likewise drawing attention to the superior advantages possessed by Georgia, Florida, and other States on the Mexican Gulf, with regard to position, harbors, resources, and distance from the Eastern markets. If the South should become a great center of the ship-building industries, and manufacturing capital be transferred thereto from the over-crowded North-East, it may possibly involve a revival of the ancient feud between the North and South. The West is already separated from New York and Washington in all save nominal allegiance. Between the protected manufacturer and the Western farmer there is little in common; and now that free-trade lessons have educated the latter into regarding his rival as a creature who fattens by the sweat of another's brow, the last strong link of sympathy is snapped. There is magic in a name, and it is simply the magic of the word "greatness" which now keeps the East and West in cohesion. It was not yesterday that the latter began paying attention to its own ports along the Pacific, and to its own chances of manufacturing

successfully, and with each diversion of traffic from the San Francisco to the Colon-Panama railway route the agitation recommences. When the seaway is open commerce will be entirely turned aside from the trans-continental route. It will travel by ocean direct from New York, Boston, Brunswick, or New Orleans to its destination, Californians deriving no particle of advantage from its transport. What may we expect them to do in self-defence? To sit still and be half ruined, or to buckle to and manufacture and export for themselves? Is it not reason to suppose that they will try to turn their coal, iron, and copper to account, dispensing with New York as far as may be? New Orleans, again, instead of sending the raw product North as now, is certain to reel and weave its own cotton when once the China ports are brought within a few days steam. Economic laws may be over-ridden for a time, circumstances, indeed, may justify such a course, but when the producing country suddenly finds itself placed in juxtaposition with its customers, it is not likely to allow far-off States to divide its profits. As things now stand it may not pay New Orleans to manufacture and ship its own cotton, but with the Panama Canal open it will, and should the change we anticipate take place, Eastern interests will seriously suffer.

Further, a considerable portion of the European grain supply comes from ports on the Pacific or Gulf of Mexico; Californian growers finding it pay them better to ship around Cape Horn to London than to send across continent to the Atlantic ports. When, however, vessels can steam through Panama, from San Francisco or Diego to Europe, the greater portion of the Western grain trade (not only from districts contiguous to the coast, but from places far inland) will be diverted, ceasing to pass over the Northern railway lines.

However, there is another aspect to this interesting problem. As remarked in a former article on "Shipping Prospects in China," there is a possibility that the trade of this country may benefit from M. de Lesseps' enterprise, at least for a time, perhaps even in the long run. Divided by such enormous distances from the Oriental markets as the Eastern States now are, a transfer of capital and energy to the Pacific must have resulted sooner or later, and it may be that by connecting the Atlantic and Pacific this transition may be retarded rather than hastened. Instead of fixed capital transferring itself from the East to the West, it is probable that the manufacturers of New York and Pennsylvania, aided by the shortened passage and other facilities afforded by the canal, will endeavor to hold their own against the competition arising on the Pacific and Mexican Gulf.

Another canal, uniting the Caribbean Sea with the Pacific Ocean, is projected. Having been taken under the patronage of the American people, and being strongly supported by the Washington Senate, it is quite likely to be open for traffic before the first vessel steams merrily through the great French undertaking. Although not a sod has yet been turned, and although the gross distance from inlet to exit will be considerably longer—about 180 miles against less than a quarter that distance dividing Limon Bay and Paraiso, the point where M. de Lesseps' canal enters the Pacific—the physical difficulties are by United States engineers esteemed inferior to those presented by the rival scheme. It may therefore prove, after all, the pioneer enterprise. It follows the natural line mapped out by the river St. Juan and Lake Nicaragua. Should it at any time be finished, it will doubtless become the more fashionable route, lying some parallels further from the equator. The one great disadvantage it offers is the high summit level—1,076 feet, which must be attained by lockage. On the other hand, the Panama project dispenses entirely with all such contrivances.

Ead's Tehuantepec ship-railway is also meeting with keen support, and like its rivals, will probably become *un fait accompli*, before this century closes. As a first result of its anticipated success, a vigorous effort is being made to excavate a passage across the Florida peninsula. Although the two schemes have no distinct reference to each other, the Tehuantepec route will vastly benefit by the construction of the Florida channel. The other projects to which we had been alluding are beside its influence; traffic from New York to Port St. Juan or

Colon would gain nothing in distance by following the proposed canal route, and might lose considerably in time and money, whereas steamers bound for Campeachy Bay, or other places on the Mexican Gulf, would find it economical to avoid the long sea voyage around Cape Florida. But the towns which would most benefit from the Florida enterprise are Galveston, and those at the mouth of the Mississippi. These will be brought several days nearer to the Northern States, while the passage from New Orleans to Europe will be shortened some hundreds of miles. Sir E. J. Reed proposed an alternative scheme, namely, to construct a ship railway across Florida, and it is quite likely that in the course of time his proposition may be revived, especially should Captain Ead's railway prove as successful as hoped. To commerce between New York and the East, the advantages of the Florida-Tehuantepec route are manifest as compared with the Panama line; the saving in distance to Honolulu and the Japanese and Chinese ports will be enormous — at the very least a thousand miles; while, as regards local trade between ports on the Gulf of Mexico and those along the Northern Pacific, all attempts at comparison are out of the question. A glance at a large map of the world will reveal the immeasurable superiority of his route; indeed, supposing it to be successfully completed, it is difficult to perceive what commerce could select the Panama and Nicaragua routes. European trade with the Orient is sure to prefer the Suez Canal, so will a portion of that issuing from the North Atlantic ports of America, while such percentage of the same commerce as selects the inter-oceanic route will certainly let its choice rest upon Tehuantepec. As a fact, then, supposing Mr. Ead's project should be accomplished, the Panama waterway could only command the shipping passing from North Atlantic ports in Europe and the United States to ports on the West coast of South America, and even this, its legitimate right might be seriously compromised by the questions of climate and expense. When near the equator, ten degrees is a consideration, besides which Ead's railway is contemplated to cost little more than one-third of the estimated expenditure on the Panama Canal. Admitting there is a large under-computation somewhere, the balance still seems to incline upon one side, and should experience demonstrate the feasibility of these ship-railways, shareholders in rival concerns will have little reason to plume themselves upon their good fortune. On the other hand, a canal once dug costs comparatively little to keep in order or work, whereas haulage, power and maintenance of the proposed railway will prove a terrible tax upon the annual incoming. Actual questions, however, over an extended period will alone solve these moot problems.

Another scheme demanding notice is the proposed junction between the Bay of Biscay and the Gulf of Lyons. Such a communication may be said to exist at the present day. The waters of the Garonne, passing Bordeaux, flow through Marmande and Agen to Toulouse, where they unite with those of the Canal du Midi, or, as it was formerly called, the Languedoc Canal. According to an eminent French engineer, M. Abt, the new waterway is to begin at Bordeaux and end at Narbonne, running parallel with the Garonne as far as Castels, at which point it will join the Canal Latéral as far as Mas d'Ageny. From thence it will diverge from the present canal and pass through Agen, Gers, and St. Genès to Montech, where it will again unite with the Canal Latéral. Subsequently at Toulouse, it will connect itself with the Canal du Midi, the united waters flowing together into the Valley of Bastide d'Anjou, after which it will pursue an independent course to the Mediterranean, passing Carcassonne, Lézignan, and Béziers. Practically, then, the French Chauvinists purpose utilizing in part an existing channel, deepening the streams sufficiently to permit of the largest men-of-war passing through. It is very doubtful whether this undertaking could be made to pay commercially. The distance to be travelled is immense, while the various gradients met with along its route will necessitate a multitude of locks. Besides, its original cost is estimated by the State engineers at 1,500,000,000 francs, or £60,000,000 sterling. Supposing even it could be constructed for half that sum, which is the lowest estimate that has been formed, a dividend of only four per cent. would require £1,200,-

000 per annum. How is it to be earned? The task seems hopeless. If an excessive tariff is charged, merchant vessels will prefer to pass through the Straits of Gibraltar, or if patronage is courted by moderation, dividend earning will be out of the question. As a purely business speculation, then, this project seems doomed to failure; nevertheless, it would pay the French nation well to construct. Politically, it would render them independent of that *bête noir* "Gib," at the same time developing the commerce of the whole empire. Before long a vast population will line its banks; large towns will arise in every convenient locality; a hundred industries, now undreamt of, will spring into existence, while the country for leagues on each side will be opened up. Havre, Brest, and the other English Channel and Biscayan ports—indeed all Northern France will benefit as well. Commerce with the Mediterranean and the East will be revivified and developed, besides which many new home markets will be opened up. Should the canal lead to the resurgence of French cotton manufacturing industries, as seems inevitable, and to nothing else, it would be cheap to France at treble any sum it is likely to cost. What a splendid gift from the French nation to itself this seaway would be! A small present sacrifice, a mere passing of wealth from the hands of the thousands into those of the hundreds, to be diffused again throughout France in a few months, perhaps days, and the Gallic race would in one moment attain a degree of influence, importance and prosperity which fifty centuries could not endow it with while its northern and southern ports are so widely separated. Before this splendid ambition our own projects for making Manchester a seaport and connecting the waters of the Bristol and English Channel sink into insignificance. From a commercial aspect both these enterprises deserve the name of "great," but they have little or no political bearing; scarcely more so than the more modest proposition for severing the narrow neck of land that now divides the sister Lochs Tarbet. Granting that ocean liners will steam uninterruptedly up the inky Irwell to Manchester, the question is purely one for Lancashire to consider, and even supposing that good-sized ships traverse the isthmus which divides Bridgewater Bay from the estuary of the Exe, the consequences and the gains will be ours alone to count. Far different with the French aspiration, which, if ever consummated, must revolutionize the political and mercantile *status* of Europe, and render France, in point of strategical position, the most powerful factor in Western politics. Gibralter, as a military station, would lose half its significance. One European nation—the greatest—will be rendered independent of the "Pillars of Hercules," holding in its own right the chief ocean-way between the Atlantic and *Mare Internum.*

England, with uncalculable interests to defend in the East, may well pray for the non-achievement of this speculative ambition, for it would necessitate her doubling her Mediterranean fleet, to avoid a sudden concentration of the Cherbourg, Brest and Toulon ships. It has been reckoned that the men-of-war harbored at Brest could reach Port Said, *viâ* the Garonne Canal, some four days earlier than the English Channel cruisers, who would have to round the "southern rock." For our influence, then, it may be hoped that the labors of construction may prove too severe for the French engineers. Still, our greatness should render us free from petty jealousy at the rising splendor of another power. Where, however, we should feel the shoe pinch most, would be in our trade with the East. At present some portion of the silks and other Oriental wares consumed in the north of France, are shipped direct to England, and thence re-shipped to ports on the French coast; with the ship-canal in existence this commerce would cease. We should also lose our profits on the £5,000.000 worth of French products now sent to our shores for transhipment, or, at least, upon a large proportion thereof. In many other ways we should feel its adverse effects. Our shipping industries would receive a violent shock, while those of France would spring into fresh activity; our flag would be less frequently seen in eastern waters, while theirs would cease to be a stranger, and if the French people desired to avenge Waterloo, they could do so by freeing the canal to native-owned ships, and charging high dues to those bearing a foreign emblem.

Having entered upon a new course of enterprise at home and abroad, which even the death of Gambetta will not terminate, it is improbable that French Chauvinism will long be baulked in this favorite ambition; besides, canal digging has become an actual mania with our excitable neighbors. Millions upon millions of francs are being spent by the Government on public works. The Seine is being deepened between Paris and Rouen, and a magnificent project for making Paris a seaport is slowly fructifying. M. Boquet de la Gaye, hydrographer to the navy, proposes to dredge the river deep enough to allow vessels drawing twenty-three feet of water to ascend to the capital. Although the difficulties are stupendous, and the cost will be enormous, and although engineers and capitalists look grave, the gay city has not lost heart, and at some time in history will essay to place herself on an equality with London. Great attempts are also being made to improve the Rhone navigation between Lyons and the sea, and before many decades are past, China steamers may be seen alongside the wharfs of the provincial capital.

In foreign parts the French canalizing spirit is equally alert. M. Deloncle and M. de Lesseps have between them decreed the separation of the Malay peninsula. The isthmus of Kraw is to be cut through at its narrowest point, shortening the voyage for steamers some three days or more. This will be another blow to England's commercial supremacy. If Euro-Oriental traffic is diverted from the Straits of Malacca, the star of Singapore will set. Both this town and Malacca owe their fame and sudden rise to the trade between the Orient and Occident passing through these straits. Singapore affords a convenient place of call, forms an admirable coaling station; but given a change in the route taken by the China ships, and this resplendent settlement will lose its *raison d'être*; its vast population, existing mostly by their commerce with passing vessels, will migrate in shoals to places where the battle of life may be fought on fairer terms. Trade between Singapore and vicinal ports could not maintain one quarter the present inhabitants, and in a lesser degree the same argument applies to Malacca. Saigon—the French settlement—it is supposed, will benefit and grow rich by the ruin of its British rival. The Kraw Canal project and French activity in the Indo-Chinese peninsula are engendered by the same restive spirit, the same craving after colonial empire, the same yearning after glory. Nothing will subdue or bridle this unfortunate longing for renown except repeated disappointments and the constant eating of Dead Sea fruit.

On the face, it does seem probable that Saigon would arise on the ashes of Singapore, but the vagaries of trade are so unaccountable that it would not be surprising to see the French colony neglected, and new stations springing up at the entrances to the sea-way. This would only be re-acting the experience of Suez, Ismailia and Port Said. French designs might also be defeated through the construction of a second canal or ship railway by English capital, in which case M. Deloncle's calculations as to profits and receipts might require scaling some eighty per cent. Several sections of the peninsula are devisable, and if one canal is made to pay it will certainly provoke emulation before long. These are considerations which investors in these high-flown speculations will do well to take to heart; nevertheless, as political and commercial problems, they demand the earnest attention of our Government.

In Northern Africa, Commandant Rouvaire's proposition for flooding the Tunis-Algerian chotts and thereby re-establishing the Lake Tritones known to the ancients, is coming within measurable distance of experiment. If the "chotts" between Gabes and Biskra are flooded, as M. de Lesseps says they will be ere long, an inland sea, in size nearly fifteen times that of the Lake of Geneva, will have been created by the work of man's hands. The prospect is indeed dazzling. Moreover to this project there seems no conceivable objection. It invades the rights of no one; apparently can do nothing but good. Neither we nor any other nation have a right to object to France erecting for herself a colonial empire in these uncivilized regions. By occupying and reclaiming them she will only work out the evolutionary edicts of progressive nature. Besides, the existence of a vast watershed in the desert will tend to its fertilization, if not to its

early repopulation, at the same time defining accurately the territorial limits of several nationalities. Cutting the necessary waterway will prove no light or costless task. When, some time ago the scheme was examined by a Government commission, it was shelved, the estimated expenditure fairly frightening official Paris. Then, when the gallant projector was plunged in despair, a good fairy appeared in the shape of the "universal separationist," who had but to touch with his wand the rock of French speculation, and the sixty million francs required were forthcoming. At least M. de Lesseps says that sum is provided. If all goes well, then, and the new report to be drawn up this spring should prove favorable, the current year may find operations commenced. Mr. Rouvaire considers that the lake, when made, will prove immensely valuable for commercial and military purposes. It will provide a new trade route in the direction of the African Sahara, also securing a strategic line for the defence of French conquests in Algeria and Tunis.

The Isthmus of Corinth is likewise doomed. General Türr and Count de Lesseps are completing their preparations for disconnecting the Morea. The short four miles of limestone rock, which now divide the "chafing waters" of Ægina and Lepanto, is likely soon to be cut in twain. As, happily, the intervening land between the two gulfs is nowhere more than one hundred feet high, this Periander-aged problem looks likely to be solved by the Nineteenth Century. What Nero subsequently failed in, the present may now be accomplished. That Emperor attempted the task, driving a 200 feet canal about half-a-mile inland from the western shore, near Kinchrea. His work, however, was stopped by the outbreak of rebellion in Gaul, which necessitated prompt and vigorous action, and ever since the scheme has rested, so far as active operations are concerned. Traces of this ancient channel may still be seen, but Time, the destroyer, has gradually filled in its bed, and the present depth is inconsiderable. As regards political influence, this waterway can exercise little or none. If it possesses any interest watever outside of Turkey and Greece, that interest will be confined to commerce. Even in this respect its value will be mostly felt by coasting traders. To vessels passing between ports on the Adriatic and the Black Sea, or beyond, it will doubtless prove of some utility, shortening by a couple of days or so, an inconvenient, if not dangerous passage. Athens, Salonica, and Smyrna will also benefit by a quickened route to the Italian ports; still it remains a moot question whether the canal will attract any sufficient percentage of the Euxine trade with places west of Marseilles, without which element it can scarcely prove remunerative. Natural seaways have one preponderating advantage over artificial ones. Nature demands no tolls, other than an occasional life, or shipload of cargo; whereas public companies require payment, frequently at exorbitant rates. Besides, there is a universal inclination to gamble, even when the dice played with are human lives. Many a foreign shipowner will prefer a longer and perhaps a more dangerous voyage to paying M. de Lesseps's company the heavy tolls it will certainly demand. Only a trifling gain will accrue to vessels bound to or from Gibraltar, or which touch at Malta, Algiers, or any other North African port. All trade between the Bosphorus and ports on the west coast of Italy must pass Messina, and to this again the saving will be but slight. Even the Messageries steamers which ply from Marseilles to Athens and Salonica, and whose route lies through the straits of Bonifacio and Messina, will scarcely find in the slight economy of time and distance, an adequate recompense for the extra charges which must be imposed.

Another scheme, quite as useful, if not more so, is to disunite the Crimea from the Russian mainland. Between Odessa and the grain producing districts of Azof and the Don there is a constant flow of commerce, and to merchants engaged therein the proposed northern passage will prove unmistakably valuable; while both Varna and the Danube ports will be brought into quicker communication with those fertile regions. On the other hand, it is very improbable that Kertch Strait will loose its present hold upon commerce with Constantinople and beyond; but it may happen, however, that in winter time, when Kertch is frozen in, a channel may be kept open through the canal, in which case the gain will be simply Euro-

pean; the question of a cheaper grain supply being evidently involved.

Probably, however, the Russian Government, when giving their sanction and authority to this project, were as much actuated by strategic as by commercial exigencies. With a second and thoroughly defensible exit, the Sea of Azof would become many times as valuable, if converted into a naval harbor. In its bewildering and unassailable recesses, a fleet of any given power might be collected—practically with impunity. Given money—which "bankrupt Russia," as they call her, can always find for military purposes — any number of leviathans might be built and launched almost unknown to us; certainly without our being able to interfere. Torpedoes and land fortifications could easily render the Perekof Canal and Kertch Strait impregnable; yes, even to attack from our latest types of steelclads. To blockade the hidden Russian vessels would require a couple of fleets instead of one; besides which both would have to be as powerful as that of the enemy, otherwise the blockading admiral would run a serious risk of being taken in flank by a navy circumnavigating the peninsula. Constructing the canal will also serve as a sort of guarantee against a renewed bombardment of Odessa. Any foreign fleet that essays the task thereafter will hazard destruction by a flank movement from Perekof, the vessels stealing out under the shelter of the Tendra peninsula. That this strategic highway will be cut, and cut shortly, scarcely admits of a doubt. To secure her empire, Russia will not stick at any trifling expenditure, and as a fact this sea-canal would make her southern dominions fairly secure against foreign attack; at the same time enabling her to re-establish her maritime position on the Black Sea, in defiance of the Treaties of Paris and London.

On the Lower Danube, Russia is equally active. She wishes to open the Kilia branch of that river to vessels of the largest tonnage by means of a deep sea-water canal, falling directly into the Euxine. Without entering into the diplomatic dispute whether Russia possesses any legitimate claim to the right bank of this outlet, the Danube navigation is a question demanding earnest attention on the part of Europe. Is it right for Russia (a power whose commercial policy is retrogressive, if not actively malignant) to be entrusted with the keys of this international highway? The Kilia mouth discharges a larger volume than either the St. George's or Sulina, its navigation is perhaps less difficult, and as a consequence it is more frequented by merchantmen. If the most important highway now, what will it be when the Russian seaway is cut? Will not its utility be enormously enhanced? Besides, there is always the contingency of the Danube Commission closing its labors, in which event the Sulina branch will soon become unnavigable and deserted. Trade will then be thrown entirely upon the Russian Canal—for the general good, or not? Admitting that a deepened outlet may, probably will promote the interests of Galatz, Ibraila, and other riparian towns, there is something intensely disquieting in the prospect of the dreaded Northern Power holding a firm grip upon this river. Heavy, perhaps prohibitive tolls are sure to be levied; fresh, or differential import duties may be imposed; additional taxes are likely to be laid upon exports, whilst even the new cutting may be subserviated to military expediencies and aggressive operations. This contingency, by the way, has been more than darkly hinted at by the *Golos*, *Viêdomosti*, *Petersburskiaïa Gazeta*, and other semi-official journals. The Vienna correspondent of the *Daily Telegraph*, after expressing his belief that the Kilia Canal will make Europe the tributary of Russia upon the Lower Danube, and that this "will bring ruin to the riparian States, great and small," very properly remarks:

"It will sooner or later be discovered what an irreparable blunder it was to allow Russia to return to the Danube. In any case, England, whose flag covers two-thirds of the total navigation of the lower part of the river, cannot afford to be Russia's dupe in this matter, and will certainly not be led astray by any misstatement of facts connected therewith."

One other Russian enterprise deserves a word *en passant*. During the summer of last year (1882), the Government military canal at Cronstadt was opened for traffic, vessels with a draught of 14 feet successfully entering Lake Ladoga at low water.

Another bold separationist wishes to

marry the Gulf of Saros with the Sea of Marmora, thereby making Europe independent of the Dardanelles. However, as a small mountain has to be cut through, this proposition may be placed by the side of schemes for bridging the channel or tunneling between Kintyre and Fairhead. Salonica, again, is unlikely to have an exit to the Gulf of Contessa, for some generations at least; but on the other hand, there is every prospect of Schleswig-Holstein being shortly divided. The distance to be excavated must be reckoned inconsiderable in these days of colossal undertakings, nor would the cost be extravagant, compared with the enormous benefits which Germany must obtain. The Skager Rack is frequently closed by ice during the long winters. At such times St. Petersburg, Riga, Stockholm, Dantzig, Kiel, Copenhagen, and other Baltic ports are shut out from all maritime communion with other ports. The proposed canal would greatly obviate this inconvenience, while, as regards the naval marine of Germany, it could not fail to exercise a re-invigorating effect. At present a navy is of little use to the Central Power; it can always be blockaded at the entrance to the Sound. But given a seaway out to the North Sea under cover of German guns, and the striking power of the Baltic fleet would be intensified tenfold. Moreover, the completion of this military and strategic outlet, would do more than anything else to lessen the German land-hunger in the direction of Denmark. To a puissant and aspiring ruler like the octogenarian Emperor of Germany, it must be extremely galling to find his superb creation shut in by the cannon of that small kingdom, which only preserves its independence through the jealousy of Europe. The existence of a trans-Schleswig ship-canal may be expected to allay in part the bitterness of this feeling, and its construction is purely a matter of time and opportunity. For the moment the enterprise is shelved, the Berlin Treasury having to find the "wherewithal" to execute another undertaking of more immediate necessity. From Bremerhaven to Bremen the Weser is to be made navigable for ocean-going vessels, and it will be a keen race between Manchester and the German town as to which shall first welcome a trans-Atlantic line. The event will be watched with some interest.

Three millions sterling will, it is estimated, be spent upon the Weser improvements, and the Government engineers promise conclusion in or about the year 1889. This intention opens up for Bremen the prospect of taking first place among the shipping centers of Germany; possibly, indeed, it may rival Berlin in point of population and splendor, as time goes on. Anyone who carefully studies the relative positions of Bremen and Hamburg, in connection with the European railway system, can hardly form but one conclusion, namely, that the former port, when open to ocean liners, will appropriate the chief trade with New York. This cannot but affect the future of Hamburg, and perchance, may seriously retard its progress. Germany, like France, is also paying increasing attention to its inland communications, several enterprises of great moment being now in progress in connection with the Elbe and Moselle. However, their importance is chiefly local and they can be dismissed without further notice here.

In Canada, the Welland Canal, connecting Lakes Erie and Ontario—of itself a splendid achievement—has been open to traffic for some time, and steamers of very considerable tonnage now pass from one lake to the other. However, this canal is but one section of a gigantic waterway which is intended to place the great Northwest in direct communication with Europe. Great efforts are now being made to deepen and reconstruct the various canals that now lead between Kingston and Montreal, and when a sufficient depth has been attained—uniform with that in the Welland channel—it will become possible for grain vessels to load in Manitoba and unship in Liverpool or London. Although this project attracts little attention here, it cannot fail to prove of immense consequences to the well-being of our colony and ourselves. Not the least advantage will consist in our consuming more Canadian and less American corn, and in the cheapening of our food supply, which must inevitably follow from this increased competition. In the second place, the Canadian route may appropriate to itself some proportion

of the rich trade centering in "Porkonopolis." According to a calculation that has been made, the distance between Chicago and Montreal by the Welland Canal system is 150 miles less than from Chicago to New York, by way of Buffalo and the Erie Canal. This fact, if true, should not be lost sight of, and alone might justify the enormous expense which Canada is now incurring in connection with this scheme.

In India and British Burma, millions are being spent on irrigation works and in improving the internal navigation of the Punjaub and North-western States. One colossal enterprise was opened in state last year, and another of almost equal value and proportions is drawing towards completion. However, the interest in these undertakings is chiefly local; few people in this country care a fig about the Sirhind, Bari Dôab, Soane, Orissa or Ganges Canals, and if the Indian language is to become "household" in this relation, it will be through a very different project. Probably few persons will call to mind at once the Island of Ramaiswaran or Ramisaram; it certainly has not earned for itself, so far, a gold letter page in the book of history, nevertheless, this *terra incognita* may yet win for itself a measure of fame not far inferior to that which has fallen to the lot of Nicaragua and Tehuantepec. Between India and Ceylon lie two islands, Ramisaram and Manaar, connected together by a sandbank commonly known as Adam's Bridge. Palks Strait divides the former from the Indian mainland, but the passage is dangerous and navigable only for small vessels; while the strait between Manaar and Ceylon is even less accommodating. It is now proposed to cut through Ramisaram and thus shorten the voyage to Madras, Calcutta, and the further East. This useful, non-ambitious, and realizable plan is under consideration by the Government of India, and it may, indeed be hoped that its execution may be sanctioned without delay.

Even Japan is afflicted with the canalizing mania, a bold project being on foot to dig a passage between Hamada and Hami, in the Island of Nipon; thus cutting off the dangerous point of Salo-no-Misaki or Cape Chickakoff, so dreaded by all mariners in those waters. It is proposed to adopt the Dutch system, and if commenced at all, the work will occupy at least three years, the distance to be cut being about ten miles. Japan certainly offers a wonderful opportunity to the marine engineer. Its population is as large as that of Great Britain and almost as dense; the people are wealthy, industrious, honest and intelligent, the revenue is not to be despised; coal and iron are plentiful, while the various sections of the Empire are separated by narrow straits. Here, indeed, is a splendid field for Sir Edward Watkin and his associates. By tunneling the narrow seas dividing Yesso, Nipon, Sikok and Kiusiu, no national interest can be endangered; the Japanese can only benefit. Besides this, the speculation would probably pay investers infinitely better than gold mining in India, as the want of some such connection is a standing grievance with the thirty-four million inhabitants of Japan. In shaping these islands nature has, perhaps, unintentionally adapted them to the requirements of the "separationist" as well as to the submarine "connector." The main islands want joining together, while all round the coast there are obstructive necks of land which might be beneficially dispensed with. New Zealand also affords a good field for marine engineering. The chief island is extremely long and narrow in places, presenting every inducement for successful partition. Two strips of land have been critically examined with a view to eventual operations, and before long a company may be started to give the chief city an eastern outlet.

Returning to the New World, we find Nova Scotia practically doomed, if not in the immediate present, at least in the near future. A similar fate seems in store for the odd-shaped Peninsula that now interposes itself between Cape Cod and Buzzard Bay, and which interferes with navigation between Boston and New York; while even the younger Continent is not without its schemes, unambitious ones though, it may be said. The natural configuration of Australia precludes our young colonies from fascinating their imaginations with any stupendous problems. Providence has proved too truly beneficent, giving Southern Australia a genial climate and tides that gently lave, scarce wash its shores. A rugged outline is therefore out of the

question; even deep indentations are rare. There are, it is true, a few promontories which may be severed for the general good, the approaches to one or two harbors may be improved, inland navigation may be attended to, but this is all. The great engineering feats which will stand as monuments of Nineteenth or Twentieth Century industry must be left to the older worlds to accomplish.

THE SELF-PURIFICATION OF PEATY RIVERS.

By W. N. HARTLEY, F. R. S. E.

From the "Journal of the Society of Arts."

THOSE chemists who are well acquainted with the classic researches of M. Pasteur, must have experienced a feeling of surprise when informed in the pages of the *Journal of the Chemical Society* that organic matter of a particularly stable character was oxidised and destroyed by the oxygen dissolved in water, under the influence of a comparatively low temperature. M. Pasteur has shown that, even at a temperature of 30° C., the oxygen of the air has but a very trifling action on extremely changeable material, such as the albumenoid matter in yeast water, or a solution of sugar. ("Annales de Chimie et de Physique," 3d series, vol. lxiv., pp. 35 and 36, also p. 71.)

Miss Lucy Halcrow and Dr. Frankland (*Journal of the Chemical Society*, vol. xxxvii., p. 506, Trans.) describe certain experiments, made with the object of testing Dr. Tidy's conclusions, that peaty water in the River Shannon loses more than 38 per cent. of its organic elements by oxidation, during a flow of only one mile. They contend that if peaty water possesses this extraordinary affinity for oxygen at ordinary temperatures, it cannot be necessary to appeal for proofs of it to large bodies of water, which are always liable, more or less, to alterations in the proportion of their organic elements from other causes.

They tried, first, the effect upon air of prolonged exposure of peaty water to daylight, but without agitation; second, the effect upon air of violent agitation with peaty water; third, the effect upon air of violent agitation with waters free from oxidizable organic matter. Their experiments lead to the conclusion that, if peaty matter dissolved in river water is spontaneously oxidized at all (of which they consider there is no sufficient proof), the process takes place with extreme slowness, and cannot be accomplished to any considerable extent in the flow of a river. The evidence proved the fact that peaty matter is less oxidizable than animal matters under the same conditions.

During these experiments, it was observed that a considerable precipitation of brown peaty matter occurred when the strong bog drainage was mixed with a comparatively small bulk of distilled water. This precipitation, it was observed, promised to throw light upon the amelioration of peaty waters which had been remarked by Mr. Bateman and other engineers.

In criticising Dr. Tidy's experiments on oxidation, Dr. Frankland ("On the Spontaneous Oxidation of Organic Matter in Water," Loc. Cit., p. 538) remarks on the apparently superior action that Dr. Tidy attributes to air acting on running water, which is absent in the case of falling water, unless it falls naturally in a river bed. The influence which so favored the oxidation of polluted water, running in rivers with numerous unpolluted affluents, appears always absent when the water is dashed into foam in a glass bottle, violently stirred up with glass rods, and especially when water is merely "exposed to light and air in a bottle."

Whether air has apparently a purifying action on river water, falling or running, superior to that exerted on water contained in a bottle, is a point which has not been proved by Dr. Tidy, though the means of proof are exceedingly simple. It appears to me, after a careful perusal of Dr. Tidy's paper, that all he has proved regarding the Shannon waters, is a decrease in peatiness during their onward flow. It is, therefore, an open question whether the self-purifying process is

due to oxidation. Indeed, Dr. Tidy himself states that, in the case of peat, the quantity of organic matter is kept in check by the following means, which are two, namely:

1. "The inherent power that water possesses of self-purification from the oxidation of the peat by the oxygen held in solution in the water. This process is enormously helped by certain natural and physical conditions, whereby the more complete aëration of the water, and the more intimate contact between oxygen and the peat, is effected."

2. "Mechanical precipitation by admixture with coarse mineral suspended matter. The artificial means of purifying peaty water are, storage, subsidence, and filtration."

We have, therefore, an alternative process of purification which is here termed "mechanical precipitation."

I am disposed to give every possible consideration to the question of oxidation, and am ready to allow that water exposed to air in a closed bottle, is always exposed to the same air, or, if the experiment be made in an open bottle, that it is not brought under the influence of fresh air except by the slow process of diffusion. Now, if it were proved that peaty matter in water was removed by aeration by *fresh air* and oxidation, we should be forced to the conclusion, sunlight being ineffectual, that the fresh air of the country contains some minute constituent, not present in sufficient quantity in the confined space of a bottle to make any perceptible difference in the water. Such an agent is ozone, which, in the atmosphere of the open country, never exceeds one volume in 400,000 of air. Although I have examined this question, I do not propose to offer any analytical data as evidence whether atmospheric ozone can, or may, act as an oxidizer of the organic matter of flowing waters; the evidence at my disposal shows that it does not. I mention this matter to show the train of reasoning which led to the following investigation. In the meantime, I shall only remark here that running waters are subjected to agents other than atmospheric air, which may, and do, sometimes act as purifiers. They are in contact with the river bed, and the soil on its banks.

River waters, as is well known, contain, besides peaty or other organic matter, certain mineral constituents, which are dissolved out of the river banks, or are the result of solvent action on the rocks which lie in the bed of the stream, or which compose the strata through which spring waters pass to feed the river. On the bed and the banks of the river, such solvent action may be expected to be most energetic where waters are dashed against the rocks in the act of precipitation over a fall. It, therefore, seems to me that the relation of the mineral constituents of streams to the self-purification of the water from organic matters, has scarcely been adequately studied by Dr. Tidy, notwithstanding his statements above quoted. I am led to this remark by certain observations of my own, made on different occasions during the last fourteen years.

In October, 1869, during a short residence in Ireland, I first observed that some peaty streams become rapidly decolorized, while others flow for a considerable distances without undergoing any visible alteration.

In August, 1874, while staying in Invernesshire, I remarked the course of the River Affaric, which flows from Loch Affaric through Loch Benĕvian, to join the River Glass, a run of six miles over a hard rocky bed (quartzite, micaceous schist, and basalt). No alteration in the color of the water was detected, for three weeks, while the observations though, were carried on, no rain fell, and there was abundance of sunshine. Several falls occur, and the stream is frequently lashed into foam along its course; it is, therefore, submitted to aeration in the fullest degree. This aeration is of the kind most likely to effect oxidation, the air being of the most highly oxygenated character, and charged with ozone. As the peaty coloration was not of a dark shade, any bleaching it could undergo would most certainly be noticeable. Nothing of the kind occurred, and although this "naked eye" inspection of the river was to my mind perfectly satisfactory evidence of the unaltered condition of the water, I do not wish it to be generally accepted as such. Certainly, nothing occurred resembling the bleaching process recorded by Dr. Tidy as having taken place at Doonass Falls, on the River Shannon, and the oxygenation of

the water was not productive of any visible result.

My experiments on peaty waters in Ireland, in 1869, showed that a certain amount of coloring matter was separated when a certain spring water of 26° of hardness was mixed with a soft peaty water. Furthermore, that the purification was much more effectual when the mixture was softened by an addition of lime water, as the calcic carbonate which separated carried down nearly all the peaty matter. The decolorizing power of aluminic sulphate was found to be such that two or three grains removed in twelve hours the whole of the peaty brown color from ten gallons of water. From this result, it was thought that certain clays, when mixed with water, would have a similar effect, and experiment proved this anticipation to be correct.

Mr. J. Y. Buchanan, formerly of the scientific staff of H. M. S. *Challenger*, in his investigation of the waters of highland lochs, found that the bottom waters were generally perfectly clear and colorless, while on the surface, and for several fathoms below the surface, water was colored with organic matter, and not always quite clear. When accompanying Mr. Buchanan in the steam yacht *Mallard* this fact was brought under my personal observation in Loch Ness, in places where the water is 50 to 110 fathoms deep, and the bottom consists of fine white or blue clay.

At the commencement of the year 1880, I felt that some information regarding the clearing of peaty streams would be valuable, and accordingly I placed my notes at the disposal of Mr. Gerard A. Kinahan, Associate of the Royal College of Science, Dublin, and advised him in the carrying out of this research in my laboratory.* I propose, therefore, to give in detail the methods of examination resorted to, together with the nature of the waters examined, the results obtained, and the conclusions arrived at; but, first, it may be advantageous if I define the propositions which are accepted. They are the following:

1st. Aeration by agitation in bottles is not effective in the purification of waters from organic matter. (Miss Lucy Halcrow and Dr. Frankland.)

2d. Some peaty streams become clear in the course of their flow, while others do not.

It remains for us to examine evidence of oxidation, or of other action, on the organic matter of peaty waters caused by natural aeration, and to investigate t influence of the river bed upon flowing streams.

The Effect of Natural Aeration on Peaty Waters.

In consideration of this question, the evidence available may be gathered in two ways, namely, by the "naked eye" inspection of rivers, and by analytical data concerning the organic carbon and nitrogen in the water. Many most important observations were made on small streams, but those yielding unimpeachable evidence were made on two rivers situated in the County Wicklow, in the course of which occur two very considerable falls of 360 and 700 feet respectively. The first of these is the Dargle River, which is precipitated over a rocky mountain-side, forming the well-known and beautiful Powerscourt waterfall. In October, 1881, on a fine, bright, and warm day, samples of water were collected. There was a slight flood on the river, and the water was unusually peaty, as during the two preceding days rain had fallen upon the hills; but, nevertheless, the water was free from turbidity. The first sample was taken a short distance above the fall, where the river flows through a deep channel in the mica-schist. From this point the water is precipitated over the face of the rock in a thin layer, presenting a remarkably large surface to the air. It is frequently dashed against rocks, and distributed in the form of spray, collecting again into a steady stream at the termination of its descent. It is altogether surprising to see how small a quantity of water is presented on so large a surface of rock. At the foot of the fall, separated 800 feet horizontally, and 360 feet vertically from the channel before mentioned, the second sample of water was collected. No visible drainage of any kind enters between these two points. The long range of mountains constituting the drainage area of the River Dargle, above the fall, is principally composed of granite, the

* Report on the Clearing of Peaty Waters. Second eries, vol. iii. Proc. Roy. Irish Academy, pp. 447, 596.

mica-schist occurring only at a short distance from where the descent commences. Peaty matter is distributed over nearly the whole area. When the two samples were examined in the laboratory, in a vertical tube eighteen inches long, there was no visible difference between them. Both showed a clear dark-brown color, with very little suspended matter, and deposited only a slight brown sediment after standing.

The analysis of these waters was made in the following manner: A litre of each sample was evaporated down to 100 cc. in flasks, the complete expulsion of the water being accomplished in a glass dish, under a glass shade of the form employed by Dr. Frankland. All the precautions observed by Dittmar and Robinson were attended to, and the residue was burnt in a combustion tube open at both ends, the anterior portion of which was plugged with silver wire. In burning the residue in an open tube, the air necessary was very carefully purified by being collected in a glass gas holder charged with diluted solution of caustic alkali. As it was discharged, it passed over caustic alkali, in sticks, through a soda-lime tube, and through two bottles containing oil of vitriol. The current of air was maintained as a steady stream by the pressure of six inches of water, glass taps being used to regulate the pressure.

It is important that no india-rubber joints be used except where absolutely necessary, the air being exposed as little as possible to the india-rubber. The water residue was removed from the basin by a platinum spatula, and the final portions were detached from the glass, by rubbing the surface with some granulated copper oxide. The water residue, after being transferred to a boat of platinum recently made red-hot, is covered with copper oxide previously ignited. The gases from the combustion, passing out of the tube, enter first a small U-tube containing a 50 per cent. solution of sulphuric acid saturated with chromic acid, then a small tube containing fused but porus calcium chloride, pounded into very small pieces, and sifted free from dust. The carbonic acid was collected in a soda-lime tube, the soda-lime being separated by a plug of glass wool from a layer of calcium chloride. These solid absorbent materials were all pounded and sifted, so as to pass through meshes of wire gauze with about 400 holes to the square inch, all dust being separated by a finer sieve. The weighings were made on a balance capable of turning easily with the 1-10th of a milligramme. The nitrogen was estimated by burning the water residue with soda-lime, sifted as before, in a copper boat, in a tube through which passed a stream of perfectly pure hydrogen, incapable of giving any reaction showing presence of ammonia when passed through Nessler solution. It is important that the hydrogen be carefully tested, since it has been found to yield ammonia after very careful purification with solid and liquid reagents, capable of absorbing the alkali. The same supply of gas was found not to contain ammonia after it had been strongly heated in a glass tube. Of course, the presence of nitrogen oxides in the sulphuric acid would fully account for the formation of ammonia, by the action of nascent hydrogen disengaged in the apparatus, but it is scarcely conceivable that this could pass out of the acid liquid in which it would be formed. The apparatus, however, was of the form known as Kipp's, and if the action be rapid and the gas accumulates in the central bulb, the zinc becomes almost dry. Under these circumstances it is conceivable that ammonia might be carried off by the current of gas; but it is scarcely credible that it should pass uninterruptedly through a very efficient drying apparatus charged with oil of vitriol. In all cases precautions were taken to ascertain that no source of error should arise in this way, by making blank experiments previous to commencing the actual analytical operation. The analyses were made in duplicate, and in the weighings for the organic carbon the allowable error was taken as under three-tenths of a milligramme of carbon dioxide—or less than one part of carbon in ten millions of water. Nitrates and nitrites were estimated, the quantity was that merely occurring in rain-water; and as we are not concerned either with these constituents of the waters, or with chlorine and solids, I need not describe further the analytical process. The results obtained from the analyses of the water above and below the Dargle waterfall, are stated in the following table:

ANALYSIS OF WATER FROM THE DARGLE RIVER, ABOVE AND BELOW POWERSCOURT WATERFALL. THE CONSTITUENTS ARE STATED IN PARTS PER 100,000 OF THE WATERS.

	Above the Fall.	Bel'w the Fall.
Organic matter:—		
Carbon	0.946	0.944
Nitrogen	0.072	0.077
Ammonia	0.001	0.001
Nitrogen, as nitrates and nitrites	..	..
Total combined nitrogen	0.073	0.078
Chlorine	0.48	0.88
Total solid matter	4.20	4.40

REMARKS.—Clear, but deeply colored with peaty matter, October 1st, 1881.

The proportions of organic carbon and organic nitrogen, under the circumstances of such effective aëration as a fall of 360 feet in a vertical direction, and for most of the distance in the form of spray, should undergo some alteration if oxidation is possible by aeration. The two constituents in the two samples show no further variation than may be fully accounted for by experimental error, namely, merely two parts of carbon and five parts of nitrogen in 100 million parts of the water.

The next water examined was that of the Carawaystick brook, which flows out of a small sheet of water called Kelly's Lough, situated on the south-eastern slope of Lugnaquilla, at an elevation of 1,700 feet above the sea. The descent of this stream is rapid, for in a flow of less than two miles the water runs at a level lower than Kelly's Lough by 1,200 feet. At one part of its course it is precipitated into the valley of Glenmalure, 700 feet below, falling over a bed of granite and schistose rock in a series of foaming cascades of nearly perpendicular descent, and exceedingly picturesque in appearance.

Aeration and agitation are here as complete as possible. In no part of its course is any diminution in the peaty brown tint of the water visible, though a most careful comparison was made of the waters above with those below the falls.

Samples of this water, collected both in summer and winter, were taken at a distance horizontally of 1,600 feet apart, the vertical height between the points being 700 feet. No side drainage enters with the fall, nor is the water submitted to any action or change of condition, other than complete aeration, which can possibly effect the organic constituents. In January, 1882, when the winter samples were bottled, there was a light mist on the hills, otherwise the day was fine and dry, but not cold. The waters were clear, and of a light olive brown tint. The samples collected in summer, August 15th, 1881, were deeply colored with peaty matter. The streams are generally browner in summer than in winter, and in frosty weather they are often quite colorless. A table, showing the results of these analyses, follows:

ANALYSES OF WATER FROM THE CARAWAYSTICK RIVER ABOVE AND BELOW GLENMALURE FALLS.

IN 100,000 PARTS OF WATER.

	Organic matter. Carbon.	Organic matter. Nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Total solids.	Remarks.
Samples collected in Winter.								
I. Above falls	0.284	0.022	......	0.004	0.026	......	......	Clear, but colored with peat. Collected Jan. 7, 1882
II. " "	0.286	0.025	......	0.004	0.029	1.5	3.26	
III. Below falls	0.284	0.021	.. ...	0.004	0.025	1.6	3.34	
IV. " "	0.289	0.025	......	0.004	0.029	......	......	
Samples collected in Summer.								
V. Above falls	1.06	0.054	trace	......	0.054	1.30	......	Deeply col'd with peaty matter.
VI. Below falls	1.17	0.053	trace	......	0.053	1.4	.. ..	
VII. From Kelly's Lough	0.68	0.054	trace	......	0.054	1.2	......	Rather peaty. Collected Aug. 15, '81

The composition of the water above and below the falls is almost identical; there is no decrease in the amount of carbon and nitrogen, which shows that aeration of the most effective character has no effect of the slightest kind in decreasing the organic constituents. An analysis is added of the water from Kelly's Lough.

I consider the foregoing analyses conclusive evidence, that a peaty river water cannot undergo the slightest degree of purification from its organic constituents, by the natural process of aeration.

I now come to the consideration of the altered conditions in a river water, caused by the reception of a mineral matter of a more or less soluble character.

The Mechanical Action of Insoluble Matter is Devoid of Decolorizing Power.

It was found by experience that the depth of color of peaty water was a fair indication of the quantity of organic matter present, and the following evidence is dependent on this fact:

(1.) At the point where samples of water were taken out of the stream which flowed from Kelly's Loch, another series was collected under the following circumstances: The first came out of the river at the foot of the falls, then at the head of the falls a quantity of sand clay, about a cubic yard in measurement, was thrown into the stream; just above this point the second sample was taken. The whole of the water was rendered very turbid. A descent was then made to where the first sample was taken. After waiting until the turbid water had flowed for some time, a third sample was taken. On comparing the color of these waters, after the turbidity of the last had subsided, it was found that the first and second showed no difference in color, while the third was the darkest.

The following materials were added in large proportions to peaty waters, and after thorough mixing by violent agitation in tall cylinders, the mixtures were allowed to subside. Any diminution in tint of the water was noted by comparison with the original sample.

(2.) A pure quartzose sand, even when added in large quantities to the water, had no perceptible action on its coloring matter.

(3.) Pure gelatinous silica, which acts to some extent as a mordant of aniline dyes, was quite without effect.

(4.) Magnesia was without action.

(5.) Pure carbonate of lime has only a very slight action on the peaty coloring matter.

(6.) Powdered chalk and limestone were found to reduce the tint slightly.

(7.) Clay of different kinds, which had been treated with a seven per cent. solution of hydrochloric acid, so as to dissolve out the soluble matter was found to have only a very slight decolorizing power.

The results of the experiments described as Nos. 1. 2, 3 and 4, prove that there is no decolorizing action on the peaty coloring matter which can be described as *mechanical.* Pure carbonate of lime, powdered chalk, and limestone, are slightly soluble in water, and it is a chemical action of these substances which reduces the peaty tint. The action of the insoluble constituents of clay appears likewise to be due to the iron and alumina they contain, to which the peat coloring matter attaches itself as to a mordant.

The Agents Operating in the Self-Purification of River Waters.

The decolorization of peaty streams by mine drainage.—The Carawaystick brook, running down into Glanmalure, joins the Avonbeg. This river, uniting with the Avonmore at the "meeting of the waters," forms the Avoca River. The changes which occur in the Avonbeg are irregular; but there is, in general, a diminution of the peaty tint.

The Avoca River, however, in the course of its flow, soon receives on its east bank waters from the mines of Tigroney, Cronebane, and Connary, which waters contribute to the streams ferrous sulphate and alumina sulphate in comparatively large quantities, while smaller proportions of such substances as copper sulphate, and arsenic, are found therein. On the west bank of the river, but lower down, are the Ballymurtagh and Ballygahan mines. A sample of water, taken at the tail of the landers, at Ballygahan, there were found 56.8 grains of ferrous sulphate, and 54.7 of alumina sulphate per gallon. This water is capable of removing, in a most complete manner, the coloring matter from fifty times its volume of very peaty water. A dark brown precipitate first settles, and then a deposit of ochre occurs.

The small tributaries of the Avoca River contain considerable quantities of

dissolved mineral matter, as shown by the following analyses:

ANALYSES OF THE WATERS OF THE SMALL TRIBUTARIES OF THE AVOCA.

	Parts per 100,000 of the water.	Remarks.
"Red Road" Stream.	Ferric oxide. Alumina.... 3.8; Copper 0.05	Colorless, slightly acid, ochre in suspension.
Tuniahinch Stream.	Ferric oxide. Alumina. .. 4.8; Copper ....0.02; Total solids..12.8	Colorless, slightly acid, brown sediment.

A sample of slightly turbid but very peaty water, taken from the Avoca River, below Ballygahan mines, deposited a brown precipitate after being at rest for some time, and the coloring matter was much reduced. On another occasion, the tint of the water was not so deep, and after settling, the color was quite removed. The precipitation of the peaty matter is followed by a deposit of ochre.

When a few drops of a solution of ferrous sulphate are added to a quantity of peaty water, a peculiar turbidity is the result, this afterwards increases to a brown precipitate, which collects at the bottom of the vessel, the water being free from peaty coloring matter.

It is very noticeable, in the Avoca River, that the stones are covered with an ochreous deposit, and that ferruginous matter is deposited in the pools. It appears that the ferrous salt in solution carries down the organic matter at the time it is oxidized, for this is the action observed when a pure solution of ferrous sulphate is added to peaty water.

Samples of water were collected on the 16th January, 1882, at Tigroney Weir and Black Dog; between these points, the Avoca River falls only fifty feet in a flow of three miles, and therefore aeration, which, *per se*, has been shown to be without action on the organic matter in the waters of the Caraway stick Brook and the Dargle River is certainly inoperative in this case.

The subjoined analyses of these samples show that with the increase in the mineral matter there is a decrease in the organic matter held in solution:—

ANALYSES OF WATER FROM THE AVOCA RIVER, BEFORE AND AFTER RECEIVING MINE DRAINAGE.

PARTS PER 100,000 OF WATER.

	Organic matter. Carbon.	Organic matter. Nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Total solids.	Remarks.
I. At Tigroney Weir	0.231	0.026	trace	0.011	0.037	1.8	4.88	Slightly peaty.
II. " "	0.229	0.028	trace	......	0.039	......	......	
III. At Black Dog...	0.098	0.019	trace	0.008	0.027	2.3	9.26	Colorless.
IV. " " ...	0.093	0.019	trace	......	0.027	......	......	A trace of iron present. Collec'd Jan. 16, 1882.

The effect of low temperature on dissolved peaty matter.—Mr. G. A. Kinahan remarked that, when there was a considerable quantity of snow on the hills forming the drainage area of the tributaries of the Avoca River, the peaty color of the waters was diminished much below that of ordinary occasions, and, indeed, the waters frequently were quite colorless. Mountain streams presented the greatest difference in appearance in frosty weather; some which were usually deep brown in color became perfectly clear and free from color during frost.

When peaty water is frozen in tall cylinders, a layer of deeply-colored water collects at the bottom, and the water resulting from the thawing of the ice is quite free from color. If the freezing takes place from the sides of the vessels, a core of peaty water collects in

the center. When freezing occurs with great rapidity, a mass of clear ice is formed, in which are liquid enclosures of concentrated peaty water.

When freezing proceeds in a cylinder from above downwards, a clear and colorless block of ice may be removed, and a layer of deeply colored water is left at the bottom. Repeatedly filling up with peaty water, and again freezing, causes the formation of a very small quantity of a brown sediment. It does not appear from this that freezing causes a precipitation of peaty coloring matter in an insoluble form, though it does cause a purification of the water frozen, and a concentration of the peat solution. As, however, it was the water and not the ice of the streams which was under observation, there must be some other cause for its greater purity during frost.

Mr. Kinahan explains it in the following manner:—In frosty weather the surface of the bogs is frozen, preventing the percolation of waters which thus run away clear; and the water supply is often derived from melting snows, which are, therefore, uncontaminated. Thus, in one case, it was noticed that though a quantity of peaty water had collected behind a snowdrift, yet the water flowing from the other side of the drift was quite colorless.

On the Chemical Action of Clays, or the Mineral Constituents of Soils, in Purification of River Water.

From my opening remarks, my views, as to the action of clays in the self-purification of river waters may be gathered. In order to ascertain how far the mountain streams of the county Wicklow are actually, or may possibly be, purified from organic matter by the chemical action of the clay in the bed or on the banks, specimen of clays and disintegrating argillaceous rocks were collected for examination.

Experiments were made by shaking up weighed quantities of the clays with carefully measured volumes of water, contained in tall cylinders. When the water had become clear, a large portion was decanted or syphoned off, and placed in another cylinder, for comparison with the color of an equal volume of the original water. By this treatment, it was found that, provided only a sufficient quantity of the clay was used, any peaty water could be decolorized.

I will now give the description and the analysis of the clays collected by Mr. Kinahan, together with an account of their action.

No. 1.—A fine cream-colored clay, containing fragments of iron pyrites and milk quartz, was found to be a most efficient purifier. When added to peaty water, the greater portion settled down almost immediately, but the very fine particles remained in suspension for some time. Complete subsidence left the water very clear and colorless, when sufficient clay had been used; the sediment was covered with a brown layer, as if the coloring matter was there deposited. With a liter of dark peaty water, 4 grammes of clay rendered the water clear and colorless; 2.5 grammes caused destruction of the color, accompanied by persistent turbidity, which was not apparently decreased by filtration; 1.5 grammes changed the brown color to olive green.

The clay was shaken up with water, in the proportion of 10 grammes of the former to 300 cc. of the latter; the liquid was filtered, and the filtrate was found to possess the property of decolorizing peaty water. Tho clear solution has an acid reaction, and contains a small quantity of ferrous and alumina sulphates. The insoluble portion of the clay was digested with hydrochloric acid, and well washed. Its action as peaty water was scarcely perceptible after this taeatment.

Its composition was as follows:—

Analysis.	Per cent.
Insoluble in hydrochloric acid	92.51
Ferric oxide	2.35
Alumina	2.43

No. 2.—A disintegrated steatic shale of a light yellowish red color. It yields on treatment with water, a turbid yellowish liquid, which clears on standing, the suspended particles subsiding; the resulting clear liquid, which is neutral to test paper, contains very little dissolved matter. Only a very minute inorganic residue is left after digesting 5.5 grammes of the clay with water, filtering and evaporating the filtrate to dryness.

Its composition:—

Analysis.	Per cent.
Insoloble matter	78.28
Ferric oxide	8.12
Alumina	7.96
Manganese	trace

In a liter of water, 25 grammes caused perfect purification. Insufficient additions of the clay caused a persistent turbidity, or an olive green color, as in the previous case.

No. 3.—A bluish clay occurring in a cliff on the Ballynagappoge brook. When added to a brown peaty water, it removes all but the green tint, and leaves a slight turbidity. It forms a dirty turbid liquid when mixed with clear and colorless water, and a peculiar turbidity to some extent remains after the liquid has been left some time for subsidence to take place. This turbidity, which resembles that of imperfectly purified peaty waters, is found to be removed by an addition of a small quantity of a red clay about to be described:—

Analysis.	Per cent.
Insoluble portion	81.79
Ferric oxide	6.72
Alumina	6.47

No. 4.—A brick-red clay from one of the tributaries of the Mucklagh brook. With pure water, this clay yields a perfectly clear solution after the fine particles have subsided. From a liter of water, 20 grammes of clay removed all the peaty matter. It is very efficient in purifying action, but insufficient quantities of the material cause the before-mentioned changes in the water, from brown to olive-green, or else a turbidity which, from the color of the clay, makes the water appear of a red color.

Analysis.	Per cent.
Insoluble matter	72.41
Ferric oxide	9.70
Alumina	9.41

Mr. Kinahan has remarked that the blue clay, No. 3, seems to have been originally a red clay like No. 4; its present condition being due to percolating peaty waters. I will quote what he says on the matter:—

"1st. A similar blue clay overlies No. 4, as seen in the banks along the Mucklagh brook, the passage from one to the other being gradual; red stones and particles of red clay are seen occurring in this over-lying clay.

"2nd. The residues left by both specimens, after digestion in hydrochloric acid, are alike.

"3rd. The percentage of insoluble matter in No. 3 is greater than in No. 4, the more soluble portions, iron and alumina, having been probably removed by percolating waters."

No. 5.—A whitish sandy clay from the banks of the Carawaystick brook, where it occurs in an ancient moraine above the falls. It was this material which was thrown into the stream above the falls, and found to have no perceptible action as a decolorizer. In the laboratory this result was confirmed; even when added in very large quantities, it produced very little alteration in the color of peaty water.

Analysis.	Per cent.
Insoluble matter	93.91
Ferric oxide	1.63
Alumina	2.17

No. 6.—A disintegrating granite, from Aughavannagh, county Wicklow, a rock which underlies the peat of the district. Its constituent minerals are quartz, black and white mica, and felspar, passing into china clay. This specimen acts very slowly, the impurities in the water settle out in a layer after the mineral has been shaken up with the water, and repeated shaking causes a marked reduction in color. The felspar, which is a very hard variety containing very little iron, acts in the same way. It was noticed that the layer of water lying next the mineral for some time in a glass cylinder, was less colored than that above.

Analysis.	Per cent.
Insoluble matter	86.68
Ferric oxide	3.80
Alumina	6.24

The turbidity which is mentioned as occurring after the peat-coloring matter is removed, is due to the pedetic motion of the fine clay particles. Such turbidity is at once removed by an addition of saline matter in solution, as, for instance, common salt, or sea water, or, as is shown here, by an excess of clay. (For further particulars regarding this clay turbidity, see the *Quarterly Journal of Science*, April, 1878, vol. viii., "On the Movement of Microscopic Particles Suspended in Liquids," by Prof. W Stanley Jevons, F.R.S.; also in connection with this subject, "An Explanation of the 'Brownian' Movement," W. N. Hartley, *Monthly Microscopical Journal*, June, 1877.)

The Action of Metallic Hydroxides and Oxides.

It was thought desirable to ascertain the action of commonly occurring forms of metallic oxides as purifiers of peaty water, the method of testing being the same as that carried out with the clays.

The coloring matter of peat was rapidly and efficiently precipitated by aluminic hydroxide, ferric hydroxide, and manganic hydroxide. Alumina is by far the most active, the iron compound coming next. The oxides of these metals were much less rapid in their action. Only after repeated shaking with the water did the manganese dioxide and ferric oxide produce any perceptible alteration in tint.

Aluminic hydroxide was mixed with water, and the solution filtered. The filtered liquid caused percipitation of the coloring matter from peaty water. Hydroxides of iron and manganese, submitted to the same treatment, yield solutions which are quite inactive.

The Action of the Clay in the Bed and Banks of a Stream upon the Water which flows against it.

As an evidence of the active nature of a river's bank as a means of purifying the waters, a careful observation, made by Mr. G. A. Kinahan, has shown that the "Ballynagappoge brook, the head waters of which are deep brown, and the feeders of which are equally peat, is purified to a great extent by the blue clay, No. 3, which forms a cliff on the banks of the stream, and against which the waters wash and remove therefrom a considerable quantity of the mineral." Clay No. 3, has been proved to be a most efficient purifier, and after contact of the waters therewith, the stream is seen to be very slowly but steadily reduced in color. The waters fall about 500 feet in a distance of a mile and a-half, the stream flowing rapidly in a succession of little falls; at about five hundred yards above Rosahane bridge, the rate of fall is diminished, and the rocky bed is changed to one of clay and gravel. The peaty matter is here reduced to a mere trace. Just above the bridge, where the fall is slight, there are several marshy places from which iron stained waters flow into the brook, and below the bridge the water is completely decolorized. The ferrous carbonate contained in the waters from the marshy land, yields ochreous precipitations of peaty matter, and renders the water most beautifully clear and colorless. The ferrous salt is produced by the action of water and decomposing organic matter upon the iron compounds in the clays and gravel. The deposits of ochre on the stones and in the pools of the streams, resemble those caused by the mine drainage waters discharged into the Avoca River. This is a true case of the self-purification of a river water by the action of a mineral constituents contained in its bed and banks.

From a number of experiments made on peaty coloring matter in water, I conclude that it acts like an organic acid, and that it is probably a body of the type of alizarine or litmus, being only slightly soluble, or even insoluble, in pure water, but readily dissolved in water containing traces of alkili, or of soluble carbonate, such as ammonia or potash. With metallic oxides, iron and alumina, it forms insoluble compounds of the nature of "lakes." Lime water also precipitates it. Mineral acids, sulphuric, hydrochloric, and nitric, precipitate it. Peaty water may be perfectly bright, and free from turbidity. These facts, and a further observation that subsidence will not clear a peaty water of its coloring matter, lead to the conclusion that the coloring matter is held in solution, and precipitated as a lake by various mineral bases.

On Dr. Tidy's Conclusions as to Natural Oxidation of Peaty Matter.

On page 295 of the Chemical Society's *Journal*, vol. xxxvii., appears the following passage ("Tidy on River Water"):—

I have had opportunities, during a fortnight's inspection professionally, of studying this subject in that most wonderful of great rivers, the Shannon, along a run of very nearly fifty miles (from Portumna to Limerick), the river receiving, throughout the whole course examined, not only feeders containing—except in one or two cases—an even larger quantity of peat than itself, but the drainage, of a deep coffee tint, and in not inconsiderable streams, from huge bogs covering many square miles of country. I am aware that I am now entering upon a subject where great differences of opinion exist, and I would approach it cautiously.

Let me, then, put the case of the Shannon below Portumna, containing a quantity of peat, represented by 0.9 of organic carbon per 100,000 (for in arguing this question of oxidation I shall, in the main, appeal for manifest reasons to the indications of the combustion process), receiving, as it continues to flow, an enormous quantity of black peaty matter, and streams much more peaty than itself, and drainage from an area that can only be described truly as a peat bog. Should we not expect, I ask, if the water contained in itself no inherent power of purification (I mean, of course, the power of effecting oxidation as the peat), that a sample examined thirty-five miles further down the river, say at Castle Connell, would contain an enormously increased quantity of matter? But the reverse of this is the case. For not only does the two-foot tube tell that the peat is manifestly less, but on analysis the organic carbon at Castle Connell is found to be only a little over one-half what it was at Portumna. What then has become of it? Certain it is the peat could not have evaporated, and I confess I can invent no possible explanation of its disappearance except by believing that, in the course of its flow, it has been oxidized by the oxygen held in solution by the water. At any rate those who hold non-oxidation in running water must explain how it is that the absolute organic carbon, by a flow of thirty-five miles (even in the face of the fact that the river in the course of its flow is receiving enormous volumes of highly peat-charged water) is lessened in so remarkable degree.

Samples of water were taken from either ends of Lough Derg, through which the Shannon flows; the length of the lough in twenty five miles. The difference is the organic carbon of these two samples was only 0.18 parts per 100,000.

A third sample was taken one mile below Killaloe. As a matter of fact, there is less bog drainage going into the river in this short run than at any other spot in the fifty mile flow examined, although even here the quantity that finds its way into the river is not inconsiderable. Nevertheless, in the course of this one mile flow, the organic carbon fell from 0.8 to 0.48. Incidentally, I may mention, that these results were confirmed by the oxygen process, and by the appearance of the water in the two-foot tube. From this point to within nalf-a-mile from Castle Connell, the quantity of peat drainage that enters the river is enormous; indeed, an extensive peat bog, covering miles of country, pours into the river its absolutely black drainage water in full-sized streams. At O'Brien's-bridge, the organic carbon was found again to have risen to 0.84 per 100,000, and I have no doubt, from the two-foot tube observations, although I have no analysis in proof, that below this spot the quantity of organic carbon would have been even greater. This highly peaty water, after a very short run indeed, although (and I must again insist on this point) here and there receiving notable quantities of bog drainage, becomes very manifestly less peaty, the organic carbon being reduced from 0.84 to 0.593 per 100,000 parts.

How can this remarkable result be accounted for? Thus, between the two points where these samples were taken, are to be found the far-famed falls of Castle Connell, where, in the course of a few hundred yards, the level of the river sinks fifty feet. The extent of aeration the water must undergo in these magnificent falls must be considerable. The part they play in effectiug the purification of the water is strikingly manifest, in comparing the dark tint of the water as it foamed and bubbled over the rocks with which it first came into contact in the upper part of the Doonas Falls, with the comparatively light peaty tint the water exhibited as it played over the rocks situated at the lower parts of the falls. I watched these changes of tint for some hours along the course of the falls, and a more magnificent natural experiment I never witnessed. If this alteration in actual quantity, as shown by analysis, is not oxidation, I am totally at a loss to conceive what it can be.

I have quoted Dr. Tidy's own written words, in order that I may not in any way misrepresent his opinions, or the nature of the evidence upon which he founds his doctrine of the oxidation of peaty matter in running water. The organic carbon at Castle Connell was found to be only one-half what it was at Portumna, thirty-five miles further down the Shannon. Let me draw attention to the following words:—

Certain it is the peat could not have evaporated, and I confess I can invent no possible explanation of its disappearance except by believing that, in the course of its flow, it has been oxidized by the oxygen held in solution by the water.

It will be remembered that, at Castle Connell, the river sinks a trifle of 50 feet in the course of a few hundred yards, and the water collected by Dr. Tidy below the falls contained less organic carbon than that collected above. "If this alteration in actual quantity, as shown by analysis, is not oxidation, I am totally at a loss to conceive what it can be." Shortly afterwards, we are told, p. 300—

These, then, in the case of peat, are the natural means whereby the quantity in running water is kept in check.

1. The inherent power that water possesses of self-purification from the oxidation of the peat by the oxygen held in solution in the water.

I have already shown the most complete aeration, effected by a descent over falls 360 and 700 feet in height (not a mere 50 feet), is quite without action—that there is no oxidation of the peaty matter.

The evidence is directly contradictory to the above positive statement.

How then are we to account for the diminution in organic carbon, as traced in the flow of these peaty streams? The question admits of a very simple explanation. Mr G. A. Kinahan visited the Doonass Fall on the Shannon, at my desire and collected samples of water, on both sides of the river, above and below the falls. His analyses showed that there was a greater difference between the two samples from opposite banks, above the falls, than between either of these samples and those taken below the falls. Moreover, the organic carbon was found to be increased in quantity in the samples collected after the water had descended the falls.

A few bottles full of water taken out of a very large river, here and there, are not average samples of the whole water in the river, even when the greatest care is exercised in collecting the samples.

The following remarks from Mr. G. H. Kinahan, of the Geological Survey, who lived for many years on the banks of the Shannon, in the neighborhood of Doonass Falls, and for part of the time at Castle Connell, warrant this statement; and, at the same time, fully account for the discrepancies in the results obtained by Dr. Tidy and Mr. Gerard A. Kinahan.

In the Shannon, between Killaloe and Castle Connell, the flood waters from the counties of Tipperary and Limerick, on the south-east and south, are 'black floods' (peaty water), especially those that flow into it between O'Brien's-bridge and Caslte Connell; while the flood waters from the Clare side, to the north and north-west, except the stream from O'Brien's-bridge bog, are 'red flood,' highly charged with the red muds from the *débris* of the red basal carboniferous shales. These different classes of flood may differently affect the water at the fall of Doonass. If rain falls only in the counties of Tipperary and Limerick, there will be a 'black flood' over the falls, while, if the rain falls only in the county Clare, it will be a 'red flood;' but if the rain is falling on both sides the Shannon, the results will be very different. If, during such a rainfall, you stand at the World's End Weir, Castle Connell, the flood on the Limerick side will be black, and that on the Clare side, red, the two differently-colored waters going separately over the weir, to be slightly mixed below; bnt the great mixing does not take place until they reach the Falls of Doonass, below which the red ferriferous waters are found to have cleared out the peaty coloring matter. The waters flowing over the falls are more often colored with peat than otherwise. While living at Castle Connell, some of the largest floods I saw on the falls were black ones. When the rain falls only to the southward of the Shannon, the 'black flood,' going over the fall is met by a 'red flood' coming out of the Annacotty, or Mulkear River, which neutralizes and destroys the peaty color in the water before it reaches Limerick. On account ot the land on each side of the Shannon, above Castle Connell, the waters on each side of the river above the falls must give very different analyses.

These remarks are in accordance with the observations made on the clearing of the peaty rivers of the county Wicklow by the action of clays and soluble mineral matters.

The analyses of the waters collected immediately above and below Doonass Falls are the following:—

Analyses of Water from the River Shannon. The Samples were Collected from the Right and Left Banks, above and below Doonass Falls.

Parts per 100,000 of Water.

	Organic matter. Carbon.	Organic matter. Nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Chlorine.	Remarks.
Above Falls.							
I. From right bank......	1.036	0.053	0.004	trace	0 057	1.75	Slightly peaty and colored with suspended matter.
II. From left bank........	0 818	0.040	0.002	trace	0.042	1.70	
Below Falls.							
III. From right bank......	1.063	0.049	0.002	trace	0.050	1.87	Collected July 16th and 18th, 1881.
IV. From left bank.......	1.090	0.045	0.001	trace	0.046	1.87	

Dr. Tidy's results, which I conjecture were obtained from an examination of the waters on the Limerick side of the river only, above the falls, may be easily reconciled with the fact that the lessening of organic matter is not caused by oxidation. When we consider that the ferruginous mineral matter, free from bog waters, is discharged on the Clare side, that when mingling with the bog drainage it precipitates the peat, that these waters when flowing on a level must, to some extent, mingle, but when descending falls must be more or less mixed, then the phenomenon witnessed at Doonass Falls is capable of easy explanation. It is nothing more than the mixing of two waters, followed by a precipitation of organic matter contained in one of them.

I have here made no allusion to the question of oxidation in sewage-polluted streams; that has already been treated of by Dr. Franklin. He shows that a flow of between eleven and thirteen miles of a polluted stream has very little effect on the organic matter dissolved in the water, even at a temperature of 18° C. And he has shown that the case of the River Wear, flowing between Bishop Auckland and Durham, which has been quoted by Dr. Tidy in illustration of his theory of oxidation of sewage, the purification is caused by an admixture of highly ferruginous waters, a fact which does aot appear in Dr. Tidy's quotation. I have felt it desirable that facts concerning the possible oxidation of peaty matter in water by natural means should be laid before the Society, and that the cause of the amelioration of such water, which has been not unfrequently observed, should at least receive some explanation in harmony with all hitherto ascertained facts.

Discussion.

The Chairman in inviting discussion, remarked that the paper was confined to the self-purification of peaty waters, and it would be well, therefore, not to travel beyond that subject.

Dr. Voelcker said he felt personally indebted to Professor Hartley for the facts he had brought forward, inasmuch as they fully confirmed the view he had long entertained. At least thirty years ago he had worked with Professor Mulder on the humic acid series, and found it exceedingly difficult to effect oxidation of humic or ulmic acid. The view he had long entertained was that the removal of peaty matter from water was due to a separation of these organic acids of the humic series by mineral matters, especially basic iron salts, aluminic salts, and in a minor degree, by lime salts. The color of peaty water is due sometimes to very small quantities of acids in combination with either ammonia soda or potash; these alkaline ulmates and humates were soluble in water, and gave an intensely brown color if the quantity of acid were at all considerable. The ammonia compounds were easily decomposed, and very frequently in waters which were not very brown, but distinctly yellow, organic acids were in combination of ammonia. These compounds were easily decomposed, sometimes by the new addition of a large quantity of water, as Dr. Frankland had shown; the brown colored humic acids being precipitated. If he were asked to name a solution which was perfectly neutral when cold, and gave off ammonia on boiling, and yet remained neutral on cooling, he should say, try the experiment of extracting some rotten farmyard manure with water; when cold it would be perfectly neutral, but, on boiling, the ammonia would fly off, and a precipitation of brown colored humic acids would take place, and the clear liquid was neutral. He mentioned this to show that it was possible to have peaty waters very little colored, and containing more saline ammonia than many other waters; it also explained possibly the larger proportion of ammonia in peaty waters than in most other potable waters. He was glad the Chairman had confined the discussion to the question of peaty waters; and he must be allowed to utter a caution not to apply the remarks made in the paper to the self-purification of ordinary rivers, polluted with sewage. There were great differences in the facility with which various organic matters were oxidized; some oxidized so rapidly that you could not lay hold of them, whilst others were very difficult to oxidize or burn away; even with oxide of copper they could not be burned completely without the aid a a stream of oxygen or of chlorate of potash or some similar powerful oxidizing agent. The question of self-purification of ordinary river waters had been care-

fully avoided in the paper. It was unfortunate that Dr. Tidy had used an illustration which was not strictly accurate when he referred to the self-purification of peaty waters as an act of oxidation; whereas he himself fully agreed with Professor Hartley that the change was mainly due to the intervention of mineral matters. On the other hand it was also unfortunate that experiments on oxidation should have been with a substance, the very last that he should have chosen, if he wished to ascertain whether objectionable matter in river water was removed by oxidation. Of all these objectionable matters, there was hardly anything so stable and persistent in character as peat. It would be only going one step further to shake up lignite, or even powdered carbon or coke, with air and water, and see what amount of oxidation you would get. Certainly, if he wanted to show something which did not take place, he should choose the oxidation of peat; this, however, proved nothing as to the self-purification of ordinary polluted river water.

Professor Corfield concurred in the views just expressed. He should like to ask Professor Hartley if it had occurred to him that the large amount of evaporation which must take place in the summer, as the water went over the falls, might not account for the concentration of the water to some extent; and whether he did not prove too much in showing that the organic nitrogen and carbon were not reduced, and even in some cases increased; whether such apparent increase might not be due, especially in the case of chlorine, of the actual concentration of the water by evaporation. This took place in water flowing over land, especially in the summer, as sewage water sometimes passed off at the other side of a field more concentrated, as regards several constituents, than when it came on.

Mr. Homersham said it was quite correct, as stated by Mr. Hartley, that peaty water, at a warm temperature, held in solution a greater quantity of peat than when cold. In 1847, he had occasion to have some very exact experiments made on this subject, by the late Professor Ronalds and the late Mr. George Newport, F.R.S. The results were as follows:—Water from 57° to 65°, held peat in solution to the extent of about five grains per gallon; when the temperature was increased from 55° to 67° it held something like $7\frac{3}{4}$ grains; and when it was increased to 84°, there was as much as 14 grains. This was ascertained with water and peat from the Manchester water-works drainage ground, the peat, in many places, being 16 feet thick. Professor Hartley had treated the subject purely from a chemical point of view, but those who had practical experience of peaty water, and other water collected in large reservoirs, knew that something else had to be taken into consideration. There was the biological aspect of the question. You could not take a slow stream, running even a short distance in warm seasons, without getting a variety of living organisms in the water, first vegetable and then animal; these tended to work a marked effect on the color of the water. If you took water such as that from Loch Katrine, or the large impounding reservoirs that supplied Manchester, you would find twenty or thirty different sorts of organisms, vegetable, animal, and diatomaceæ in a gallon. If he had been seeking a title for the paper he should have called it, on the self-pollution of river waters, for he knew of no river where the water at its source was not much purer than it was lower down. In the spring, blossoms, and in the autumn, leaves, unavoidably fell into it, besides other foul matter, and thus the water became impure the further it flowed. Even in districts where peat lay heavily on the ground, you often got springs issuing from the sides of the hills of a most pure character, perfectly free from organic, and almost free from mineral impurity, but when that water was conducted through closed pipes, and was impounded in spring water lodges for bleaching purposes, vegetable organisms soon pervaded the water. He had seen very pure water at the head of a stream in dry weather, which in a very short run was filled with large quantities of a very fine weed, so much so, that in warm weather it took a man every three or four miles to clear it out. The weed grew rapidly, and it decayed almost as rapidly. As soon as it decayed, small animal life appeared in the water preying upon it. He knew of no river or spring water exposed to the air which did not get, more or less, polluted in this way.

Mr. Warington thought it would be found, in Professor Way's earliest paper on the absorptive power of soils, that clay would decolorize an aqueous solution brown in color from the presence of organic matter. He found that such solutions, when filtered through clay, or when clay was added to them in sufficient quantities, were decolorized. There was no doubt Mr. Hartley had pointed out what was the real process in nature by which these coloring matters were discharged from peaty water. Professor Detmer, who had worked a good deal on humic acid, had come to the conclusion that it was a colloid body, and this enabled one to understand better how it might be carried down in a pseudo-mechanical way by precipitants. Hardly any soil could be taken, if it contained clay, which would not be found perfectly efficient in removing the coloring matter from water contaminated with peat. But if they came to the conclusion that peat was not oxidized in water by aeration, it did not necessarily follow as a corollary, that this non-oxidation was due to the fact that peat was a vegetable substance. It had been too hastily concluded by some that vegetable nitrogenous substances did not nitrify, whereas animal nitrogenous substances did so readily. Although some vegetable substances were hard to be attacked by oxidizing agents, whether by organisms, or by the oxygen of the air, yet all were not alike, some being easily oxidized. If they wished to know what substances were capable of being destroyed in a river, they had simply to inquire whether they were capable of supporting the life of bacteria, or of affording food to plants; if so, they would, under favorable circumstances, be destroyed.

Mr. De Rance said he should like to offer a remark in confirmation of the views of his colleague, Mr. G. H. Kinahan. It was noticeable in the English Lake District, where the tops of the hills were covered with peat, and the water very brown, that when it came in contact with the refuse from the mines, it soon became colorless. It was worthy of note, also, that in areas where, from the nature of the strata, the water was of some hardness, from the presence of chemical constituents, it was far less peat-stained than in soft-water districts. It was only in areas like portions of Lancashire and Cornwall, where the strata were millstone or granitic, yielding soft waters, that you got those deep amber colored waters which painters were so fond of delineating. This no doubt was of some importance in a chemical point of view, but being a geologist, he was not prepared to discuss it. With regard to the differences observed in summer and winter, he might remark that the sources of these rivers were generally at a considerable elevation, where the temperature was low in winter; and, in fact, last autumn, when engaged on the geological survey of Yorkshire, he had found the frost so hard, that he could walk in safety on what, in the summer, would have been an impassable quagmire; and the snow, which was then lying on the ground, being melted by the sun, ran off comparatively pure. In summer, again, there were dry times when the peat rotted away, and a heavy thunder shower would carry down large quantities of it; but in winter, when the wet weather was more continuous most of the peaty matter would be carried down the first day or two, and afterwards the water would be comparatively pure

Mr. R. B. White said indigo, annatto, and other coloring matters were precipitated from water by lime, the process being well known to practical dyers. In the Cordilleras and the Andes there were large areas of peat bogs, and the rivers descended not a few hundred, but six or seven thousand feet, before they reached the plains, and as they did not lose their color in the descent, the observations of Prof. Hartley were well borne out. But after they reached the plains and their waters mingled with other rivers containing large quantities of alluvial matter in suspension, the water became comparatively clear, and much more drinkable. The natives of those countries were quite aware that those waters which appeared a little turbid were more wholesome than the clear but dark colored waters from the hills.

Mr. Hartley in reply, said the amount of evaporation in one of the experiments could have been next to nothing, the air being saturated with moisture at the time. A slight effect might in some cases be produced from this cause. With regard to the biological aspect of the question,

and the disappearance of the carbon and the nitrogen in the water from this cause, it must, of course, be taken into consideration in a sluggish river, and especially in the summer time; but in the time occupied in falling 360 or 700 feet no biological action could take place, and therefore the cause of the reduction of carbon or nitrogen must be sought either in the chemical action of the air or of the material mixed with the water. He had not referred to Professor Way's papers, as he had worked at the subject entirely from his own point of view, but he was glad to find that the results agreed so well.

Mr. WARINGTON said he did not mean to intimate that Professor Way had experimented on peat, but with such brown drainage waters as occurred on a farm, colored with humic matter.

Mr. HARTLEY said the results obtained by Prof. Way seemed to confirm those obtained by Mr. Kinahan in his laboratory. With regard to the descent of peaty water over falls of 5,000 or 6,000 feet, with the results which had been mentioned, it was a most satisfactory confirmation of the view that there could be no considerable amount of oxidation taking place by the action of the air, because, if such an action took place as had been supposed with a fall of fifty feet, in the case of the South American rivers, they ought to be completely bleached, even if they were almost black before the aeration took place.

RESERVOIR CONSTRUCTION.

By MAX KRAFT.

"Wochenschrift des österreichischen Ingenieur-und Architekten Vereins. From Abstracts of the Institution of Civil Engineers.

THE reservoirs constructed to supply the motive power to the machinery employed in the mine-workings near Freiburg are reckoned among the most important of the kind in Germany. They form an upper and a lower system of reservoirs of various capacities, and are situated between the rivers Mulde and Flöka. The catch-water drains and supply channels are together over 10 German miles in length, and are constructed, for the most part, in solid masonry of an oval section, 5 feet 8 inches in width, and 2 feet in depth; some of them are even navigable.

The water thus collected is stored in eleven reservoirs, of which the most important are the Grosshartsmannsdorf, the Dörnthal, and the Dittmannsdorf.

The country being open, these reservoirs are noteworthy for the length rather than for the height of their banks. As regards capacity, the Lower Grosshartsmannsdorf is the most important, while the Dörnthal reservoir has the highest embankment.

Let L = length of embankment.
H = height of embankment.
h = height of embankment above water-level.
S = width of base of embankment.
B = width of top of embankment.
h_1 = depth of outlet below top water-level.
w = width of waste weir.
t = depth of waste weir.

Then the following table gives the chief dimensions of the most important reservoirs:

Name of Reservoir.	Capacity.	Area.	L	H.	h.	S.	B.	h_1.	w.	t.
	Cub. meters (= 220 gallons.)	Sq. met. (=10.764 sq. feet.)	Meters (=3.28 feet.)	Met.	Met.	Met.	Meters.	Met.	Met.	Met.
1. Up'er Grosshartsmannsdorf	570.790	194,890	491.0	10.43	0.737	55.7	11.4 to 13	..	3.4	1.14
2. Middle do.	320,171	122,324	629.5	9.8	0.74	53.5	15.9 " 17	9.5	1.8	1.10
3. Lower do.	1,456,116	601,926	725.4	8.3	0.89	46.6	17.0	7.4	..	..
4. Dörnthal	1,283,869	199,029	284.4	16.5	0.74	54.0	12.5	13.1	6.8	2.3
5. Dittmannsdorf	528,982	101,726	376.2	13.1	0.89	45.2	11.4	8.5	7.9	1.9

The total capacity of the eleven reservoirs that constitute this system is 5,083,-324 cubic meters, equal to a daily supply of 13,926.9 cubic meters, without taking into account the fact that these reservoirs are filled 1.5 time in the course of the year.

Again, taking B and S in terms of the height of bank, and

$a+a_1$=the sum of both slopes in terms of H.

q=capacity corresponding to each unit of height of bank.

q_1=capacity correspondiug to each unit of length.

f=capacity corresponding to each square meter of the area enclosed.

The following results are obtained:

	B.	S.	$a+a_1$.	q.	q_1.	f.
				Cubic meters.		
1. Upper Grosshartsmannsdorf ..	1.09–1.2H	5.3H	4.1–4.2H	54,726	1162.4	2.92
2. Middle do.	1.53–1.7H	5.4H	3.87–3.7H	32,671	514.1	2.62
3. Lower do.	2H	5.5H	3.5H	175,074	2007.3	2.42
4. Dörnthal	0.75H	3.2H	2.5H	77,810	4514.2	6.45
5. Dittmannsdorf................	0.87H	3.4H	2.53H	40,380	1406.1	5.20

Which furnish a ready means of comparing the relative advantages as regards the situation and construction of each reservoir. A similar table, showing the cost of each, unfortunately cannot be constructed, as the data necessary are obtainable only for those built in later years, viz., the Dörnthal and Dittmannsdorf reservoirs, which cost per cubic meter of capacity 0.354 and 0.572 of a shilling respectively, which is, considering the dimensions of the banks, exceedingly low in comparison with recent masonry dams. The embankments on the Freiburg system are similar to those in Austria, so far as the materials, clay and earth, employed in their construction are concerned, but differ in the manner in which they are disposed. In Austria the puddle is laid in the center; in Freiburg it forms the inner slope of the bank, which has this advantage, that it keeps the entire bank dry and free from water; whereas the Austrian mode of construction admits of it penetrating through one-half of the bank, which, under certain circumstances, may be attended with disastrous consequences.

The Dörnthal reservoir is an excellent type of all the others. It is very well situated in a valley running south-east and north-west, and was designed originally, in 1787, with a bank 27½ feet high; this, however, was increased to 33½ feet in 1791, in which year it was built. In 1842–44 the present bank was constructed at its present height (54 feet) on a new site. The base of the bank reposes on the *débris* overlying the gneiss, through which the clay portion of the bank is carried down 11½ feet into the solid rock, which was blasted out to a width of 56 feet; below this again, a trench, 9 feet wide and 6 feet deep, was also cut the entire length of the bank, terminating at each extremity in returns at right angles to the center line of the bank. Thus the inside portion of the bank is composed entirely of a mass of clay (56 feet) at its base, and 18½ feet at the top; its outer slope being protected from the action of the waves by squared stone pitching. The top of the bank is also covered by a 1 foot 7½ inches coating of well-rammed puddle, and on its inner edge a solid masonry breast-wall, extending its entire length, is built.

Three cast-iron out-let pipes are laid in the bank at different levels; the valves of these pipes are of the ordinary Austrian pattern, and are worked from the valve-house by square iron rods resting on rollers.

The drainage from the country in the neighborhood of the reservoirs is, for the most part, intercepted by catch-water drains, and supplied to the landowners and mill proprietors, whose claims have to be considered. The waste-weir is of solid ashlar; a series of cast-iron standards is provided, the grooves of which receive planks 6 inches wide and 3 inches thick; each of these is provided with an iron hook in the center, so that by means of a rod they can be lowered or raised at pleasure, and the top water-level of the reservoir regulated accordingly. The

waste-water course descends by two vertical steps to the level of the artificial canal provided for the overflow. The reservoir is supplied from the Friedrich Benno tunnel and the brook flowing into it; the latter has, however, to be delivered undiminished into its natural channel, for the use of those entitled to the water.

BACK PRESSURE ON VALVES;

OR THE EQUILIBRIUM LINE BY EXPERIMENT AND THEORY FOR BROAD-SEATED VALVES.

Read at the Annual Meeting of the American Society of Mechanical Engineers, November, 1882,

By S. W. ROBINSON, Prof. Mechanical Engineering Ohio State University.

The title of this paper perhaps poorly indicates its true import. More specifically, an attempt is made to determine by experiment what fractional part of that pressure exerted upon the top surface of a valve by a fluid is required, when acting from below, to raise that valve from its seat. The valve is supposed to have a broad surface of contact with its seat, as in the case of a slide valve in the steam-chest of an engine where the flanges have a broad lap.

Every designer of steam engines with slide valves finds the question asked of himself, "What force is necessary to slide the valve on its seat?" The first showing of an answer may be, "It depends on the pressure and the coefficient of friction." Probably but little is positively known about either the *effective* pressure or the coeffcient of friction in this case. The present object is to throw light upon the question of effective pressure.

To illustrate, suppose a common D slide valve has a surface contact with its seat of $12'' \times 12''$ outside, less a $5'' \times 10''$ D cavity inside, as shown in Fig. 1, the cavity being supposed in communication with the exhaust. It seems evident that the effective pressure is not $p_1 \times 12 \times 12 = 144p_1$, where p_1 is the pressure in lbs. per square inch, because why may it not be nearly the $p_1 \times 5 \times 10 = 50p_1$ as due to the cavity itself? The most thoughtful, probably, will assume some area between the inside and outside, in the neighborhood of that inclosed by the dotted line, by which to multiply p_1 for the effective pressure. This line I hereafter in this paper call the "*equilibrium line*," and the area inclosed within it the *equilibrium area*," because the pressure p_1, multiplied by it, equals a force which, acting from below, will just lift the valve from its seat.

When the equilibrium area falls between that of the cavity and of the outside of the valve, it appears that the fluid, pressing on the top of the valve creeps under the edges to some extent, and that the film of fluid under the valve near the cavity relieves itself by escape into the cavity. It would seem from this reasoning—required for explaining the equilibrium line—that for true surfaces, a film of almost infinitesimal thinness is constantly creeping along between the "contact" surfaces toward the cavity, and that on this route the pressure of the creeping fluid is continually falling, starting with nearly that of the higher pressure side of the valve, and ending at the cavity with the pressure of the latter.

When the whole lifting pressure exerted by the creeping fluid between surfaces is known, the equilibrium line can be located. The theoretical determination of this lifting action is the same as that for finding the force tending to throw a packing ring from its seat. Formulas for the latter are given in this MAGAZINE, Vol. XXIV., p. 441, and following, in my paper on *A Rational System of Piston Packing.** According to that paper it appears that a rectangular valve like Fig. 1, and for an incom-

* I take occasion to here correct an oversight in this paper as published in this Magazine, but corrected as appearing in the proceedings of the Am. Soc. Mech. Engineers, viz.: In lower line of 1st column of page 445, after "Hence," add "hereafter calling P the absolute pressure, and p_2 the back pressure;" and in equation (9) read the first member "$P-p_2$." Also page 448, in the "Practical Table for Single Ring Packing," regard the figures "4," "6," "8," "10," as raised up one place higher; and below 10 add "12." Also opposite 4, thus raised, put at the head of the 1st column to the right, ".0431;" and at head of last column ".0451."

pressible fluid, the pressure falls on a straight-line declivity from p to p_2, p being the pressure just at entry, and p_2 that within the cavity. The fall from p_1 to p is due to the acceleration for entry into the space. The latter, it is shown in the paper cited, will be a small fraction of the whole, but will be neglected here.

In Fig. 2 let the shaded area stand for a section through a D slide valve, like Fig. 1, the square outlines being best for the present purpose.

If p_1 be the absolute pressure on the top of the valve, and p_2 the absolute back pressure in the cavity, then, according to to what is said above, EF will represent the line of falling pressure from p_1 to p_2. Also it is evident that if the area IJKL stands for the total absolute downward

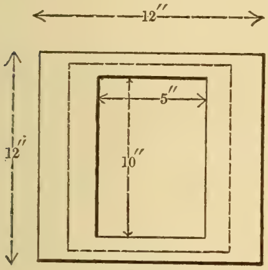


Fig. I.

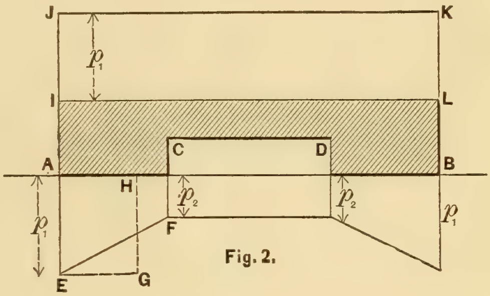


Fig. 2.

pressure upon the top of the valve, then the area AEFC will represent the total absolute lifting pressure at one side of the valve due to the creeping fluid along AC, and tending to raise the valve from its seat. Now draw EG parallel to AC, and make the area AEGH = AEFC. Then the lifting pressure of the creeping fluid neutralizes so much of the top pressure IJKL as lies directly over the length AH. Hence H is the position of the equilibrium line. Likewise we may find the point on BD.

From the figure it appears that AH will equal a half AC only when $p_2 = 0$.

As an example, suppose $p_1 = 60$ and $p_2 = 15$. Then the mean pressure on AC is

$$P = \frac{p_1 + p_2}{2} = 37.5$$

and $$P \times AC = p_1 AH,$$

or $$AH = AC\frac{P}{p_1} = AC\frac{37.5}{60} = AC.625.$$

Hence the equilibrium line is nearer C than A. More specifically, it appears that $62\frac{1}{2}$ per cent. of the pressure upon that part of the top of the valve which lies above AC and BD is counteracted by the lifting action of the creeping fluid.

This supposes the fluid incompressible like water (nearly so). Hence the above calculation is suited to the case of a valve working in water. For quite a range of pressures under this case we may quote from the paper on piston packing referred to:

$\frac{p_1}{p_2}$ =	$\frac{P}{p_2}$ =	$\frac{P}{p_1}$ =	
2	1.5	.750	…..A.
" 4	" 2.5	.625	
" 6	" 3.5	.583	
" 8	" 4.5	.562	
" 10	" 5.5	.550	

where p_1 is the absolute pressure upon the top of the valve, p_2 the absolute back pressure, and P the mean pressure due to creeping fluid. Then

$$AH = AC\frac{P}{p_1} \quad . \quad . \quad . \quad (1).$$

thus locating the equilibrium line.

If the fluid is elastic, like air, and the flow in the space AC is adiabatic, we have from the same source:

$\frac{p_1}{p_2}$ =	$\frac{P}{p_2}$ =	$\frac{P}{p_1}$ =	
4	2.72	.680	…B.
6	3.94	.657	
8	5.18	.647	
10	6.42	.642	

And for the isothermal flow of elastic fluids:

$\frac{p_1}{p_2}$ =	$\frac{P}{p_2}$ =	$\frac{P}{p_1}$ =	
4	2.80	.700	…C.
6	4.09	.682	
8	5.41	.676	
10	6.73	.673	

These tables differ but little, and values may be selected to suit any case in hand,

including even a condensable fluid like steam, because whether steam, or water due to condensation, fills the space AC, the case will fall within the tables above. If an exception is to be made on the supposition that, with steam, the percentage of water will increase on the route from A to C, it will be to the effect that the lifting force is greater than the calculation would give, and hence on the safe side.

To illustrate further the use of the above tables and formula, suppose a D slide valve in a steam or air chest is 10 inches over all, and with a D cavity of 4 inches. Each flange will then be 3 inches if equal. Further, take $p_1=90$, $p_2=15$. Then

$$\frac{p_1}{p_2}=6 \quad AH=AC\ .682, \text{ or } AC\ .583$$
$$=2.05 \text{ inches or } 1.75 \text{ ins.}$$

if the last table be selected, or the first respectively.

Hence, doubling the values, we get for both flanges outside the equilibrium line: 4.1 or 3.5 inches respectively, or within the equilibrium line 5.9 or 6.5 inches respectively.

Hence the effective pressure of the valve upon its seat is as though the last-named widths of equilibrium area only were subject to the pressure p_1

THE EQUILIBRIUM LINE BY EXPERIMENT.—In the Spring of 1881 Student J. H. McEwen subjected this question of back pressure to experiment, in my mechanical laboratory, as a practical problem in course. The appliance explained below was made by him for the purpose. In experimenting the data were procured partly by Mr. McEwen and partly by Mr. C. F. Marvin. The results obtained from reducing the data to the valve are given in the plate.

The appliance employed is shown in section in Fig. 3, in which A represents the valve, B its seat, D the bed-plate of the containing chest, C an inverted hollow piece, which, when bolted to D, together constituted the chest, G a nozzle for admitting the fluid pressure, H a cock for draining the chest, E a lever, which, by means of a strut reaching up through the seat, will lift the valve A, and F the scale pan for loading the lever.

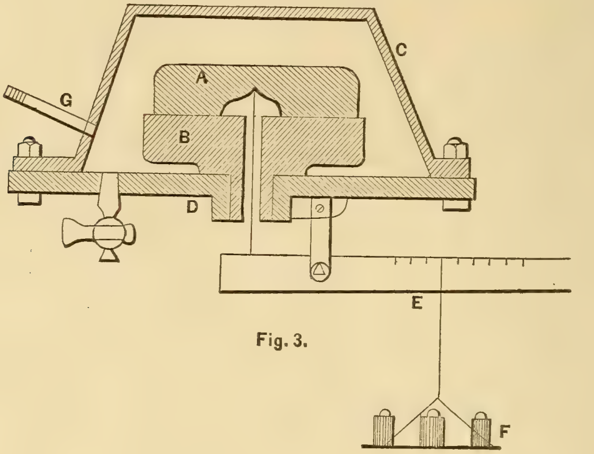


Fig. 3.

The valve and seat are circular in plan, and not rectangular. They were carefully fitted together by facing truly in a lathe, and then by grinding with emery. The grinding was done partly in the lathe and partly by hand with cross motion. It is perhaps to be regretted that this fitting was not done by scraping, but as they were, the surfaces possessed the properties of surface plates in high degree. Placing A upon B with clean joints, the former would float upon the latter for five or ten seconds, lubricated by air. When settled, A would lift B and support it for a considerable time. The surfaces were mirror-like for smoothness throughout. Though the job was well done for grinding with emery flour, yet it is believed that a somewhat more satisfactory contact could have been ob-

tained by the scraper. The outside diameter of the valve A is six inches, and its cavity two and one-tenth inches. The seat B was about three-fourths of an inch larger than A outside, with a central aperture of about one inch. This excess of surface on B was intended for insuring contact on the entire lower surface of A, even should it get eccentric with B. The seat B was mostly free from the bed-plate, so as to relive it from the strains and flexure of the latter. A neck projecting down from B was pressed into the bed D, with a free space between B and D, thus supporting B on a short leg. The strut from the lever E nearly filled the central hole in D, and extended to the bottom of the cavity in A. This cavity in A was made conical at the center, so that the strut would always carry A concentric, and poised as it was raised. The thickness of A and B was about two inches. The lever was borne on knife-edges.

Steam is the only fluid experimented with. It was admitted at the nozzle G; condensed water was let out at H.

In the experiments weights were put on till the valve was raised, or, as in some cases, the weights were shifted along the graduated lever. The pressure of steam in the chamber upon the top surface of the valve was varied from 10 to 84 lbs. per square inch by gauge, or from 25 to 99 lbs. per square inch absolute. The results reduced to the valve are given in the plate where the circle dots were obtained by one series of observations and the cross points by another, the two series being several weeks apart in points of time. The vertical scale at the left is for the steam gauge pressure in the chamber. The horizontal scale gives the number of lbs. pressure per square inch of the cavity area which was necessary to lift the valve. The figures given in this scale were reduced to the valve from the experimental data read off from the weighted lever, the weight of the lever, scale-pan, strut and valve being taken into account, but not the atmospheric pressure to which the cavity under the valve was exposed. Hence the figures in both scales are apparent pressures per square inch—one for the entire area of the six-inch circle of the valve, and the other for the area of the circle of the cavity, two and one-tenth inches in diameter.

The curve running among the dots is supposed to be drawn where the sum of the squares of all the distances from the line to the dots is the least, that is to say, that line is the probability line for all the observations, according to the theory of "*least squares.*" It is assumed, however, that the line should run through the origin. From this line the figures in the first two columns of the following table were read off:

TABLE D.—FOR A CIRCULAR VALVE 6 INCHES IN OUTSIDE DIAMETER, WITH A CIRCULAR CAVITY 2.1″ DIAM. EXPOSED TO THE ATMOSPHERE.

Appt. pressure lbs. per sq. in. on valve 6″ diam. p_1-p_2	Appt. pressure lbs. per sq. in. on cavity of valve 2.1″ diam. p'	Equilib. area such that $a'(p_1-p_2)$ will just raise valve: sq. inches. a'	Diam. of equilib. area inches. d.
5	8.	5.6	2.6
10	17.	5.8	2.7
15	26.	6.0	2.8
20	36.	6.2	2.8
25	46.	6.4	2.9
30	57.	6.6	2.9
35	69.	6.8	2.9
40	81.	7.0	3.0
45	95.	7.3	3.0
50	112.	7.8	3.1
55	129.	8.2	3.2
60	150.	8.7	3.3
65	172.	9.2	3.4
70	198.	9.8	3.5
75	230.	10.5	3.7

A better notion of the quantities given in the table and plate perhaps can be obtained from Fig. 4, giving a section of the valve and its seat. Within the valve outline are drawn arching circles springing from the seat. These define the greatest and least equilibrium lines, which here are circles to the diameters 3.7″ and 2.6″ respectively, between which all the figures in the last column of the table fall. The interpretation of Fig. 4 is this: For 75 lbs. apparent pressure per square inch upon the top of the valve, the equilibrium circle is 3.7″ in diameter. That is, if the valve be taken from the chamber, and its seating face be turned off or cut away, with the exception of a sharp circular line to the diameter of 3.7″, so that when the valve is returned to its seat, this circle is the only bearing of the valve upon its seat; then the same lifting force will be re-

quired to raise the valve from its seat as before, for this stated pressure of 75 lbs. Again when the stated apparent pressure is 5 lbs. per square inch upon the top of the valve, then the same lifting force will raise it as when its face is cut away to a ring-line bearing of 2.6″ in diameter. In each case the atmospheric pressure is in full upon the cavity.

The relation between the pressures and areas may be expressed thus:

Let p_1=the absolute pressure per square inch within chamber.

p_2=ordinary back pressure upon the cavity.

p'=lifting force per square inch of cavity, approximate.

P=mean absolute lifting pressure per square inch, exerted by the creeping fluid between A and B, Fig. 3.

A=area of valve, here 6″ diameter.

a= " " " cavity, here 2.1″ diameter.

a^1=equilibrium area.

Then,

$$p_1\mathrm{A}=p_1(\mathrm{A}-a')+p'a+p_2a'$$

or

$$a'(p_1-p_2)=p'a$$

$$a'=a\frac{p'}{p_1-p_2} \quad . \quad . \quad . \quad (2).$$

As an example, take the pressure p_1-p_2=50 lbs. for the first column of table. Opposite this, in the second column, we find p^1=112 lbs. These, in the above formula, observing that a=3.46 square inches, give a^1=7.8 square inches, as found in the third column, for the equilibrium area. But this formula is only applicable to this particular valve.

It seems desirable that the theoretical formulas given above for rectangular valves be brought to the test of verification or disproof, by this series of experiments. To do so it will be necessary to deduce a general formula for circular valves in the same way as was that for rectangular valves, and then compare that theoretical formula for circular valves with the experimental valves of the table. If the theoretical results agree with the experimental ones, then not only the theoretical formula for circular valves is verified, but that for rectangular valves, also.

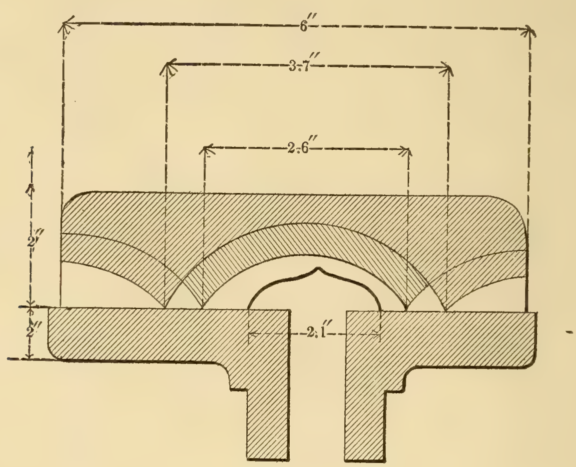


Fig. 4.

From my paper on *A Rational System of Piston Packing*, above referred to, p. 446 of this Magazine, we may quote,

$$p-p_2=\frac{\delta fsl}{a}\,\frac{v^2}{2g} \quad . \quad . \quad . \quad (3).$$

as expressing the fall of pressure p to p_2 in a space conducting a fluid of invariable density d, with the velocity v, where l is the length of the space, a its section, s the perimeter of the cross section, and f the coefficient of friction of the fluid upon the surfaces.

If the space be very thin, with a thickness t, then, for a unit's width we have s=2 and a=t, so that

$$\frac{s}{a}=\frac{2}{t} \quad . \quad . \quad . \quad . \quad (4).$$

Then for a very short length we may put dx for l, and dp for $p-p_2$, giving

$$dp=\frac{2\delta f}{t}\frac{v^2}{2g}dx \quad . \quad . \quad . \quad . \quad (5).$$

Applying this to the part $a\ b\ c\ d$ of the space A B C D, where the thickness is constant, the velocity of the incompressible fluid will vary as it moves from A C to B D. This velocity will, in fact, be inversely proportional to the distance from O. So that, if v_2 = the velocity at B D, we have, if r=B O, A B=b, A O=R, and a O=x,

$$\frac{v}{v_2}=\frac{r}{x} \quad . \quad . \quad . \quad . \quad (6).$$

Then, replacing v by its value,

$$dp=\frac{2\delta f}{t}\frac{v_2^2}{2g}\frac{r^2}{x^2}dx \quad . \quad . \quad (7).$$

Integrating this between the limits AO and BO; or x and r, and p and p_2, we have

$$p-p_2=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left(\frac{1}{r}-\frac{1}{x}\right) \quad . \quad . \quad (8).$$

Here p is the pressure in the space A B C D at the line $a\ d$, situated at the distance x from O. This line is circular, and may be considered as extending to the entire circle about O. Then the absolute pressure upon a circular strip of width $d\,x$, and radius x, is

$$p2\pi x dx,$$

and for the entire under surface of the valve A, the total absolute lifting pressure is

$$P\pi(R^2-r^2)$$

a quantity which is equal to the integral of the last expression above. Writing this equation, and at the same time introducing p form equation (8) above, we obtain

$$P\pi(R^2-r^2)=\int_r^R\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left(\frac{1}{r}-\frac{1}{x}\right)2\pi x dx$$
$$+\int_r^R p_2 2\pi x\,dx$$
$$=\frac{2\delta f}{t}\frac{v_2^2}{2g}\pi r^2\left\{\frac{R^2-r^2}{r}-2(R-r)\right\}$$
$$+p_2\pi(R^2-r^2)$$

Whence,

$$P=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2\left\{\frac{1}{r}-\frac{2}{R+r}\right\}+p_2$$
$$=(p_1-p_2)\frac{R}{R+r}+p_2 \quad . \quad . \quad . \quad (9).$$

since, from equation (8), for pressure, we have

$$(p_1-p_2)\frac{Rr}{R-r}=\frac{2\delta f}{t}\frac{v_2^2}{2g}r^2$$

Dividing by p_2, we obtain

$$\frac{P}{p_2}=\left(\frac{p_1}{p_2}-1\right)\frac{R}{R+r}+1 \quad . \quad . \quad (10).$$

in which R=$b+r$. See Fig. 5.

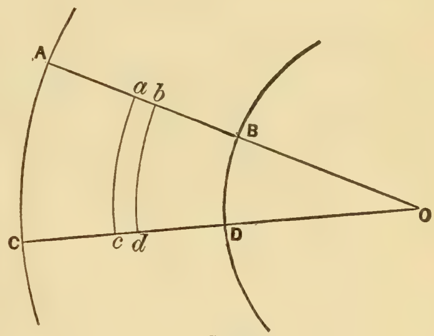


Fig. 5.

Or, if the relation of the mean absolute lifting pressure P, due to the creeping fluid, is desired in its relation to p_1, the last equation is readily modified to that end, viz.:

$$\frac{P}{p_1}=\left(1-\frac{p_2}{p_1}\right)\frac{R}{R+r}+\frac{p_2}{p_1} \quad . \quad . \quad (11).$$

These equations are for an inelastic fluid like water. For elastic fluids, even for the isothermal condition, the analyses are too complex to be introduced here for the circular valve. But for rectangular valves of the form of the ordinary slide valve, the solution is already given above in connection with Figs. 1 and 2. See tables B and C, and eq. (1).

But in comparing tables A, B and C, we observe that the mean lifting absolute pressure P, divided by the absolute pressure p_1 upon the top of the valve, gives a ratio which is nearly the same in the three tables, for any one pressure p_1. Or more briefly, it seems to make but

little difference whether the fluid, pressing upon the valve and creeping along under it, is compressible like air or steam, or whether it is more nearly incompressible, like water. Hence, according to equation (1), it appears that the equilibrium line is nearly at the same position, whatever the fluid, though for high pressures the differences in position are greatest. This fact enables us to make use of equation (11), and the theoretical position of the equilibrium line determined thereby, for incompressible fluids in comparing our theoretical results with the results of experiment in table D.

The comparison thus made will enable us to judge of the value of tables A, B, and C, for use in calculating the position of the equilibrium line in ordinary cases of the slide valve.

For our circular valve it is plain that equation (1) will not answer. But regarding Fig. 2 as a section of a circular valve, such as shown in Fig. 3, then to find what part of the top surface of valve under the pressure p_1 will be balanced by the P upon the surface A C, we have

$$P(\pi R^2-\pi r^2)=p_1(\pi R^2-\pi x^2),$$

the first member being the total lifting pressure due to the creeping fluid between the valve and seat, and the second member being the top pressure p_1 multiplied by such a part of the top surface taken uniformly around the outer edge of valve as shall just counteract that lifting pressure.

In the last equation, x is the radius of the circle of the equilibrium line. Hence, the diameter of that circle is twice the value of x from that equation, or,

$$d=2x=2R\sqrt{1-\frac{P}{p_1}\left(1-\frac{r^2}{R^2}\right)} \quad . (12).$$

Here d stands for the same as given in the last column of the table D. Combining (12) and (11), we obtain

$$d=2r\sqrt{\frac{R}{r}-\frac{p_2}{p_1}\left(\frac{R}{r}-1\right)} \quad . \quad (13).$$

for the diameter of the equilibrium circle for circular valves, as found by theory.

Now, values of d calculated by (13) should agree with the experimental values of the last column of table D, except for the differences due to the fact that the theoretical values are for inelastic fluids, while the experimental values were obtained by experiments with steam, differences which have already been shown to be small.

A comparison of the theoretical values from (13) with the experimental values of the last column of table D will therefore be of interest. This comparison is given in

TABLE E.—COMPARING THEORETICAL AND EXPERIMENTAL DIAMETERS OF THE EQUILIBRIUM LINE FOR R = 3″ AND r = 1 05″.

Appt. pressure per sq. in. on valve. p_1-p_2	Ratio of pressures. $\frac{p_1}{p_2}$	Lifting and seating pressures. $\frac{P}{p_2}$	Diam. of experimental. d.	Equilib. area, calculated d.
5	1.33	.936	2.6	2.53
10	1.67	.897	2.7	2.85
15	2.	.871	2.8	2.92
20	2.33	.852	2.8	3.02
25	2.67	.838	2.9	3.09
30	3.	.827	2.9	3.14
35	3.33	.819	2.9	3.19
40	3.67	.812	3.0	3.22
45	4.	.806	3.0	3.25
50	4.33	.801	3.1	3.27
55	4.67	.797	3.2	3.29
60	5.	.793	3.3	3.31
65	5.33	.790	3.4	3.33
70	5.67	.787	3.5	3.34
75	6.	.785	3.7	3.35
∞	∞	.741	—	3.56

Probably 10 to 15 per cent. lower for elastic fluids.

Though the last two columns are, for practical purposes, identical, yet it is to be remembered that perhaps a considerable difference is due to the fact of elastic and inelastic fluids as above mentioned.

To obtain some insight to the differences of calculated values for elastic and inelastic fluids, compare Table A for inelastic fluids with Table C for elastic fluids, flowing isothermally; C being chosen in preference to B, because the differences are greater, but ranging here, however, mostly within 15 per cent. for values of $P \div p_1$. Though the law of velocity of the creeping fluid in circular valves differs from that in rectangular ones, giving cause for a difference in $P \div p_1$ for elastic and inelastic fluids, which will not be exactly the same for rectangular as for circular valves; yet it is probable that these ratios of pressures will not be very far removed from each other. Assuming these differences iden-

tical for all forms of valve, and allowing the 15 per cent. range above mentioned, then, putting in eq. (12) valves for $P \div p_1$ differing by 15 per cent. from those in Table E, we find that the corresponding calculated diameters of equilibrium areas are less by about 20 per cent. than those given in the last column of Table E. This throws the calculated values all to the other side of, or makes them smaller than, the experimental diameters of the equilibrium areas of the table, showing that for an elastic fluid these experimental diameters are high by about 10 per cent. This may perhaps be accounted for by the fact that during the experiments, when the chest was frequently opened to examine the valve, the seating surfaces were found to have upon them slight traces of a fine gray deposit, and this appeared to be most dense at and near the outer edge of the seating surfaces. The tendency of this would evidently be to keep the valve slightly elevated, and to hinder the free entrance of the creeping fluid into the space under the valve, and to favor its passage and escape after entrance, thus causing a greater effective pressure of valve upon seat and enlarging the experimental equilibrium areas to the figures observed, and to values higher than those calculated on the supposition of a purely elastic fluid, flowing isothermally.

From these considerations it appears that the theoretical deductions are to be relied upon, and that the formulas resulting may be adopted in practice without hesitation. Formulas for elastic fluids and circular valves have not been worked out, but sufficient effort for them has been made to ascertain that such formulas would be neither elegant nor convenient for use. But fortunately, such formulas are mostly needed for rectangular valves with wide flanges on only two sides, as in slide valves for steam engines, for which case tables A, B and C will serve for all fluids and all requirements.

Practical formulas for calculating the equilibrium areas of ordinary rectangular slide valves of steam engines.

Let Fig. 6 represent a longitudinal section through one end of a double slide valve, with L the entire length over flanges, l the width of cavity, and q the width of the equilibrium area.

It is evident that l must include cavities in the seat as well as in the valve, and perhaps bridges between The side bearings of the valve, at the top and bottom of the cavity, are regarded as so narrow that they may be neglected.

Then the upward pressure due to P against the entire under surface of the valve is

$$P(L-l),$$

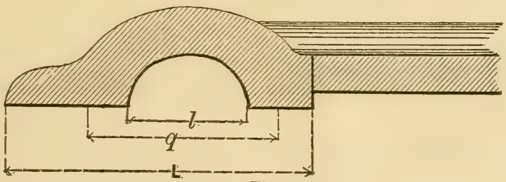


Fig. 6.

and the part of the pressure upon the top of the valve which this equilibriates, is

$$p_1(L-q),$$

which quantities are equal to each other. Equating and solving for q, we obtain

$$q=L\left\{1-\frac{P}{p_1}\left(1-\frac{l}{L}\right)\right\} \quad . \quad . \quad (14).$$

as the breadth outside of which the pressure p_1 is neutralized, and by which we are to multiply p_1 to obtain the effective pressure (absolute) upon the valve.

The total *effective* pressure of the entire half valve of Fig. 5 against its seat is therefore

$$Bq(p_1-p_2)=B(p_1-p_2)L\left\{1-\frac{P}{p_1}\left(1-\frac{l}{L}\right)\right\} \quad . \quad . \quad . \quad . \quad (15).$$

where B is the entire breadth of the valve.

As an example in application of (15), suppose a half valve be $L=10''$, $B=12'$

$l=4'$, $p_1=90$ lbs. per sq. in. absolute, and $p_2=15$ lbs. per sq. in., or one atmosphere. Then Table A for rectangular valves gives $P \div p_1 = .583$, and the total effective pressure is, by (15),

$$12.\times 75.\times 10\times[1-.583(1-.4)]$$
$$=5850 \text{ lbs.} \quad . \quad . \quad . \quad (16).$$

while the total pressure upon the entire half valve is

$$12\times 75\times 10=9000 \text{ lbs.},$$

which latter, it appears, will never be exerted for pressing the seating surfaces together.

The resistance to sliding for the half valve will be

$$5850f$$

where f is the coefficient of friction, the value of which no attempt is made to elucidate in this paper.

The result (16), would evidently have been the same had the cavity been placed at the middle in Fig. 6, as in ordinary slide valves.

For circular valves find d from Eq. (13), and Table E. Then the total effective pressure of valve against its seating surfaces is

$$\frac{\pi d^2}{4}(p_1-p_2) \quad . \quad . \quad (17).$$

Conical valve seats, such as for wing valves of pumps, may be treated as circular surfaces where the slant height of the cone is R, &c., and d in Eq. 13, may be found. The cone is then to be developed, and such part of $\frac{1}{4}\pi d^2$ used as the developed cone is of 360°.

Where valves have an extraneous pressure applied, as in case of springs to assist in holding the valve upon its seat, it is evident that this pressure is to be added to that above obtained for effective pressure. Such spring pressure will in effect figure the same as the presure on top of the valve over the cavity.

It may be remarked that the ratio of pressures $P \div p_1$ is for small lifts of valve independent of the thickness of the space between valve and seat. From this fact we are to conclude that the same force which starts the valve upward from its seat in testing it, is to be continued until the space becomes so thick, that the accelerative force required to give the escaping fluid its motion, becomes so great as to absorb an appreciable portion of the total head causing flow. The acceleration to be accounted for in this problem is that at entry to the space, and also along the space to the exit. The accelerative force along the space between valve and seat would be *nil* for rectangular valves and inelastic fluids, but not for others. These points are important to a rational theory of perfect "pop" safety valves.

It is advisable to here observe that the experimental results given in this paper serve not only to confirm the theory of the equilibrium line, &c., for valves, but that the theory of packing rings, as given in the paper on *A Rational System of Piston Packing*, above cited, based upon the same principles, also receives confirmation in the same degree.

INDUCED CURRENTS IN A MAGNETIC FIELD.

By LIEUT. J. B. MURDOCK, U. S. Navy.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE great development of modern electricity can be directly traced to the discovery by Faraday in 1831, of the principle of electro-magnetic induction. From his discovery, that the movement of a conductor near a fixed magnet, or a closed circuit through which a current was flowing, caused a current to flow though the conductor moved, has arisen the dynamo machine with all its applications. The principles, therefore, underlying the observed facts are of great commercial importance. Since the days of Faraday they have been carefully studied, and from the results of these investigations many useful, practical relations can be drawn.

In order to obtain the mathematical relations in a practical form several definitions and conventions have to be established, and it will be advisable to consider some of these. The region,

whether near a magnet or not, throughout which magnetic effects are produced is called a *magnetic field.* Thus every current causes a magnetic field, as is evident from the deflection of a magnet near it. A unit magnetic pole is a magnet pole of such a strength that when placed one centimeter from an equal and similar pole, it would repel it with the force of one dyne. A magnetic field is of unit intensity when a unit pole placed in it is acted upon with a force of one degree. By comparing the last two definitions, unit pole evidently produces unit field at unit distance. The same intensity would evidently exist at a greater distance from a more powerful pole, as at two centimeters from a pole of strength four, or as three centimeters from a pole of strength nine. When a numerical value is given to the strength of a field, the evident meaning is that a unit pole would be acted on by a force of that number of dynes. As an intermediate step in comparing the magnetic effects of magnet poles and voltaic currents, the conception of a *magnetic shell* is useful. This is supposed to be a thin disc of any shape, having its opposite side of opposite magnetic polarity.

In order to investigate the action in a magnetic field, it is sufficient in any case to know the direction and intensity of the force acting on unit pole at all points in the field, and any scheme delineating graphically, or otherwise, the force and direction of its application at all points, will be a true representation of the field. One of the earliest and best methods is that due to Faraday, of sprinkling iron filings on a sheet of paper over a magnet. The experiment is common, and the curved lines in which the filings arrange themselves between the poles are well known. Made in this way, the resulting curves are not strictly accurate, as they are all in one plane, while the field extends in all directions around the magnet, but they illustrate clearly how a field may be considered to be mapped out, and are of assistance in comprehending other suppositions. These curves evidently show the direction of the magnetic forces, and are therefore called lines of magnetic induction, or more commonly *lines of force.* The force on a unit pole is the greater the nearer it is to the magnet, and in this experiment the lines of force are more numerous near the magnets, leading immediately to the conception that the closeness of the lines to each other may be a measure of the force, and consequently of the intensity of the field at any point. Maxwell has shown that if in any part of the field the number of lines of force passing through unit area is proportional to the magnetic force at that point, the number passing through unit area in any other part will correctly represent the magnetic force there. This conception leads to a new definition of intensity of field, which is that the intensity of field at any point is the number of lines of force passing through a square centimeter perpendicular to the lines at that point. In a unit field, therefore, one line of force passes through each square centimeter of a plane perpendicular to the lines of force. If the lines of force are parallel as are those of the earth's magnetism in any small area, the field is said to be uniform.

A line of force being the direction in which the magnetic forces act, is shown at any point by the direction taken by a suspended magnet. It is impossible to imagine two lines of force crossing at a point in space, as two conflicting forces at that point would unite to form a resultant force, and the direction of this would be the line of force passing through the points. Every magnet consists of two poles, which in any field are acted upon by forces in different directions, but being inseparable the resultant motion of the magnet is due to the difference of the forces. It is easy to see, however, from any observed motion how a single north pole would move if it were perfectly free, and this gives the positive direction of the lines of force, as that in which a free north pole would move. This, in the field, is of course from north to south polarity. It is of the utmost importance to bear this convention clearly in mind. As the lines of force indicate the direction of the force, there can be no force exerted in a direction at right angles to them. Surfaces everywhere normal to the lines of force are called equipotential surfaces, and the relation between the lines and the surfaces enables one to be derived from the other.

It may be well to investigate one or two special cases illustrating the conventions already adopted. In a uniform

field, as the lines of force are parallel, the equipotential surfaces are planes. The direction of the lines is in the earth's field indicated by the dipping needle, and the positive direction is downward in the northern hemisphere. In the case of a free pole the lines of force are radial from the point and the equipotential surfaces are spheres. If the pole is of unit strength there must be one line of force through every square centimeter of a sphere of unit radius, making 4π lines of force in all. The number for a pole of strength m would similarly be $4\pi m$, and the intensity of the field at any point would be shown by the number of these lines passing through unit area of an equipotential surface containing the point. The positive direction of the lines of force is from the pole if it is north, towards it if it is south.

If a magnet pole be moved in a magnetic field work must be done. This work may consist of work done in moving the pole against a repelling force, or in preventing its movement by an attractive force. As these are directly different the former is called positive and the latter negative work. If now, after positive work has been done on the pole, as for instance in moving a north pole towards another, it be left to itself it will tend to move away. It has evidently acquired potential energy, and as this is due to the work previously done on it, this work becomes a measure of the potential energy gained. The term *potential* is used to express this relation, and is measured at any point by the potential energy possessed by a unit pole at that point. The definition commonly given for magnetic potential is that it is measured by the work required to move a unit pole from an infinite distance to that point. The work measured in ergs is numerically equal to the potential measured in units of potential. If a pole of strength two were brought up from infinity, twice the work would have to be done on it, but the potential at the point would be the same as at first. The potential is therefore equal to

$$\frac{\text{Work done in moving pole.}}{\text{Strength of pole.}}$$

Work and potential are evidently different, but are frequently confused. In every magnetic field there is a distribution of magnetic potential, the potential at a point near the magnet pole being greater than at a point more remote, and greater as the strength of the pole producing the field is greater. The potential at a point due to a pole of strength m can be proved to be $\frac{m}{r}$, r being the distance from m. All points equally distant would therefore be at the same potential, or the equipotential surfaces would in this case be spheres as already stated.

As potential is measured by work done, there can be no work done in a surface normal to the direction of the forces in the field, and consequently the equipotential surfaces would in all cases be perpendicular to the lines of force, as in any other direction some component of the force would act and work would be done.

Gauss showed that the potential due to a magnetic shell at any point would be ωi where ω is the solid angle subtended at the point by the shell, and i is the strength of the shell. At all points on an equipotential surface the value of the solid angle must be the same. The measurement of the angle becomes extremely difficult at points situated obliquely to the axis normal to the shell at its center, but it can be easily seen that as the point becomes oblique, it must come nearer to the shell. In the plane of the shell itself the solid angle, and consequently the potential are everywhere zero. The general shape of the surfaces is therefore that of deep bowls, concave towards the shell, and most remote from it on its axis. The lines of force start normal to the shell on its north side, and curve round, cutting successive equipotential surfaces at right angles to the south face, into which they fall perpendicularly.

If a pole of strength m be brought up to a point near a shell, the amount of work done will, as already stated, be m times that done in bringing a unit pole up to the same point, or m times the potential due to the shell at that point. The expression $\pm m\omega i$ is therefore a measure of the work done, being positive if it required work to be done to move the pole, negative if work had to be done to prevent its movement still farther. As we have seen the number of lines of force of a pole of

strength m is $4\pi m$. This number radiate through a solid angle 4π, which is the solid angle at the center of a sphere. Through any solid angle ω, the number of lines would therefore be $m\omega$, and representing this number, cutting the shell by N_1, the work done is expressed by $N_1 i$. Similarly, if the pole had been brought to a point at which the potential due to the shell was $\omega_1 i$, the work done would have been $m\omega_1 i$, or $N_2 i$. Potential being measured by work done, any change of potential can be obtained only from work. If, therefore, the pole be moved from one point to the other, the work done on unit pole is measured by the differences of the potentials, or,

$$v_1 - v_2 = \pm i(\omega - \omega_1)$$

If the pole m is moved, the work done is

$$m(v_1 - v_2) = \pm i(N_1 - N_2)$$

Any relation traced for a magnet shell is capable of application to a closed voltaic circuit. The lines of force of a current encircle it concentrically, as is shown by the tendency of a magnet pole to move at right-angles to the direction of the wire. If a wire carrying a current be bent into a closed curve, the lines of force in passing round the wire would combine their effects on a magnet pole placed near, so that the resultant line of force would be normal to the plane of the circuit; or, in other words, the magnetic effect of a closed circuit is the same as that of a magnetic shell. By adopting a particular ratio of units the magnetic effect of any voltaic circuit may be made equal to that of a magnetic shell of a given strength whose edges coincide in position with the circuit. The electromagnetic unit of current fulfills this relation, being that current, unit length of which acts with a force of one dyne on unit pole at unit distance. This unit of current, therefore, causes unit field at unit distance, as by definition does also a shell of unit strength. By measuring our current, therefore, in these units, the relations traced for the field due to a shell can be applied to that produced by a voltaic circuit.

The potential due to a voltaic circuit through which a current of strength C is flowing is $-C\omega$, the negative sign being due to the fact that a positive current causes a negative potential. The positive direction of a current is that in which the hands of a watch move, but if the north pole of a magnet be brought near a circuit in which the current is flowing in this direction, it will be *attracted*, or negative work is done in moving a free north pole up to such a circuit. The potential is therefore negative, and, as in the same way experiments show that a current in the negative direction causes a positive potential, the potential due to a voltaic circuit must always have the opposite sign from that of the current. The equipotential surfaces and lines of force are similar in character and number to those of the equivalent magnet shell. The direction of the lines of force is important, and can easily be determined by the simple experiments referred to. A positive current in a circle attracts a north pole, and the positive direction is therefore towards that face on which the current appears to flow in the positive direction. Viewed from the other side of the circuit the current would appear to be negative, and on that side a north pole would be repelled. These relations are summed up by saying that the directions of the current and the lines of force are related in the right-handed cyclical order. A useful comparison is that commonly given by the use of a corkscrew. If the wrist be rotated to the right, the point of the screw advances. Letting the rotation of the wrist correspond to the movement of the current, the motion of the point will be that in which a north pole moves, or the positive direction of the lines of force.

As the relations proved for a magnetic shell hold in the case of a closed voltaic circuit, the work done in moving a pole m near a closed circuit in which a current of strength C was moving is $-C(N_1 - N_2)$, N_1 and N_2 representing as before the number of lines of force of the pole which are enclosed by the circuit. If the pole were fixed and the circuit moved the same work would have to be done, as the same forces of attraction and repulsion would have to be overcome through the same distance. This expression becomes, therefore, the formula for the work done in moving a closed voltaic circuit in a magnetic field. If N_1 is larger than N_2 the work done is negative,

or the circuit tends to place itself in a position in which it encloses the maximum number of lines of force. If the direction of the lines of force of the field is opposite to those of the circuit, the latter will, if free to move, first rotate so as to bring its own lines of force to correspond in direction to those of the field, and will then move in such a direction as to enclose the maximum number possible. It is seen in this connection that a maximum of negative lines is practically the same as a minimum of positive. The number of lines enclosed by the circuit depends on the inclination of its plane to the direction of the lines, varying in the ratio of the sine of the angle the lines make with the plane of the circuit. If this angle is called θ, the circuit tends to turn into a position in which $\theta=90°$, and the work it would do in turning, or the potential energy it possessed before movement, is measured by the expression $C(N_2-N_2 \sin. \theta)$. This principle is evidently of importance in its relation to electro-motors, but we must continue on another line of investigation.

If a closed circuit be placed in a magnetic field it will exhibit no tendency to move if no current be passed through it. If a current pass the wrong way, the circuit will, as we have seen, move to make the number of lines of force enclosed a maximum. In doing so it performs work, and this work must be derivable from the energy of the current. Part of this energy is dissipated as heat, and part expended in moving the coil. We have, therefore, t, being the time occupied in the movement,

$$CEt=C^2Rt+C(N^1-N_2) \quad . \quad . \quad (1)$$

or
$$CR=E-\frac{(N_1-N_2)}{t}.$$

But CR is the E. M. F. remaining in the circuit, and is less than the original E. M. F. by the quantity $\frac{N_1-N_2}{t}$ which must also be of the nature of an E. M. F., but acting in opposition to that already existing. This expression is, therefore, the measure of E. M. F., induced in the circuit by its motion. (N_1-N_2) is the change in the number of lines enclosed by the circuit at the beginning and end of the time t. Hence we see that the E. M. F. induced in a closed circuit moving in a magnetic field, is measured by the *rate of change* of the number of lines of force inclosed, and is opposite in direction to that of the current which causes the motion. This result follows directly from the principle of conservation of energy, for if a current flowing in a given direction caused motion of the circuit, and this motion induced a current in the same direction as the original, it would increase the motion, and hence the energy of the system. Lenz' law, which says that if a current is induced in a magnetic field, it is in such a direction as to oppose by its electro-magnetic action, the cause inducing it is, therefore, directly deducible from the principle of conservation of energy.

We have thus far considered the case in which a current is flowing in the circuit at first, but the same rules apply in cases where there is no initial current. The formula $CR=E-\frac{(N_1-N_2)}{t}$, being true for all values of E, must hold when $E=O$, but in that case CR is the E. M. F. in circuit $=-\frac{(N_1-N_2)}{t}$ as before. This appears also from the fact that by diminishing t, by increasing the velocity of the circuit, the induced E. M. F. may be made to equal or overcome that previously existing, and there would eventually be a current flowing in the same direction, whether a current existed in the circuit at first or not. On the supposition that $E=O$ (1), becomes

$$O=C^2Rt+C(N_1-N_2) \quad . \quad . \quad (2)$$

or
$$CR=-\frac{(N_1-N_2)}{t}, \text{ as above.}$$

As in this equation the original energy was zero, the energy of the induced current can be derived only from the work done in moving the coil, and if no external work is done by the coil, the energy thus applied appears as heat in the circuits.

The E. M. F. induced in the time t, being numerically equal to the change in the number of lines of force inclosed and acting by Lenz' law in such a direction as by its electro-magnetic action to oppose the cause inducing it, must tend to cause a current flowing in such a manner as to keep the number of lines inclosed by the circuit constant. If a coil be

moved so as to inclose a smaller number, the induced current must add enough to make the loss good; if moved so as to inclose a greater number, the induced current would cause enough to pass in the opposite direction to neutralize the gain. Consider the case of a coil moved along the lines of force, the observer looking along them in the positive direction. If, in the movement, the coil incloses a decreasing number, the induced current must make good the deficiency, and this by the rule already given for the relation between the direction of the lines of force and that of the current would be done by a right-handed or direct current, one flowing in the direction in which the hands of a watch move. If the coil moved inclosing an increasing number, the gain would be neutralized by the lines due to a left-handed or inverse current. Hence the rule given by Silvanus Thomson in his "Elementary Lessons"—"a decrease in the number of lines of force which pass through a circuit produces a current around the circuit in a positive direction, while an increase in the number of lines of force produces a current in the negative direction around the circuit." This rule is general, and is most useful, but in its application it must be remembered that the currents appear positive or negative to an observer looking along the lines of force in the direction in which a free north pole would move. Viewed from the other side they would of course appear to pass in the opposite direction. These relations, although somewhat difficult to reason out, are easily verified by experiment. It may be well to illustrate the application of the rules given. Suppose the lines of force to pass down perpendicularly through the paper, and a coil to be at rest in the plane of the paper. It encloses a maximum number of lines of force, and if rotated so that the right-hand edge comes to the front, while the left-hand goes behind the paper it will inclose a constantly decreasing number of lines, and a positive current will be induced. The E. M. F. will at first be small, as the rate of change is small, the edges of the coil moving almost along the lines of force. The rate will gradually increase until the coil has moved through one quadrant and is edge on to the observer, when, as the motion of the edges is at right-angles to the lines, the rate, and consequently the E. M. F. is a maximum. In this position the coil incloses no lines of force, and during the second quadrant it will move, inclosing an increasing number, and inducing, therefore, an inverse current. But the side of the coil now seen is the opposite to that in view during the first quadrant, and the inverse current is therefore in the same absolute direction in the coil as the former direct current. During the second quadrant the rate and E. M. F. decrease, becoming a minimum when the coil has completed a half revolution and is again in the plane of the paper. On entering the third quadrant, the number of lines inclosed decreases, and a direct current is induced; but as the same side of the coil is presented to the observer as in the second, the direction of the current is reversed in the coil. In the fourth quadrant the number of inclosed lines increases, but the other side of the coil is towards the observer, so that the absolute direction of the current is the same as in the third. The general direction of the current is therefore downward in that part of the coil in front of the the paper, and upward in the outer half; but as regards the coil itself, the direction of the current changes twice in every revolution, the point of change being where the circuit incloses the maximum number of lines of force. By the use of a commutator which shifts its connections at this point of the revolution, the current may be made to flow in one direction in the exterior circuit.

It now remains to trace the practical application of these results. In the formula for the induced E. M. F., let N denote $(N_1 - N_2)$, or be the change in the time t in the number of lines of force inclosed by the circuit. It is extremely difficult to obtain numerical values for N_1 and N_2, but it requires very little experimenting with magnets and iron filings to trace the general direction of the lines of force in any combination of magnets. All treatises on electricity or physics contain diagrams illustrating the distribution of the lines of force in different cases, and as the formula does not require any absolute value, but only a change or difference of values, this can be inferred with considerable exactness from an examina-

tion of the arrangement of the magnets in the field. From the formula, therefore, $E = \frac{N}{t}$ the following considerations are easily deduced:

1. The E. M. F. varies as the change in the strength of the field. If the coil passes from a position in which it incloses no lines, to another in which it incloses N lines, the latter should be as great as possible. If it moves alternately in a positive and in a negative field, the algebraic difference should be as great as possible. This relation is commonly given that the E. M. F. varies as the strength of field, but it is possible to move a coil in the most intense uniform field without inducing any current.

2. The E. M. F. varies as the velocity. If N lines are cut in a time t, the shorter t the greater the E. M. F. Theoretically, therefore, the E. M. F. of a dynamo varies as the speed, and this relation is so nearly true in practice as to be always assumed in calculation.

3. The formula gives the E. M. F. induced in a single circuit. In a coil of n turns each incloses the same number of lines of force, and each would therefore induce the same E. M. F. But the turns being in series the total E. M. F. in the coil would be n times that in one turn. If each of the n turns were moved in the field separately, n times the work required to move one turn would be done, and this amount of work must also be done when the whole coil is moved at once. The E. M. F. varies, therefore, with the number of turns in the moving coil, but as the outer turns inclose more lines than the inner, the ratio of gain would be slightly greater than the number of turns.

4. In a coil rotating with uniform angular velocity, the rate of change of the lines of force inclosed would be greatest where the coil cuts them at right-angles, or where the plane of the coil is parallel to the lines of force.

5. The larger the coil the greater the number of lines passing through it, and other conditions remaining the same, the E. M. F. varies as the area of the coil. A difficulty exists in making the armature larger and keeping the intensity of the field constant. Pole pieces are frequently used to accomplish this end, but may work disadvantageously.

Perfect working in a dynamo requires a constant change of E. M. F., and consequently a constant rate. If a large coil should revolve between the poles of two bar magnets, and no iron were present to modify the distribution of the lines of force in the field, the greater part would pass directly between the poles, and would be cut during a small part of the revolution of the coil, during which time the rate and induced E. M. F. would be high, but in other parts of the revolution the rate would be very small. The available E. M. F. would be induced suddenly, but the sudden creation of a current causes high self-induction and temporary strong extra currents, which in a dynamo are not only prejudicial but dangerous, on account of the high E. M. F. they may have. Idle wire in the armature also reduces the current. It is therefore desirable to prevent a concentration of the lines of force in a small part of the field. In a coil rotating in a uniform field, the advantage of the constant rate is attained by the change of the number of lines inclosed in the ratio of the sine of the angle between the plane of the coil and the direction of the lines, and as a field tends to become uniform would this advantage be gained. Pole pieces, by increasing the magnetic surface, operate favorably, but if they encircle the armature so closely as to approach each other nearly, lines of force will pass from one to the other which are not cut at all by the coil in its revolution. Experiments have shown that in some cases the E. M. F. is increased by cutting off the edges of the pole pieces. Edison has apparently carried this advantage to the utmost, the long magnets and immense pole pieces of his machines tending to secure a field more nearly uniform than in other types.

In an improved bichromate of potash battery M. Luigi Ponci uses a liquid thus made: One kilogramme of bichromate is crushed and dissolved in 4 litres of boiling water, and to this 2 litres of chlorhydric acid is added. A liquid is thus obtained containing chloride of potassium and bichromate of potash, which prevents the formation of crystals in the battery.

THE MATTER OF SPACE.

From "Nature."

Of late years there has been a growing tendency towards the belief that matter is present everywhere throughout the universe, as well in interstitial space as in the bodies of the spheres. Yet an older hypothesis is still widely held. The phenomena of light seem to require some substantial medium in space, but this substance has been viewed as specifically distinct from matter, and named ether. Another class of thinkers has devised still another species of substance. This is required to meet the demands of the new gravitation hypothesis, and consists of excessively minute particles, moving with intense speed, and pressing vigorously on the larger and slower particles of matter. In the past, still other species of substance were imagined; heat, electricity, etc., were each ascribed to a specifically distinct substance.

Now, however, the tide has turned, and the inclination is to believe in only a single form of substance. There are, of course, countless distinct conditions produced by the aggregations of substance, and variations from simplicity to complexity, but this may not necessarily require more than a single kind of basic particle, or whatever we may call it. If the substantial contents of space are similar in constitution to the matter of the spheres, their state of existence must be much more simplified. In the spheres we have matter ranging from the simple elementary gasses of the atmosphere, through the complex mineral compounds of the solid surface, to the highly compounded organic molecules. In outer space the variation is probably in the opposite direction, and substance may exist there in a condition much more highly disintegrated than the atmospheric gases. This view is not held by all theorists. Dr. Siemens argues that space holds molecules of considerable intricacy, comprising certain terrestrial elements, and their simpler compounds; as to the contents of space we know that there are very numerous solid masses, some of considerable size, others minute, and possibly ranging through many degrees from the largest to the minutest. Yet these really occupy but an inconsiderable portion of space, and apparently originated in solar or planetary orbs.

Such is, briefly stated, the state of knowledge and of hypothesis concerning the substantial contents of space. We need but add the uncertain reasons for arguing the presence of a resisting medium in space, and the necessity of a highly elastic condition of the light-conducting substance, to exhaust the subject so far as yet pursued.

It is held by some that the gravitation energy of the suns and planets is sufficiently great to sweep space of all contiguous material particles, except those solid masses which are saved from this fate by the vigor of their orbital motions. The atmospheres of suns and planets are retained with an energy very greatly in excess of their reverse energy of molecular motion, and therefore it is quite impossible that any of this material should escape into space, or that any similarly-conditioned material should exist contiguous to the spheres, without being forced to become atmospheric matter. The centrifugal energy of the earth's atmosphere at the equator is only $\frac{1}{17}$ of that necessary to overcome gravity. The molecules of the atmosphere have also a vigor of heat vibration about equal to their centrifugal energy. Hence the resisting energy of these molecules is far below the gravitative energy, and they are vigorously held.

The question of the possible existence of gravitating matter in interspheral space depends strictly upon that of its motor energy. If the momentum of any particle, or of the whole sum of particles, be insufficient to constitute a centrifugal energy equal or superior to the centripetal energy of gravitation, then the material contents of space must inevitably be drawn into the attracting spheres, as atmoshperic substance, and space be denuded of matter. If, on the contrary, the centrifugal energy of these particles be sufficient to resist gravitation, they will remain free, and space continue peopled by matter.

Such gravitative particles, wherever

existing in space, could not be for an instant free from the influence of spheral attraction whatever their energy of motion. If this energy be too small, they must be related to the spheres as falling bodies, and must become atmospheric matter. If the two opposite energies be equal, they must be related to the spheres as planetary bodies, and circle in fixed orbits around the center of attraction. If the centrifugal energy be in excess they must assume the condition of independent cometary bodies, temporarily influenced but not permanently controlled by any sun, and wandering eternally through space.

Such are the three possible conditions of the material contents of space. If the first obtain, space must be denuded of matter; if the second obtain, it will permanently contain matter in a partially elastic state; if the third obtain, it will permanently contain matter in a highly elastic state, since the pressure upon each other of the vigorously centrifugal particles must be great and may be extreme. Of course no single particle could long retain its direction of motion, as related to any sphere. Constant impacts must constantly vary the directions of molecular motion. But the motion of each particle is successively transferred to a long series of particles, and thus is virtually continued in force and direction. Each motion pursues its course independently, though not as affecting any fixed particle of matter, and each particle aids in the progression of a vast network of motions, proceeding in every direction throughout the universe. Thus each particle, though not actually changing its place, may have motor relations which extend in every direction to the utmost extremes of space. It is a node in an interminable network of motions, and its incessant leaps throughout the limits of its narrow space are each part of a long motor line, which affects successively myriads of particles. So far as the energy of gravitation is concerned the effect upon this incessantly transferred motion is precisely the same as if the motion was confined to a single particle. If it lack energy the motion will be a falling one; if it equal the gravitative energy it will form a closed orbit. If it exceed the gravitative energy it will form an open curve, and be only temporarily controlled by any sphere.

In this interchange of motor energy certain particles may continually decrease in vigor of motion, and if near solar orbs may be drawn in as atmospheric matter. But they can only lose motion by transferring it to others, which would in consequence become more independent of gravity. The sum of motor energies in the universe must persist unchanged, and the aggregation of atmospheric substance around any planet must cause an outflow of motor energy which will increase the motor vigor of exterior particles. In such a case the height of atmosphere in any sphere will depend partly on the attractive vigor of the sphere, and partly on the average motor vigor of the whole sum of matter. Every contraction and loss of motor energy by any portion of matter will increase the motor energy of remaining matter, and a fixed limit to the atmospheric control of every sphere must result, since in the outer layers of its atmosphere the centrifugal energy of molecular motion must increase until it equals the energy of gravitation.

Can we arrive at any conclusion as to which of the three possible conditions above considered really exists? If so we can answer the question as to the existence of matter as a constant tenant of space, and also reach some conclusions as to the character of its motor conditions.

There is one line of thought which seems to lead to a settlement of this question. If the nebular hypothesis of the formation of solar systems be accepted as true, either wholly or partly, there can be no doubt as to the interspheral status of matter. The conditions of nebular aggregation indisputably settle it.

This hypothesis holds that the matter now concentrated into suns and planets was once more widely disseminated, so that the substance of each sphere occupied a very considerable extent of space. It even declares that the matter of the solar system was a nebulous cloud, extending far beyond the present limits of that system. From this original condition the existing condition of the spheres has arisen, through a continued concentration of matter. But this concentration

was constantly opposed by the heat energy of the particles, or, in other words, by their centrifugal momentum. This momentum could be only got rid of by a redistribution of motor energy. If, for illustration, the average momentum of the particles of the nebula was just equivalent to their gravitative energy, then a portion of this energy must radiate or be conducted outwards ere the internal particles could be held prisoners by gravitation. The loss of momentum inwardly must be corelated with an increase of momentum outwardly.

This is a necessary consequence of the heat relations of matter. As substance condenses its capacity for heat decreases, and its temperature rises, hence a difference of temperature must constantly have arisen between the denser and the rarer portions of the nebulous mass, and equality of temperature could be restored only by heat radiation. This radiation still continues, and must continue until condensation ceases, and the temperatures of the spheres and space become equalized, but this is equivalent to declaring that as the particles of the spheres decrease in heat momentum those of interspheral space increase, and if originally the centrifugal and centripetal energies of matter approached equality, they must become unequal, centripetal energy becoming in excess in spheral matter, centrifugal energy in the matter of space. Thus, as a portion of the originally widely distributed nebulous matter lost its heat, and became permanently fixed in place by gravitative attraction, another portion gained heat, became still more independent of gravity, and assumed a state of greater nebulous diffusion than originally. The condensing spheres only denuded space of a portion of the matter which it formerly held, and left the remainder more thinly distributed than before. The spheres, in their concentration, have emitted, and are emitting, a vast energy of motion. This motor energy yet exists in space as a motion of the particles of matter, which therefore press upon each other, or seek to extend their limits with increasing vigor, so that the elasticity of interspheral matter is constantly increasing.

It might be hastily imagined that such an excess of heat vigor in the matter of space over that of the spheres should declare itself in temperature. But it must be remembered that temperature is no measure of the absolute heat contents of matter.

Condensation increases, rarefaction decreases, temperature with no necessary change in absolute heat contents. The expression "fire mist," so often applied to the matter of uncondensed nebulæ, gives a very erroneous impression. The matter of the solar system nebula, though containing a high degree of absolute heat, was probably of low temperature. Its great rarity must certainly have greatly decreased its temperature. As a differentiation in this matter took place, one portion becoming condensed, another portion more rarefied, the former must have increased, the latter decreased, in temperature. Eventually the extreme condensation of one portion of this matter, and rarefaction of another, caused an extreme difference in temperature. An excessive radiation from the spheres to space has taken place in consequence, the absolute heat of the former constantly decreasing and that of the latter increasing. But the difference in temperature still continues great, the influence producing it acting much more rapidly than the influence tending to obliterate it. Eventually an equality of temperatures may be produced, but only by the production of a very considerable inequality of absolute heat. This must be the final result of spheral condensation and nebulous rarefaction of exterior matter; namely, equalization of temperature, with a change from the original homogeniety to a great heterogeniety of heat contents.

But we are again brought back to the question of the motor energies of matter. Are they sufficiently great to enable a portion of this matter, when reinforced in motor energy by radiations from the spheres, to defy gravitative attraction and remain free in space? Undoubtedly so, and much greater than would be simply requisite for the purpose, since we find the matter of the planets, after their immense losses by radiation, still possessed of a considerable excess of motor energy. The earth, for instance, has an orbital motion sufficient to maintain it at a considerable distance from the sun. But the motion of the earth is but the combined

motion of its molecules. This motion once existed as independent molecular motion, which in time, under the influence of gravity, became dependent molecular motion. We have already spoken of the fact that the particles of space, in consequence of their heat motions, tend to dart off in straight lines of motion, except in so far as the gravitative attraction of spheres causes these lines to become curved. These lines of motion, so far as individual particles are concerned, are checked by the particles coming into contact with others. The motion, however, proceeds onwards, though it is carried by successive, instead of by single particles. If, however, a number of particles move in company in the same direction, they may move much further as individuals, before transferring their energies. And if an immense mass of particles come to thus move in company their individual excursions may be indefinitely extended. The lines of motion, instead of being continued by successive particles, are continued by the same particles, and molecular motion becomes mass motion. The motion of terrestial molecules, in their revolution around the sun, resemble those of the molecules in Prof. Brooks vacuum tubes, constituting his "fourth state of matter."

Now the degree of resistance of such a mass to centripetal energy will indicate the degree of resistance of the original uncombined molecules. In the earth the motion of the molecules, thus combined, yields a centrifugal energy sufficient to maintain the earth at its present distance from the sun. But this is only a portion of its molecular energies. Its molecules possess considerable independent motion, and form nodes in lines of radiation that extend in every direction. They have also lost a great vigor of motion by radiation to space. It follows that the original momentum of these molecules must have constituted a centrifugal vigor greatly in excess of their centripetal vigor. It secondarily follows that the momentum of those molecules of the nebula which still exist in space, augmented as it has been by radiations from the spheres, yields a very energetic excess of centrifugal vigor. Many of the comets have a centrifugal energy in excess of the centripetal energy of the sun, yet this represents only a fraction of the energy of their molecules, and a much smaller fraction of the energy of the material particles of space.

The combination of the centrifugal energies of terrestrial particles is due to the fact of a secondary center of gravity having been formed. The heat velocity of its particles, in excess of that displayed in their revolution around the sun, has become partly a revolution around the earth's axis, and is partly retained as heat vibration. But the heat velocity of the material particles of space is not thus secondarily employed. It is affected by the attraction of the sun, or of the nearest sphere; but evidently, from the considerations above taken, this attraction cannot be sufficient to over-balance the centrifugal energy and cause atmospheric aggregation or even to cause orbital revolution. The particles must have energy sufficient to make them independent of spheral gravity. Their straight lines of motion must become to some degree curved in response to gravity, but cannot become closed curves. Instead of becoming planetary, they remain cometary lines, of very open orbit. For if we imagine the earth to be suddenly restored to its nebulous condition, or its particles to be set free in space, they would possess a velocity of motion much in excess of the earth's orbital velocity. Hence they could not be controlled by the sun. The existing particles of space possess a still much greater velocity, and are therefore much more free from gravitative control.

Certain necessary results of this condition have been considered. The lines of centrifugal motion in space are not confined to single particles as in the earth, but are transferred from particle to particle. The effect, however, is precisely the same; this motion of suceessive particles is in no respect different in effect from what we would have if a single particle were free to move in the same direction. Each particle moves a certain distance, and then transfers its motion in that direction to another. But it immediately pursues some other direction of motion in response to impact, and this aids in the progressive movement of innumerable lines of motor energy. The great centrifugal vigor of these motions must cause an energetic compressing influence upon interspheral matter, and thus produce an

elasticity, sufficient perhaps for the requirements of light radiation.

The lines of motion thus transferred through space cannot be unvarying in their orbital directions. Nature knows no great or small in her processes, and each moving particle of the free matter of space is controlled by the same principles which control the motions of a planet. It is subject to perturbations from lateral attractions, similar to those which draw planets and comets out of their orbits, and completely change the orbit of the latter. And its impacts with other particles yield effects such as would arise in impacts between planets of oppositely moving systems. Action and reaction are equal, in this as in every case. The orbit and the speed of a line of motion may be changed through impact or attractive resistance, but only by its causing an opposite change in some other line. Thus the lines of motor energy referred to are not unvarying in speed and direction, but are unvarying in their sum of correlated speeds and directions. The variations which take place in the orbits of spheres and comets through attractive perturbation, and the greater variations which would take place did spheres come frequently into contact, are precisely similar to those which must occur in the case of interspheral particles, and any change in the direction of one orbit is balanced by an equal opposite change in the direction of another orbit, the balance of motor direction and energy in nature being exactly preserved.

If such a line of motion pursues a cometary ellipse and enters the atmosphere of a globe, it must be affected by friction precisely as if the line of moving particles were a single particle, or a minute comet. It might be obliterated by friction or resistance, as the orbital motion of a falling body is obliterated. But this obliteration is really caused by the opposing energy of opposite lines of molecular motion. The single line of motion may be distributed into a thousand lines differing in direction, but the component of these thousand lines must agree with the original line.

The transfer of motion from particle to particle here indicated may take place through attractive resistance as well as through impact resistance. The original disintegration of the matter of space must have increased, as spheral condensation denuded space of much of its material, and as radiation from the spheres increased its motor energy. If matter thus divided up into smaller and smaller particles, these may have continued as closely contiguous in space as are the molecules of spheral atmospheres. In such a case they may present the conditions of excessive rarity so far as weight of matter is concerned; of close contiguity of particles, sufficient to permit the exercise of attractive energy; of great compression, through their vigor of centrifugal motion, and of intense elastic resistance to compression. These are the conditions necessary for the transfer of the radiations of light and heat. In these radiatures motion is conveyed through space by transfer of vibratory motions, not of impacts. The vibrating particle swings between lateral chains of attraction, and causes a like transverse swing in successive particles with which it is attractively connected. Greater energy here causes only greater width of vibration, not greater rapidity of transfer. The latter depends only on the elasticity of the matter concerned. Impact transfer of motion, on the contrary, must differ in speed with every difference in vigor. It is transferred by the motions of what we know as local heat, similar to the incessantly varied heat motions of gaseous matter. As the particles are unvarying in weight, increased momentum can be gained only by increased rapidity of motion, and the lines of motion thus transferred through space vary in speed with every variation in vigor. Every motion, of every particle of matter, is really a minute portion of an orbit, which represents that of a falling body, of a planet, or of a comet, according to its rapidity. Though the momentum affects successive particles of matter the orbit is continuous, except to the extent that it is varied by perturbations through attraction and impact.

Wherever any influence aids a translation of interspheral matter—causes a wind to blow through space—the lines of motion continue to be conveyed by the same particles. The orbital motions of the spheres are such winds through space; minor aggregations of moving matter may enter the atmosphere of the

sun or other globes. But no atmosphere can become permanently increased in this manner; such masses, checked by friction, must yield motion, which flows outward. The centrifugal energy of the molecules of the external atmosphere is thereby increased, and the gain of matter must be balanced by an equal escape of matter at that critical atmospheric limit where centrifugal and centripetal energies are in balance. But any such fall of interspheral matter must aid the radiant emissions of the sun. Its loss of proper motion, its high degree of absolute heat, its increased temperature through condensation, and its consequent radiation, would make it a source of solar heat. Any such cometary matter must form part of "The Fuel of the Sun."

ON THE MOLECULAR RIGIDITY OF TEMPERED STEEL.

By PROF. D. E. HUGHES.

A Paper read before the Association of Mechanical Engineers.

During the course of some recent researches the writer has been enabled, by the aid of the induction balance, to perceive some remarkable molecular differences between the constitution of iron and of steel. There are numerous papers in the *Comptes Rendus*, from 1830 to 1850, in which it is suggested that tempered steel is a true alloy of iron and carbon, the carbon being present in varying degrees according to the temperature at which the alloy was formed, and being afterwards rendered permanent by sudden cooling. In a late discussion on this subject the writer made a few remarks, in which he pointed out the marked difference between softened and tempered steel, as to solubility in dilute sulphuric acid, and expressed the opinion, formed from these and many previous experiments, that tempered steel was a true alloy. He has since continued these experiments, not, however, to prove the chemical composition of tempered steel, but to investigate its peculiar molecular structure, as indicated by the induction balance. The apparatus necessary to perceive the effects of stress or torsion, as described in this paper, is exceedingly simple. Suppose, for instance, that we take an ordinary single-coil electro-magnet, and join its terminals with that of a telephone or sensitive galvanometer. If we now pass a current from a battery through the iron core alone of the electro-magnet, we have a sharp click at each make and break of the current. This effect was discovered by Page, and fully described by De La Rive. If we keep the current passing constantly through the core, we have no effect; but if we then give a slight torsion or twist to the core, either to the right or left, we at once hear a sharp click; and if we keep the torsion constant, and then make frequent interruptions of the battery, we have a greatly increased sound at each make or break, indicating a greatly increased force of electric current. In order to investigate this phenomenon, the author constructed a special though very simple apparatus. A coil, having a large aperture, is fixed to a board; two small abutments or supports, at a few inches distant on each side of the coil, allow us to suspend or fix an iron wire passing through the aperture, which then becomes the core of an electro-magnet. This forms the essential portion of the apparatus. The iron or copper wire rests upon the two supports, which are 20 centimeters apart; at one of these it is firmly clamped by two binding screws, while the opposite end can turn freely. The wire is 22 centimeters long, projecting 2 centimeters beyond its support. On the projecting end is a key or arm, which serves as a pointer moving on a graduated circle, and gives the degree of torsion which the wire may receive. A binding screw allows us to fasten the wire, after turning the pointer to any degree of torsion, and thus preserves the required stress as long as is necessary. The exterior diameter of the coil is $5\frac{1}{2}$ centimeters, and that of the interior vacant aperture is $3\frac{1}{2}$ centimeters: the width is 2 centimeters. Upon this coil is wound 200 meters of No. 32 silk-covered copper wire. This coil is fastened to a small board, so ar-

ranged that it can be turned through any desired angle in relation to the iron wire which passes through its center; and it can also be moved so as to lie over any portion of the 20 centimeters length of wire, in order that different portions of the same wire may be tested under a similar stress. The whole of this instrument, as far as possible, should be constructed of wood, in order to avoid all disturbing inductive influences of the coil upon other pieces of metal. The iron wire at its rear or fixed end is joined to or makes contact with a copper wire, which returns to the front part of the dial under the board and parallel to the coil, thus forming a loop. The free end of the iron wire is joined to one pole of the battery; the copper wire under the board is joined to a rheotome, and thence to the other pole of the battery. The coil is joined to a telephone or a sensitive galvanometer; and we may either pass the current in the manner described, or may reverse all the communications, passing the current through the coil instead of the wire, and listening with the telephone to the induced currents upon the iron wire alone.

In order fully to understand the phenomena which takes place, we must bear in mind Faraday's discovery of electric magnetic induction—namely, that any wire conveying an electric current induces in general a momentary secondary current in any independent circuit whose wires are parallel to it; the effect being at its maximum when two wires are parallel, diminishing as the angle of these wires is increased, and at 90° being absolutely zero. Consequently, when we place a copper wire in the axis of the coil, with the above apparatus, and pass a current through this wire, we find no effect whatever, no trace of induced currents; simply for the reason that this copper wire crosses all of the wires of the coil at an angle of 90°. We also find that no effect takes place upon torsion being applied to the copper wire. If we now place a small rod of iron parallel with the conducting copper wire, we have no effect; but if the iron rod is turned at an angle to the wires a current is observed, the force increasing from parallelism to an angle of 45°, and decreasing again from this angle to 90°, where we have again no effect. The conducting copper wire thus induces electric magnetism in the iron rod, and this magnetism reacts upon the coil; but this only holds as long as the rod is not parallel to either coil. At an angle of 90°, although at its maximum of electric magnetism, the iron rod becomes parallel to the coil upon which it reacts; consequently we have again a zero of current. In place of one rod, we may insert several short rods; and if these are all turned together in the same direction we have similar effects. Knowing this, we can understand that if each molecule of a rod were endowed with separate magnetic power, and if we could cause these to rotate through any angle round the axis, we might expect similar reactions to those of the small separate iron rods already mentioned. If we replace the copper wire spoken of by an iron wire, and send intermittent currents through it, we still have no induced current upon the coils; but the instant we apply a very slight torsion, say 10 or 20 per cent. of one turn, we at once perceive strong induced currents. These are positive for right-hand torsion and negative for left-hand torsion. Thus we can not only produce induced currents, but, without changing the direction of the primary electric current, we can change the induced currents, making them positive or negative as we please—exactly as would occur if we rotated in opposite directions the small iron bars, placed side by side with the copper wire. At this point it becomes important to know if these effects are produced by the twist given by torsion to the whole mass of the wire, or if each molecule turns separately and independently around its axis. There are many proofs that the latter view is correct; for, assuming the former, then, if an iron wire be twisted permanently by 30 or more entire turns, we should expect greatly increased effects as compared with those given by 10 or 20 per cent. of a single turn. But we find that, after the first instant of torsion, we have no increase of force in the current, even with a molar twist of 30 whole turns, which must, of course, produce a certain molecular twist; we find that the slightest torsion, say of 10 per cent, backwards, is sufficient to reverse the current, and thus more than neutralize the whole inclination which had been given to the molecules by the permanent torsion. Again, if, whilst the iron wire is under the influence of

torsion, we bring near it one pole of a large natural magnet, laid in the direction of the wire, we find that the currents gradually diminish, until, when the magnet touches the wire, we at last produce zero. The polarized molecules, which under the influence of torsion lay at a certain angle with the axis, have thus been caused to rotate back again and become parallel. Again if we approach the same pole with the magnet at right angles to the wire, we find that the current gradually returns to zero (and, therefore, the molecules to parallelism) when the magnet is about two inches; but on bringing the magnet still nearer, they pass the zero point, now giving increased reversed currents, until they reach a maximum, when the magnet is close to the wire. We have thus rotated the molecules from their original angle of torsion, say of 45° to the right through zero to 45° to the left. If this view is correct, we should expect that we might produce electric currents of reversed directions without the aid of any battery, by simply giving a to and fro torsion to the wire; and this proves to be the case. For we may join the telephone either to the exterior coil or to the simple circuit of the wire, and we shall then hear a sharp click at each movement of torsion to the right and left, thus imitating and reproducing all the effects which would be obtained by rotating a separate magnet through different angles of inclination with the wire. There are many proofs which confirm this view*; but as the object of the author is to show the remarkable difference which exists between iron and steel in this respect, he will confine himself to showing the very great apparent rigidity of the molecules of tempered steel as compared with those of iron.

A very remarkable difference appears when we turn to tempered steel. For here we find that at certain degrees of temper (*e.g.*, that known as blue or spring temper) there are only slight traces of molecular disturbance or rotation, no matter how many mechanical turns or twists we may put on the wire. In fact the molecules here seem fixed and homogeneous throughout the mass. We have perfect molar elasticity, but no traces of rotation of one part over another—in other words, no molecular elasticity. Thus in iron we have an elasticity due solely to the freedom of molecular motion. In hard steel, on the contrary, we have but slight molecular freedom, with great molar elasticity, in which the separate molecules do not rotate separately but all as one mass. It is necessary to point out this difference of molar rigidity as shown in tempered steel and in iron, because tempered steel is not the only form which thus differs in its mechanical and physical qualities from iron or soft steel. A similar difference is shown also by several known alloys of iron. We can decrease the apparent rigidity of steel by the application of heat; for if we pass a constant and powerful current through the steel wire, which previously gave but feeble traces of rotation, and then heat this wire to a red heat, a strong induced current is gradually produced. The current here has the power of rotating the polarized and heated molecules, and so giving out comparatively strong induced currents. But on cooling this wire it is impossible again to reduce it to silence. The molecules remain rigid, but at an angle to the axis. With iron, however, upon the application of heat under the same circumstances, we have a most violent rotation, which entirely disappears on cooling—proving again the great comparative freedom of its molecules. We might believe that all the above effects in steel are due, not to the rotation of the molecules, but to the more or less retentive or "coercitive" force of steel with regard to permanent magnetism. But coercitive force, while it may suffice to explain, according to accepted views, the retention of magnetism, does not explain why we can produce positive and negative currents by right or left-handed torsions, nor why we should have induced currents by torsion. If we accept the term coercitive force as simply applicable to each molecule, then we have still to consider the greater freedom of motion of these molecules in iron than in steel. It is a general belief (which the author has hitherto shared) that the molecules of tempered steel have far greater coercitive force than those of iron. A simple experiment will, however, prove this not to be the case. For if we suppose that the molecules of iron turn with far greater freedom, it follows that they should also

* "Molecular Magnetism," by Professor D. E. Hughes. Proceedings of the Royal Society, March 7–17 and May 10, 1881.

turn by the application of far less force. Now if we take a soft iron and tempered steel wire, and place them at a given distance from a suspended magnetic needle, after finding them both to be free from magnetism, and we then magnetize these wires by drawing them over the poles of a natural magnet, then we may no doubt find as usual that the tempered steel has a far greater amount of remaining magnetism. But if, instead of this, we limit the reactive force of the natural magnet by placing a piece of wood, say ½ inch thick, between the magnet and the wire magnetized, thus limiting and controlling the force to any degree, according to the interval between the magnet and the wire to be magnetized, we then find on magnetizing these two wires with a weak reactive force, and again observing its action upon the needle, that the soft iron still shows powerful retentive or coercitive force, whilst the tempered steel has but feeble traces of magnetism, or none at all. Thus, contrary to the author's previous convictions, it appears that iron possesses more coercitive force than steel whenever the inducing force is limited, and within the range of iron. If iron merely possessed greater coercitive force than steel, it would be impossible for us to employ soft iron in electro-magnets requiring quick changes of magnetism. But although, in the previous experiment, the remaining magnetism was far greater in the iron than steel, yet the magnetic force of the iron, whilst under the influence of permanent magnet, was some twenty times greater than its remaining magnetism; whilst with the steel there was but a slight difference in the force developed whilst it was under the feeble influence of the natural magnet, and when this was withdrawn.

Assuming the freedom of motion of the molecules to be greater in iron than steel, it occurred to the author that he should be able to free the soft iron from its remaining magnetism by simple vibration of the wire. This was found to be the case. An iron and steel wire are magnetized to saturation, or both may be given the same amount of permanent magnetism. We will suppose that they both deflect the suspended needle through 40°. Now taking the steel wire and fastening one end in a brass vice, give its free end a slight pull to set it in vibration: it will be found that the steel has lost but 2°, having still 38° of permanent magnetism, which cannot be further reduced by repeated vibrations. The instant, however, that a similar vibration is given to the soft iron wire, its remaining magnetism nearly all disappears; there is left at most 2°, or in some cases only a trace. Thus the molecules are seen to be so comparatively free in iron that mere vibration will aid them in rotating. These two wires were again observed vibrating whilst under the influence of the permanent magnet. There was then a greater magnetic effect produced in the iron wire than previously, the vibrations aiding the rotations produced by the natural magnet. The author was desirous to render this freedom of iron and rigidity of steel, so that these effects might be actually seen. For this purpose we may take three glass tubes, or ordinary phials, of any length or diameter, say 10 centimeters in length by 2 centimeters in diameter. If we now put iron filings in these tubes, leaving about one third vcaant, so as to allow complete freedom in the filings when shaken, we find that each tube, when magnetized, retain an equal amount of residual magnetism, and that this all disappears upon slightly shaking the tube; we are thus imitating the effects of vibration. But if in one of these tubes we pour melted resin (or in fact any slightly viscous liquid, such as petroleum, suffices) we then render these filings more rigid, and then we can no longer produce by shaking the disappearance of its residual magnetism. In pouring in petroleum we have apparently been introducing a strong coercitive force; but we know that it can only have the mechanical effect of rendering the iron filings less free to turn, and so comparatively rigid. If we desire to see the effect of torsion, we have only to shake the filings so that when the tube is held horizontal the vacant space is above, and rotate slightly (but without shaking) the tube containing the free filings about a horizontal axis. Its remaining magnetism instantly disappears upon rotation, although we evidently have not changed the longitudinal position of its particles. A similar effect takes place upon a soft iron wire, for if we magnetize it and observe its remaining magnetism, we find that upon giving a slight torsion to this wire, its remaining

magnetism instantly disappears—a similar effect to that in the rotating tube of iron filings.

The author has remarked in these researches that in all alloys of iron the molecules are far more rigid than in the pure metal and further that, with steel, tempering adds greatly to this rigidity. He is now engaged upon the question of the effect of different tempers on the same steel, and hopes in a future paper to be able to bring the results before the Institution. Soft steel, when compared with hard drawn iron, shows that the mechanical hardening of iron has not in any great degree diminished its molecular freedom. Even the softest steel shows a high degree of molecular rigidity, as compared with the hardest iron, but far less than that of tempered steel. This would seem to indicate that steel in its softest state is still an alloy, though only feeble quantities of carbon may be held in that condition. We thus perceive that a great physical change takes place in iron upon the slightest alloy with carbon; and that tempering produces this change in its highest degree. The writer therefore is strongly in favour of the view propounded long since, that steel when tempered is an alloy, containing fixed carbon in a far greater quantity than when soft. We know the physical properties of magnetic oxide of iron, of iron and tungsten, and of iron and sulphur. Now in all these the writer has found that the iron loses its molecular freedom when even slightly alloyed. The physical results are therefore the same as those produced in tempering steel; and the induction balance thus indicates strongly that tempered steel shows the characteristics of a true alloy. We could not have such a great physical difference between iron and steel, as above noticed, except by corresponding changes in its mechanical properties; and it is with a view of bringing out these relations in a discussion on this point, that the author has ventured to bring his views before the Inst. of Mechanical Engineers.

ELECTRICITY APPLIED TO EXPLOSIVE PURPOSES.

By PROF. F. A. ABEL, C. B., F. R. S., Hon. M. Inst. C. E.

From "The Engineer."

In introducing the subject the lecturer indicated the principal advantages which it had been early observed would result from a certain mode of firing explosive charges by electric currents instead of by the ordinary fuses, the best of which had inherent defects, greatly limiting their use for any but the simplest operations. He traced the history and development of electric firing from the crude experiments of Benjamin Franklin, about the year 1751, through the various stages in which frictional electricity, volta-induction apparatus, and magneto-electric machines had supplied the means of generating the current, the tendency of late years being to revert to a modified form of voltaic battery for one class of work, and to employ dynamo-electric machines for another class. The history and development of the low tension, or wire fuse, and of the various fuses employed with electric currents of high tension were also discussed, and their relative advantages, defects, and performances were described. The only sources of electricity which at present thoroughly fulfilled the conditions essential in the exploding agent for submarine mines were constant voltaic batteries. They were simple of construction, comparatively inexpensive, required but little skill or labor in their production or repair, and very little attention to keep them in constant good working order for long periods, and their action might be made quite independent of any operation to be performed at the last moment.

When first arrangements were devised for the application of electricity in the naval service to the firing of guns and so called outrigger charges, the voltaic pile recommended itself for its simplicity, the readiness with which it could be put together and kept in order by sailors, and the considerable power presented and maintained by it for a number of hours. Different forms of pile were devised at Woolwich for boat and ship use, the latter being of sufficient power to fire heavy

broadsides by branch circuits, and to continue in serviceabfe condition for twenty-four hours, when they could be replaced by fresh batteries, which had in the meantime been cleaned and built up by sailors. The Daniell and sand batteries first used in conjunction with the high tension fuse for submarine mining service were speedily replaced by a modification of the battery known as Walker's, which was after some time converted into a modified form of the Leclanche battery. The importance of being able to ascertain by tests that the circuits leading to a mine, as well as the fuses introduced into that circuit, were in proper order, very soon became manifest; and many instances were on record in the earlier days of submarine mining of the disappointing results attending the accidental disturbance of electric firing arrangements, when proper means had not been known or provided for as certaining whether the circuit was complete, or for localizing any defect when discovered.

The testing of the Abel fuse, in which the bridge or igniting and conducting composition was a mixture of the copper phosphide and sulphide of potassium chlorate, was easy of accomplishment—by means of feeble currents of high tension—in proportion as the sulphide of copper predominated over the phosphide. Even the most sensitive might be thus tested with safety; but when the necessity for repeated testing, or even for the passing of a signal through the fuse, arose, as in a permanent system of submarine mines, the case was different, this fuse being susceptible of considerable alterations in conductivity on being frequently submitted to even very feeble test currents, and its accidental ignition, by such comparatively powerful test or signal currents as might have to be employed, became so far possible as to create an uncertainty which was most undesirable.

Hence, and also because the priming in these fuses was liable to some chemical change detrimental to its sensitiveness, unless thoroughly protected from access of moisture, another form of high-tension fuse, specially adapted for submarine mining service, was devised at Woolwich. This, though much less sensitive than the original Abel fuse, was sufficiently so for service requirements, while it presented great superiority over the latter in stability and uniformity of electric resistance; and, though not altogether unaffected by the long-continued transmission of test currents through them, the efficiency of the fuse was not affected thereby. Although high-tension fuses presented decided advantages in point of convenience and efficiency over the earlier form of platinum wire fuse, the requirements which arose, in elaborating thoroughly efficient permanent systems of defence by submarine mines, and the demand for a battery for use in ships which would remain practically constant for long periods, caused a very careful consideration of the relative advantages of the high and low tension systems of firing to result in favor of the employment of wire fuses for these services. In addition to the disadvantages pointed out there was an element of uncertainty, or possible danger, in the employment of high-tension fuses, which, though fully eliminated by the adoption of voltaic batteries, in place of generators of high-tension electricity, might still occasionally constitute a source of danger, namely, the possibility of high-tension fuses being accidentally exploded by currents iuducedin cables, with which they were connected during the occurrence of thunderstorms, or of less violent atmospheric electrical disturbances. Experiments and the results obtained in military service operations, had demonstrated that if insulated wires, immersed in water, buried in the earth, or even extended on the ground, were in sufficient proximity to one another, each cable being in circuit with a high-tension fuse and the earth, the explosion of any of the fuses by a charge from a Leyden jar, or from a dynamo-electric machine of considerable power, might be attended by the simultaneous ignition of fuses attached to adjacent cables, which were not connected with the source of electricity, but which become sufficiently charged by the inductive action of the transmitted current. It therefore appeared very possible that insulated cables extending to land or submarine mines, in which high-tension fuses were enclosed, might become charged inductively during violent atmospheric electrical disturbances to such an extent as to lead to the acciden-

tal explosion of mines with which they were connected. In a report by von Ebner on the defence of Venice, Pola, and Lissa, by submarine mines, in 1866, he refers to the accidental explosion of one of a group of sixteen mines during a heavy thunderstorm, as well as to the explosion of some mines, by the direct charging of the cables, through the firing station having been struck by lightning. Two instances of the accidental explosion of tension fuses by the direct charging of overhead wires during lightning discharges occurred in 1873 at Woolwich. Subsequently an electric cable was laid out at Woolwich along the river bank below low-water mark, and a tension fuse was attached to one extremity, the other being buried. About eleven months afterwards the fuse was exploded by a charge induced in the conductor during a very heavy thunderstorm. In consequence of such difficulties as these experienced in the special application of the high-tension fuses to submarine purposes, the production of comparatively sensitive low-tension fuses, of much greater uniformity of resistance than those employed in former years was made the subject of an elaborate experimental investigation by the lecturer. Different samples of comparatively thin wires, made from commercial platinum, showed very great variations in electrical conductivity. Very considerable differences in the amount of forging to which the metal, in the form of sponge had been subjected, did not importantly affect either its specific gravity or its conductivity, and the fused metal had only a very slightly higher degree of conductivity than the same metal forged from the sponge. The conductivity of very fine wires could therefore be but slightly affected by physical differences in the metal, and the considerable differences in conductivity observed in different samples of platinum were therefore chiefly ascribable to variations in the degree of its purity. It appeared likely that definite alloys might furnish more uniform results than commercial platinum; experiments were therefore made with fine wires of German silver, and of the alloy of sixty-six of silver with thirty-three of platinum employed by Matthiessen for the reproduction of B. A. standards of electrical resistance. Both were greatly superior to ordinary platinum in regard to the resistance opposed to the passage of a current; German silver was in its turn superior to the platinum silver alloy; although the difference was only trifling in the small lengths of fine wire used in a fuse—0.25 in.—while the comparatively ready fusibility of the platinum silver wire contributed, with other physical peculiarities of the two alloys, to reduce the fine German silver wire to about a level with it. Moreover, the latter did not resist the tendency to corrosive action exhibited by gunpowder, and other more readily explosive agents, which had to be placed in close contact with the wire bridge in the construction of a fuse, while the platinum silver was found to remain unaltered under corresponding conditions. Experiments have also been made with alloys of platinum with definite proportions of irridium, the metal with which it is chiefly associated, very fine wires of an alloy containing 10 per cent. of irridium were eventually selected as decidedly the best materials for the production of wire fuses of comparatively high resistance and uniformity, this alloy being found decidedly superior in the latter respect, as well as in point of strength—and therefore of manageableness in the state of very fine wire, 0.001 in. in diameter—to the platinum silver wire. The fuses now used in military and submarine services were made with bridges of iridio platinum wire, containing 10 per cent. of the first-named metal. The electrical gun tubes in the navy were fired by means of a specially arranged Leclanche battery, and branch circuits worked to the different guns; in broadside firing, it was important that the wire bridge of any one of the gun tubes which was first fired should be instantaneously fused on the passage of the current, so as to cut this branch out of circuit; in this respect the comparatively fusible platinum silver alloy appeared to present an advantage, hence the naval electrical fuses were made with bridges of that alloy. Uniformity of electrical resistance had become a matter of such high importance in the delicate arrangements connected with the system of submarine mines, as now perfected, that the very greatest care was bestowed upon the manufacture of service electric fuses and detonators, which were in fact

made, in all their details, with almost the precision bestowed upon delicate scientific instruments, and the successful production of which involved an attention to minutiæ which would surprise a superficial observer.

One of the earliest applications of electricity to the explosion of gunpowder was the firing of guns upon proof at Woolwich by means of a Grove battery and a gun tube, which was fired by a platinum wire bridge, a shunt arrangement being used for directing the current successively into the distinct circuits connected with the guns to be proved. When the high-tension fuse had been devised, gun tubes were made to which it was applied, and an exploder was arranged by Wheatstone, having a large number of shunts, so that as many as twenty-four guns might be brought into connection with the instrument, and successively fired by the depression of separate keys connected with each. The firing of cannon as time signals, was an ancient practice in garrison towns, but the regulation of the time of firing the gun by electrical agency from a distance appears first to have been accomplished in Edinburgh, where, since 1861, the time gun had been fired by a mechanical arrangement, actuated by a clock, the time of which is controlled electrically by the mean time clock at the Royal Observatory on Calton Hill.

Shortly after the establishment of the Edinburgh time gun, others were introduced at Newcastle, Sunderland, Shields, Glasgow, and Greenock. The firing of the gun was arranged for in various ways: in some instances it was effected either direct from the observatory at Edinburgh, or from shorter distances, by means of Wheatstone's magneto-electric exploders. At present there were time-guns at West Hartlepool, Swansea, Tynemouth, Kendal, and Aldershot, which were fired electrically, either by currents direct from London, or by local batteries, which were thrown into circuit at the right moment by means of relays, controlled from St. Martin's-le-Grand. About thirteen years ago, the electrical firing of guns, especially from broadsides, was first introdnced into the Navy, with the employment of the Abel high-tension gun tube and voltaic piles. The gun tubes then used were manufactured simply for the proof of cannon and for experimental artillery operations, and were of very simple and cheap construction. Experience proved them to be unfitted to withstand exposure to the very various climatic influences which they had to encounter in her Majesty's ships, and in store in different parts of the world. The low-tension gun tubes, having a bridge of very fine platinum silver wire, surrounded by readily ignitable priming composition, was therefore adopted as much more suitable for our naval requirements. The arrangements for broadsides or independent firing, and also for the firing of guns in turret ships, had been very carefully and successfully elaborated in every detail, including the provision of a so-called drill or dummy electrical gun tube, which was used for practice and refitted by well instructed sailors. The firing keys and all other arrangements connected with electrical gun firing, were specially designed to ensure safety and efficiency at the right moment. The electric detonators for firing outrigger torpedoes, or for other operations to be performed from open boats, corresponded, so far as the bridge was concerned, with the naval electric gun tubes, and were fired with a specially fitted Leclanche battery. These electric appliances were now distributed throughout the Navy, and the men were kept, by instruction and periodical practice, well versed in their use. The application of electricity to the explosion of submarine mines, for purposes of defence and attack, received some attention from the Russians during the Crimean War under the direction of Jacobi; thus a torpedo, arranged to be exploded electrically when coming into collision with a vessel, was discovered at Yeni-Kale during the Kertsch expedition in 1855. Some arrangements were made by the British, at the conclusion of the war, to apply electricity to the explosion of large powder charges for the removal of sunken ships, &c., in Sebastopol and Cronstadt Harbors. In 1859 a system of submarine mines, to be fired through the agency of electricity by operators on shore, was arranged by Von Ebner for the defence of Venice, which, however, never came into practical operation. Early in 1860 Henley's large magneto-electric machine, with a supply of Abel fuses, and stout india-rubber bags, with fittings to resist water-pressure, were

despatched to China, for use in the Peiho river, but no application appeared to have been made of them. The subject of the utilization of electricity for purposes of defence, however, did not receive systematic investigation in England or other countries until some years afterwards, when the great importance of submarine mines, as engines of war, was demonstrated by the number of ships destroyed and injured during the war in America. The application of electricity to the explosion of submarine mines was very limited during that war, but arrangements for its extensive employment were far advanced in the hands of both the Federals and Confederates at the close of the war, men of very high qualifications, such as Captain Maury, Mr. N. J. Holmes and Captain McEvoy having worked arduously and successfully at the subject. The explosion of submerged powder charges by mechanical contrivances, either of self-acting nature or to be set into action at desired periods, was accomplished as far back as 1583. during the siege at Antwerp, by the Duke of Parma, and from that period to 1854, mechanical devices of more or less ingenious and practicable character had been from time to time applied, to some small extent, in different countries, for the explosion of torpedoes. The Russians were the first to apply self-acting mechanical torpedoes with any prospect of success, and had the machines used for the defence of the Baltic been of larger size—they only contained 8lb. or 9lb. of gunpowder—their presence would probably have proved very disastrous to some of the English ships which came into collision with and exploded them. Various mechanical devices for effecting the explosion of torpedoes by their collision with a ship were employed by the Americans, a few of which proved very effective. But although in point of simplicity and cost, a system of defence by means of mechanical torpedoes possessed decided advantages over any extensive arrangements for exploding submarine mines by electric agency, their employment was attended by such considerable risk of accident to those at whose hands they received application that under any circumstances which were likely to occur, they became almost as great a source of danger to friend as to foe. The most important advantage secured by the application of electricity as an exploding agent of submarine mines were as follows:—They might be placed in position with absolute safety to the operators, and rendered active or passive at any moment from the shore; the waters which they were employed to defend were, therefore, never closed to friendly vessels until immediately before the approach of an enemy; they could be fixed at any depth beneath the surface —while mechanical torpedoes must be situated directly or nearly in the path of a passing ship—and they might be removed with as much safety as attended their application.

There were two distinct systems of applying electricity to the explosion of submarine mines. The most simple was that in which the explosion was made dependent upon the completion of the electric circuit by operators stationed at one or more posts of observation on shore; such a system depended, however, for efficiency, on the experience, harmonious action, and constant vigilance of the operators at the exploding—and observing—stations, and was, moreover, entirely useless at night, and in any but clear weather. The other, which might also be used in conjunction with the foregoing, was that of self-acting mines, exploded either by collision with the ship, whereby circuit was completed through the enclosed fuse, or by the vessel striking a circuit closer, whereupon either the mine, moored at some depth beneath, was at once fired, or the necessary signal was given to the operator on shore. Continental nations had followed in our steps in providing themselves with equipments for defensive purposes by submarine mines, and the Danes, Swedes, and Norwegians had pursued the subject of submarine mines, with special activity and success. In the United States the subject of utilization of electricity as an exploding agent for war purposes was being actively pursued, and important improvements in exploding instruments, electric fuses, and other appliances had been made by Smith, Farmer, Hill, Striedinger, and others already mentioned, while no individual had contributed more importantly to the development of the service of submarine explosions than General Abbot, of the United States Engineers.

Illustrations of actual results capable

of being produced in warfare by submarine operations had hitherto been very few; but of the moral effects of submarine mines there had already been abundant illustrations. In the war carried on for six years by the Empire of Brazil and the Republic of Uruguay and the Argentine Republic of Paraguay, the latter managed, by means of submarine mines, to keep at bay, for the whole period, the Brazilian fleet of fifteen ironclads and sixty other men-of-war. In the Russo-Turkish war submarine mines and torpedoes were a source of continued apprehension; and the French naval superiority was paralyzed during the Franco-German war by the existence, or reputed existence, of mines in the Elbe. The application of electricity to the explosion of military mines, and to the demolition of works and buildings, had been of great importance in recent wars in expediting and facilitating the work of the military engineer. The rapidity with which guns, carriages, &c., were disabled and destroyed by a small party of men, who landed after the silencing of the forts at Alexandria, illustrated the advantages of electric exploding arrangements, combined with the great facility afforded for rapid operations by the power possessed of developing the most violent action of gun-cotton, dynamite, &c., through the agency of a detonator. The application of electricity to the explosion of mines for land defences during active war was not an easy operation, inasmuch as not only the preparation of the mines, but also the concealment of electric cables and all appliances from the enemy entailed great difficulties, unless the necessary arrangements could be made in ample time to prevent a knowledge of them reaching the enemy. But few words need be said to recall to the minds of civil engineers the facilities which the employment of electricity to explosive purposes afforded for expediting the carrying out of many kinds of works in which they were immediately interested. Electrical blasting, especially in combination with rock-boring machines, had revolutionized the operation of tunnelling and driving of galleries; and, although in ordinary mining and quarrying operations the additional cost involved in the employment of fuses, conductors, and the exploding machine, was not unfrequently a serious consideration, there were, even in those directions, many occasions when the power of firing a number of shots simultaneously was of great importance. There was little doubt, moreover, that accidents in mining and quarrying would be considerably reduced in number if electrical blasting were more frequently employed. The conveniences presented by electrical firing arrangements, under special circumstances, were interestingly illustrated by a novel proceeding at the launch of a large screw steamer at Kinghorn, in Scotland, which was recently accomplished by placing small charges of dynamite in the wedge-blocks along the sides of the keel and exploding them in pairs, hydraulic power being applied at the moment that the last wedge was shot away. In the deepening of harbors and rivers, and in the removal of natural and artificial submerged obstructions, the advantages of electric firing were so obvious that extended reference to them was unnecessary. A substitute for electrical firing, which had been applied with success to the practically simultaneous firing of several charges, consisted of a simple modification of the Bickford fuse, which, instead of burning slowly, flashed rapidly into flame throughout its length, and hence had received the name of instantanteous fuse, or lightning fuse. The fuse burned at the rate of about 100ft. per second; it had the general appearance of the ordinary mining fuse, but was distinguished from the latter by a colored external coating. Numerous lengths of this fuse were readily coupled up together, so as to form branches leading to different shot-holes, which might be ignited together, so as to fire the holes almost simultaneously. In the navy this fuse was used as a means of firing small gun-cotton charges to be thrown by hand into boats when these engaged each other, the fuse being fired from the attacking boat by means of a small pistol, into the barrel of which the extremity was inserted.

On the railways of England and Wales there were in 1881 2,263 inhabitants per locomotive, as against 2,607 in 1871, and there were 1,017 inhabitants per passenger vehicle as compared with 1,232 in 1861.

THE SECRETION OF GAS IN STEEL CASTINGS.

A Correction by DR. F. C. G. MÜLLER.

From "Iron."

Reserving to myself to deal thoroughly with the importance of silicon in the metallurgy of iron in a paper shortly to be published, I should like to rectify in the following brief reply, a few misunderstandings and errors contained in the open letter addressed to me by M. Pourcel. That letter merely criticises a few minor points in the introductory part of my paper, on the secretion of gas in steel castings. Not the slightest reference is made to the chief argument of the introduction, the large secretion of gas in completely decarbonized iron. I may here add the fact that very often also pig-iron—and not merely the white description—coming direct from tha blast furnace or the cupola, gives castings full of blowholes. In either case we have to deal only with the occlusion of previously absorbed gases, and thus it seems incomprehensible why for the intermediate products, the much more complicated reaction theory should take the place of the absorption theory. But, as already remarked, all these arguments are only subsidiary. The gist of the matter is the absence of carbonic oxide in the blowholes, and the presence of hydrogen and nitrogen.

Now, I have to note the fact that M. Pourcel does not at all doubt the correctness of my experiments, and is continually speaking of escaping hydrogen. He has only desired to disclose "anomalies" in my theory. On his account, therefore, I have not again concisely arranged the whole of my experimental material, but only for those who, like Mr. Snelus and Mr. Richards, think that my borer has decomposed the surrounding water. Besides, my paper is not a polemical treatise; it only contains a few polemical theses, in which I defend myself against direct attacks. Opposed to M. Pourcel, I have always been the attacked party. Once before, two years ago, I was compelled to face him and his followers. But that matter is buried. It is not correct, as M. Pourcel stated at Vienna, that I ill-treated his theory. Up to that time I had not referred to his theory, even with a single word; in fact, I did not know at all that he had a special theory. In my previous paper, I only opposed the statement of M. Gautier, according to which

$$2\,CO + Si = SiO_2 + 2C.$$

In the open letter I am addressed as follows: "No, Dr. Muller. Silicon does not increase the solubility of the gas in the steel; just the contrary." "You know that MM. Troost and Hautefeuille have proved by experiment that silicon almost annuls the solubility of hydrogen in steel." I reply that MM. Troost and Hautefeuille have stated in this connection only the fact that manganiferous iron melted in a hydrogen atmosphere violently scatters in solidifying, whilst silicious iron does not do that. It follows from this that liquid manganiferous iron absorbs more gas at a higher temperature than it can retain at its melting point, whilst silicious iron, on the contrary, possesses no greater degree of absorptive power at a higher temperature than in solidifying. This fact is quite irrelevant to my theory. The question is simply whether the solidified metal, with a greater contents of silicon is able to retain more hydrogen. The other experiments of the enquirers named prove that this is the case. Thus 500 grammes of pig-iron absorbed 46.6, cast steel 7.8, malleable iron 13.9, cubic centimeters of hydrogen. Quite recently, Professor Ledebur has likewise proved by careful experiments, the existence in ferromanganese as well as in silicide, of considerable, and, in both alloys, of equal quantities of hydrogen. It may be added, incidentally, that this eminent enquirer—he is a metallurgist, and not like M. Pourcel's friend, merely a chemist—has given expression to the same view at which I arrived at the same time and quite independently. But besides these experiments, we have also a *demonstratio ad oculos* direct from blast furnace practice, namely, the experiment of M. Pourcel. "The metal to which silicide of

manganese has been added does not give off any carbonic oxide at the moment of solidification, but emits flames of hydrogen, which burn on the surface at the time of casting. . . But if the silicon is added in the form of silicide of iron, and the steel contains only traces of manganese there is no disengagement whatever of gas." While we do not at present enquire into the part played by manganese, we do not lose sight of the following. The steel in question contains hydrogen but of this hydrogen not a trace is secreted as soon as silicon is added. Thus the solubility of hydrogen in silicious iron is demonstrated beautifully and unmistakably by the experiment described.

My remark that in the Bessemer operation violent spiegel reaction leads to solid steel, while slight reaction, notwithstanding high percentage of silicon, gives porous ignots, M. Pourcel has entirely misunderstood. I mean charges which have not been blown enough, and on that account give no or very slight spiegel reaction. The slag was quite liquid in all the charges examined by me, as could not be otherwise, considering the large contents of magnanese in the charge. The fact stated by me and others is no other than that, if the process is interrupted before the bath becomes highly oxygenated, rising steel remains after the addition of spiegeleisen also if there is a large remnant of silicon; but if blowing is continued for some time longer, so that large formation of oxide and strong reaction follow, the same bath gives solid steel. I explain this solidifying effect of a strong spiegel reaction by the supposition that the violent intermolecular development of carbonic oxide carries off the solved hydrogen and nitrogen mechanically, and have confirmed this assumption by gas analysis.

With regard to the participation of the several elements in the disoxidation towards the close of the Bessemer and Martin process, I have stated upon the basis of my experiences that silicon plays a very subordinate part in it. Now, M. Pourcel gives more than a thousand analyses as against my two, in order to show that in adding silicide one-third of Si is consumed. I have examined at different German Bessemer works six spiegel reactions, four of which showed no perceptible decrease of silicon, and two an increase. Those observations confirm what might have been predicted *á priori*, that in the extraordinarily high temperature at the close of the German Bessemer process the oxygen of the bath attacks principally carbon or manganese. Nevertheless, I admit that a little silicon also oxidizes, but that this loss is compensated by a corresponding reduction of silicon from the lining, or even exceeded. In the Martin process I have examined myself one reaction, and received reports respecting two others. There also was no consumption of silicon. But I must correct myself on this point after looking more carefully through my notes. The disoxidation was carried out by first adding ferro-manganese, and next, after this had operated, silicide of iron. Thus, ferro-manganese probably effected the whole reaction. A sample taken after the addition of ferro-manganese showed still a few blowholes; only after further addition of silicide the sample ingots were perfectly solid.

But I should like to remark, respecting the thousand analyses at the Martin works of Terrenoire, that it is not possible in the Martin process to study the spiegel reaction entirely by itself. For the fining process is continued from the slag, by which the added elements must be eliminated, and, in case the bath is not considerably overheated, a large amount of silicon will be consumed. It is, consequently, not known afterwards how much has been used in the disoxidation process proper. I hope to be able in a future paper to report on decisive results in this respect. In the first place at the close of the Thomas process as much as possible of the slag is to be removed and the disoxidation effected by the addition of materials containing various percentages of silicon. Thus a reaction at a high temperature is obtained without disturbing accessory processes. In order to be able to follow the analogous process at a low temperature, the same metal in the oxidized state is poured into a mould, into which by degrees cold silicide or other additions are thrown. But it must not be forgotten that the study of the disoxidation reaction is an independent matter, which decides nothing in the "absorption or reaction" controversy. Whether silicon takes part or not, at any rate a large quantity remains in the

steel, and this enables it to retain a large portion of hydrogen, by which the formation of blowholes is prevented.

Far less than the disoxidation process, the incidents in the middle of the Martin process threw no light upon the point in question. There carbonic oxide is developed regularly, and it is quite unnecessary when M. Pourcel tells me that the development of gas is decreased when silicide is added. Every scientific metallurgist knows that. Even in the ladle the formation of carbonic oxide may proceed further through the better mixing which takes place. But I think that, even if the wildest steel has become quiet after standing for some time in the ladle, reaction has completely ceased. It is too much against all experience that two easily combining liquids which have been poured into each other and stirred were not to give a perfect mixture if the whole is afterwards poured from some height in a powerful stream into a collecting vessel.

Most of my boring experiments were made in ingots of a quietly rising steel rich in silicon and manganese. I mention here that it appeared absolutely quiet on being poured into the ladle. It remained perfectly quiet for about ten seconds also in the mould, and began then to rise slowly without any signs of scattering. The gas secretion forming the worm-like tubules takes place consequently in the already solidified metal after a lengthened interval. It is impossible that it could be caused by a freshly beginning spiegel reaction. Only he who is devoid of any chemical knowledge could resist that conviction. But we may leave entirely out of consideration whether the freshly recurring formation of carbonic oxide is theoretically possible or not. For the blowholes contain no carbonic oxide. They contain hydrogen, as well as from 10 to 20 per cent. of nitrogen. Besides, it is just hydrogen which can never be produced by an oxidizing reaction.

M. Pourcel points to Professor Wedding as an adherent to his theory, and reproaches me for not having mentioned him. Beyond my principal opponents, I have named no one, not even Professor Bauerman, the only one who espoused the cause of hydrogen at Vienna. Up to the present time I am still in doubt to which party Dr. Wedding belongs. No doubt he says that certain synthetical experiments are in favor of M. Pourcel. "It is clear," he states, "that as long as silicon is added to nearly pure iron, all the carbonic oxide, whether soluble or combined with the iron, is reduced to carbon, which unites with the iron, and the blowholes cannot, therefore, contain any more carbonic oxide." This conclusion, consequently, as a matter of fact leads to what I maintain. The blowholes contain, actually, no carbonic oxide, but hydrogen and nitrogen, and do not, therefore, owe their origin to a reaction. As long as the first ingot is not found the pores of which, against the rule, contain chiefly carbonic oxide, it appears indifferent to me whether the not present carbonic oxide might be present if this or that hypothetical reaction took place in the steel or not.

In his well-known work, Dr. Wedding admits both reaction and absorption. With regard to Bessemer steel it is stated by him on page 394:—"A second peculiarity of the Bessemer products consists in the gases which are mechanically dissolved in it, or are formed by chemical reaction after manufacture, and which, it is true, partly escape during solidification, but which also partly remain and form holes." With regard to Martin steel, it is stated on page 549:—"Its property of being more easily applicable to figured castings than Bessemer steel, is based simply upon its greater freedom from absorbed gases."

The Berlin syntheses mentioned are said to have illustrated the reciprocal effect expressed in the following equation:—

$$Si(Fe)_x + 2CO = C_2Fe_x + SiO_2.$$

This reaction, from a chemical point of view, is not at at all improbable, and was discovered years ago by Caron. But it should not be lost sight of that it can take place only at a proportionately low temperature, a red-heat. At the melting point of steel, however, owing to the very greatly increased affinity of carbon to oxygen, the reverse takes place. Such reciprocal reactions are known in chemistry in great numbers. For instance, potassium reduces at a low temperature carbonic acid to carbon, while at a white heat, carbon reduces potassic oxide. In

accordance therewith, in our case the reaction would be:—

$$SiO_2 + C_2Fe_x = 2CO + SiFe._x$$

This, in the first place brings silicon into iron within the blast furnace. In the acid, Bessemer as well as in the Martin process, it likewise effects absorption of silicon and formation of carbonic oxide. It is M. Pourcel who attributes to this reaction a special importance, and cites in his letter a series of fresh proofs. Consequently, the above reciprocal reaction mentioned by Professor Wedding does not take place at the melting point of steel, and M. Pourcel at Vienna, guarded himself most expressly against having ever affirmed it. Even the best syntheses executed in the laboratory should be utilized for the metallurgy of iron only with the greatest care, because the temperatures at the experiments do not as a rule approach the degree reached in the blast-furnace and the manufacture of ingot steel and ingot iron.

THE CAPACITY OF STORAGE RESERVOIRS FOR WATER SUPPLY.

By W. RIPPL, Royal Technical High School at Gratz, Styria.

Selected Papers of the Institution of Civil Engineers.

In the English system for the water supply of towns, by collecting the drainage of large catchment basins, one of the most important problems is the determination of the capacity for storage, which should be provided in the reservoirs.

In the earlier works designed on this plan this point did not receive sufficient attention, because at that time the data required were not available. Hence reservoirs were constructed of insufficient size, causing a sensible deficiency in the water-supply in dry seasons. As the dams of the storage reservoirs could not be raised in height without endangering their stability, new reservoirs and new gathering grounds had to be added—a proceeding sometimes difficult and always costly.

For a long time engineers were obliged to apply the results of experience gained in existing waterworks to the design of new systems, by giving to the reservoirs a fixed capacity for a given area of gathering ground. If, for example, in an existing system of water-supply, a storage capacity of 2,500 cubic meters (88,288 cubic feet) was found adequate for 1,000 hectares (2,471 acres) of gathering ground, the reservoir of a new system was designed to afford a proportionate storage capacity. But as the amount of storage necessary depends on circumstances which vary in different localities, it is clear that in reservoirs thus designed, it is only by accident that a deficiency of water-supply, in a series of years is prevented.

2. Hawksley's Formula.

The purpose of the storage reservoir is to equalize the fluctuations of supply and demand during an indefinitely long period of time. The circumstances of an average year are therefore not sufficient to determine the quantity to be stored. Hence Mr. Hawksley has given an empirical rule, based on the conditions which obtain during a period of three consecutive dry years, or years in which the rainfall is below the average.*

Let R be the average rainfall during three consecutive dry years estimated in millimeters, Z the number of days' storage which should be provided.

Then, according to Mr. Hawksley,†

$$Z = \frac{1,000}{0.198\sqrt{R}}.$$

The volume of water to be stored in one day is

$$T = B + C + V - D,$$

*In such periods the average annual rainfall is taken as one-sixth less than the average rainfall of a long series of years.

†If R is in inches

$$Z = \frac{1,000}{\sqrt{R}}.$$

where B=demand of the town;
C=compensation to the stream;
V=loss by evaporation from the surface of the reservoir;
D=dry-weather flow into the reservoir.

The quantities are all estimated for a period of twenty-four hours.

According to Mr. Hawksley's formula, and in England,

$$Z=100 \text{ to } 250 \text{ days.}$$

The formula is suitable only for English conditions, where the amount of compensation-water is regulated by law, being usually one-third to one-fourth of the available supply from the catchment basin. The formula would not be applicable for German or Austrian localities, where the amount of compensation-water is settled by free agreement with the owners of the water rights.

3. Empirical Method hitherto Adopted.

The method most commonly adopted in deciding the capacity of a storage reservoir is a purely empirical one, and depends on the consideration of the period of greatest drought only.

Any probable quantity is assumed for the capacity of the storage reservoir, and it is further assumed that the reservoir is full at the beginning of the period of drought. By simple addition of the monthly supply to the reservoir during such a period, and subtraction of the supply to the town, and for compensation, also estimated for successive months, a calculation is made of the quantity in the reservoir at the end of each month for a period of a year. Should the calculation show a deficiency (the volume in the reservoir appearing as a negative quantity), the capacity originally assumed for the reservoir is increased, and the caculation is repeated.

The proceeding is an imperfect one, and is also laborious. The calculation may be shortened by assuming, as the capacity of the reservoir, the sum of all the deficiencies during the drought instead of any imperical quantity, and then making the detailed calculation for the period of a year.

But this method of calculation is open to the objection that it is only in certain cases that the capacity of the reservoir arrived at is sufficient to equalize the fluctuation of supply and demand, not only during a single drought, but during a series of periods in which the supply is deficient.

The records of the rainfall at Vienna prove this assertion. The least rainfall, and consequently the least available supply to the reservoir, generally occurs in the winter months. Thus for December, January and February, the standard rainfall is at the rate of 111 millimeters (4.44 inches). The driest winter was that of 1857-58, when for these months the rainfall was 42 millimeters (1.68 inch). In calculating the capacity of the reservoirs for the water-supply to West Vienna, this winter was taken as the basis of the calculation by the technical experts, and the empirical caculation for the driest year (1858) was made by the method explained above.

But the graphical method of the author applied to this case, and embracing a period of thirty-seven years, during which records of rainfall were available, showed that both in the dry period, 1858–59, and in that of 1855–56, a greater storage capacity was required than in the period 1857–58. Further, the true measure of the storage required was found to be that necessary to equalize the supply and demand during the period extending from May 1855 to May 1856, as it was during that period that the greatest deficiency for the whole period of thirty-seven years was found to occur.

4. New Graphical Method of Calculation.

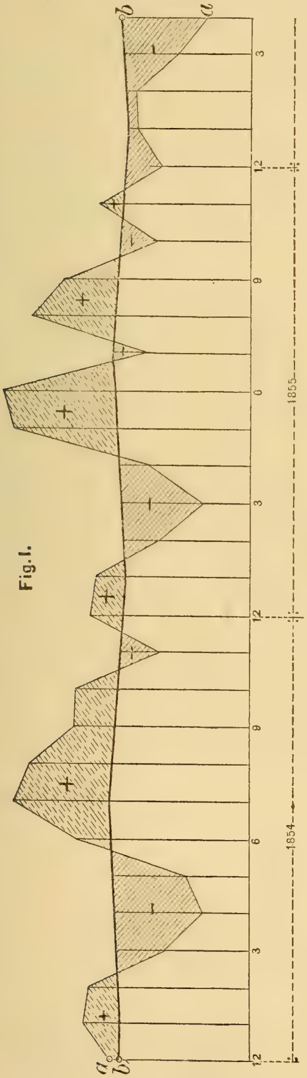
The following is an outline of the author's method of determining the capacity required for storage, to equalize the supply and demand during any period for which rainfall observations are available.

First the supply to the reservoir and the outflow are estimated for successive equal periods of time, usually one month, and for the whole period of time to be considered. The successive intervals of time are set off along an axis of abscissas, to any convenient scale, and the estimated inflow to and outflow from the reservoir in each interval are set up as ordinates. Connecting the points thus found two curves (or broken lines) are obtained,

which may be denominated briefly as the supply-curve and the demand-curve.

The form of the supply curve *a a*, Fig. 1, or curve, the ordinates of which represent the available supply to the reservoir, will in general be similar to the rainfall-curve.

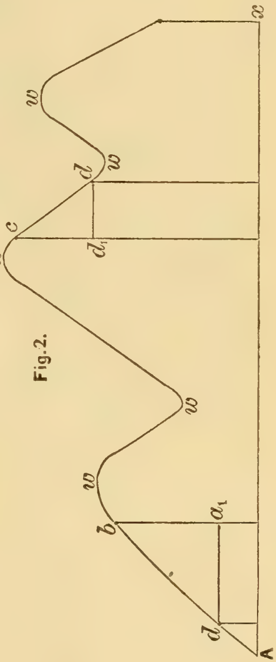
Fig. 1.



The demand-curve will have a form similar to the curve *b b*. It will be the same every year, if the compensation-water is taken entirely from the reservoir. If, however, the compensation-water is partly supplied from streams which do not flow into the reservoir, as in the case of the West Vienna project, then the quantity of compensation-water from the reservoir, and consequently the whole outflow from the reservoir, will vary, increasing as the rainfall decreases, and *vice versa.*

The difference between the two ordinates at each month's end represents either a surplus (positive) or deficiency (negative), according as the ordinate of the supply-curve is greater or less than the ordinate of the demand-curve. These surpluses and deficiencies for each month

Fig. 2.



are measured and entered in a table as follows:

Column 1 gives the periods for which the successive surpluses or deficiencies are estimated.

Column 2 the surpluses or positive differences of the ordinates.

Column 3 the deficiencies or negative differences of the ordinates.

Column 4 contains, opposite each date, the algebraic sum of the numbers in columns 2 and 3 up to that date.

The following table gives the numbers thus obtained from the diagram, Fig. 1.

Next, the numbers in column 4 are set off on a diagram as ordinates, the abscissas being the intervals of time as before (Fig. 2). The foot point of each

ordinate is the end of the corresponding abscissa, representing the period of time estimated from the point of time at which the calculations begin. By joining the ends of the ordinates a curve is obtained, which will be denominated the mass-curve.

1	2 Differences. Surplus. +	3 Defi'cy. —	4 Algebraic sum or ordinate of masscurve.
1854 End of Dec.	—	—	0
1855 " Jan.	1,048,877	—	+1,048,877
" " Feb.	697,030	—	1,745,907
" " Mar.	—	989,153	756,754
" " Ap'l	—	671,962	84,792
" " May	5,330,640	—	4,415,432
" " Jun.	5,006,492	—	9,421,924
" " July	—	197,561	9,224,363
" " Aug.	4,246,729	—	13,471,092
" " Sep.	1,426,470	—	14,897,562
" " Oct.	—	377,643	14,519,919
" " Nov.	1,621,640	—	16,141,559
" " Dec.	—	397,039	15,744,520
1856 " Jan.	828,979	—	16,573,499
" " Feb.	833,518	—	17,407,017
" " Mar.	—	1,368,287	16,038,730
" " Ap'l	—	2,651,043	13,387,687
" " May	395,225	—	13,702,912
" " June	1,548,786	—	15,331,698

The mass-curve has the following properties:

1. For the interval of time between any two points on the axis of abscissas, the difference of the corresponding ordinates is the surplus, if positive, or deficiency, if negative, during that interval. An ascending part of the curve therefore marks a period during which the quantity in the reservoir is increasing and a descending part of the curve a period during which the quantity in the reservoir is diminishing.

The crests and hollows, w, of the curve indicate those instants of time at which the supply and demand are equal

2. If a horizontal line is drawn forwards at a crest, for example, the line $w_2 f$ at w_2 (Fig. 3), the distance $e f$ of a point e on the descending part of the curve, from the horizontal line, represents the total deficiency within the period represented by $w_2 f$. To cover this deficiency there must have been previously an equal storage, and $e f$ therefore represents the amount of storage required to meet the deficiency in the period $w_2 f$.

3. From what previous point of time the storage to meet the deficiency must have commenced is found, by drawing the horizontal line $e g$, backwards from e, till it meets an ascending part of the curve. In the period represented by $g e$ the supply and demand are equal. Hence $g e$ may be termed a balancing line. This is true of any point such as x, Fig 4, on a descending part of the curve, that is the supply and demand are equal for the period represented by the balancing line $x y$, all the subordinate deficiencies being balanced by corresponding surpluses, as is indicated by the shading of the diagram.

4. In the mass-curve certain hollows t_1, t_2, t_3, (Fig. 5) may be selected. Lines drawn through these points mark off periods within which the surplus during one part of each period must be stored to balance the deficiency in another part of the period.

5. The quantity of water represented by the vertical projections Y_1, Y_2, Y_3.... of the remaining portions of the ascending curve, between the lines, flows away or at all events is not required to meet the demand during the period of time considered.

6. The vertical distances J_1, J_2, J_3.... between the lowest and highest crests and hollows in each period are the storage capacities required to equalize the fluctuations of supply and demand during those periods.

7. Balancing quantity J. The greatest possible vertical distance J, between a crest and hollow for any one period represents the capacity of the storage reservoir which is sought for. For if the storage reservoir is capable of equalizing the supply and demand during the period in which J is greatest, it is sufficient in all other periods.

8. The period in which this greatest value of J occurs is therefore the critical period. In the case shown in Fig. 5, the critical period is that represented by t', t_1; during that period all the surplus of supply over demand during parts of the period must be stored to meet the deficiency in the remainder of the period.

9. The ordinates of the mass-curve will be positive (drawn upwards) so long as the sum of the surpluses is greater than

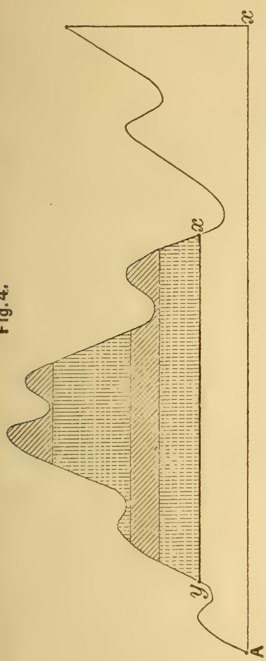


Fig.4.

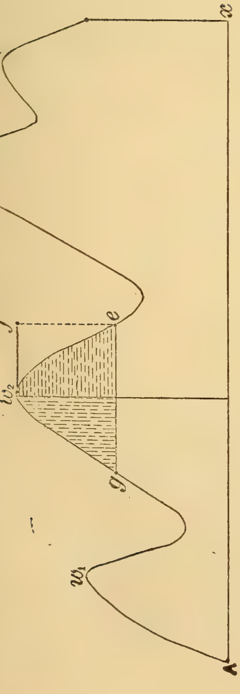


Fig.3.

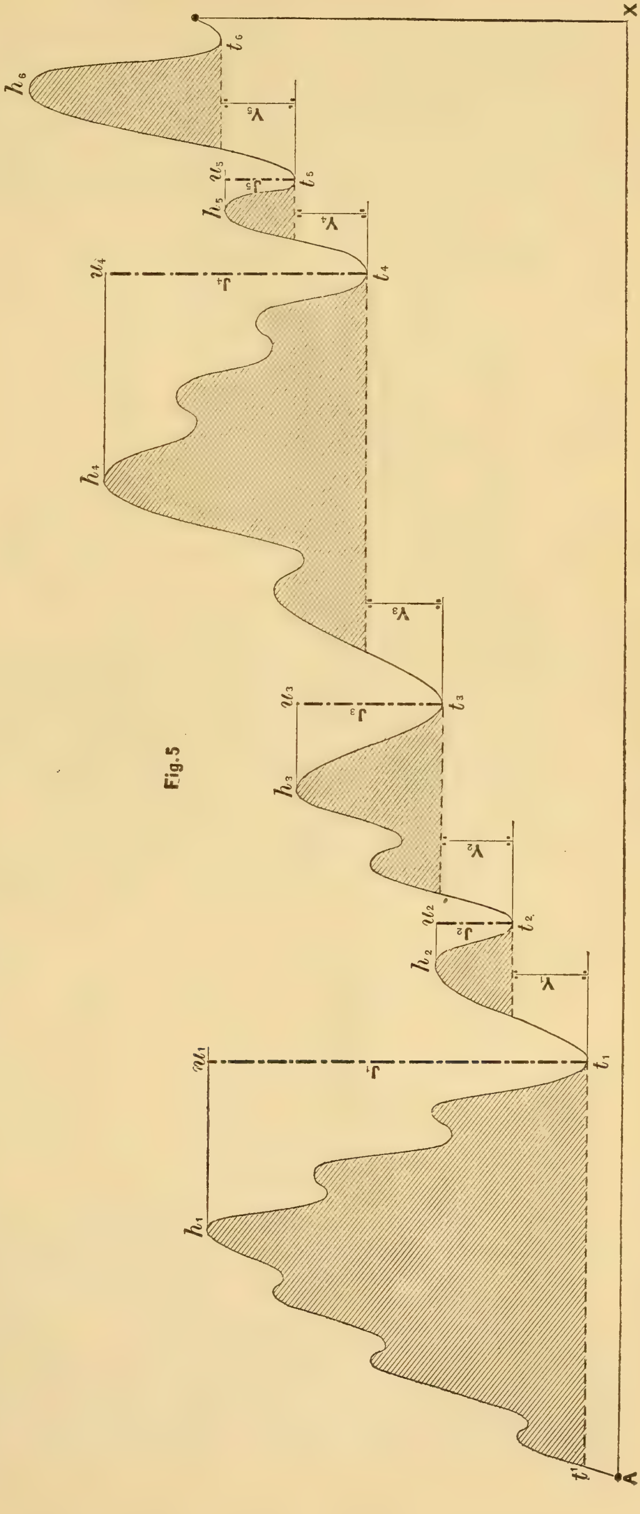


Fig.5

the sum of the deficiencies up to the point of time considered. When the mass-curve crosses the axis of abscissas the whole previous surplus is exhausted, and if it falls below the axis of abscissas, it is necessary to carry back the curve to some earlier point of time than that at which it was first started. Thus, for example, suppose the curve had been drawn from A (Fig. 6), and had been observed to fall below the axis of abscissas beyond B, then it would be necessary to prolong the curve backwards from A to *a* to make a complete investigation of the period within which the points A and B occur possible, since they fall within a period the balancing line of which is *a b*. In other words the mass-curve will remain above the axis of abscissas so long as the total supply estimated from the beginning of the time considered exceeds the total demand, and only falls below it if the demand exceeds the supply.

It is clear from what has been said that a single period of drought does not afford a safe basis for determining the proper storage capacity of a reservoir. That storage capacity can only be determined with safety, by examining a series of such periods. Hence also it is not the year in which the least total rainfall occurs which gives the measure of the storage capacity required, but the period in which the greatest fluctuation of supply and demand happens. The limitation of the time considered to a year is erroneous in principle, because the year is in reference to the question to be solved an unessential condition. The essential intervals of time are the periods during which the supply and demand are balanced.

Fig. 6.

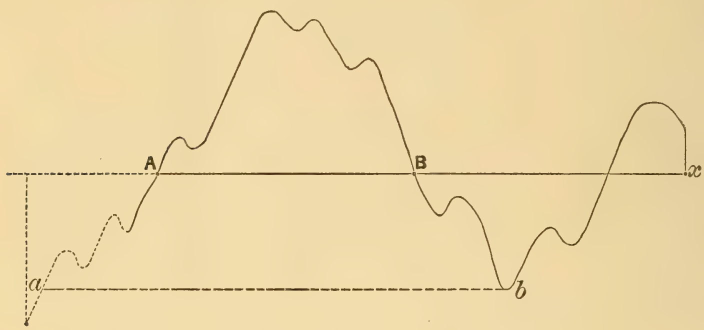
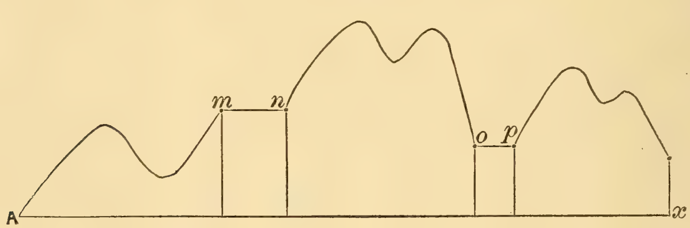


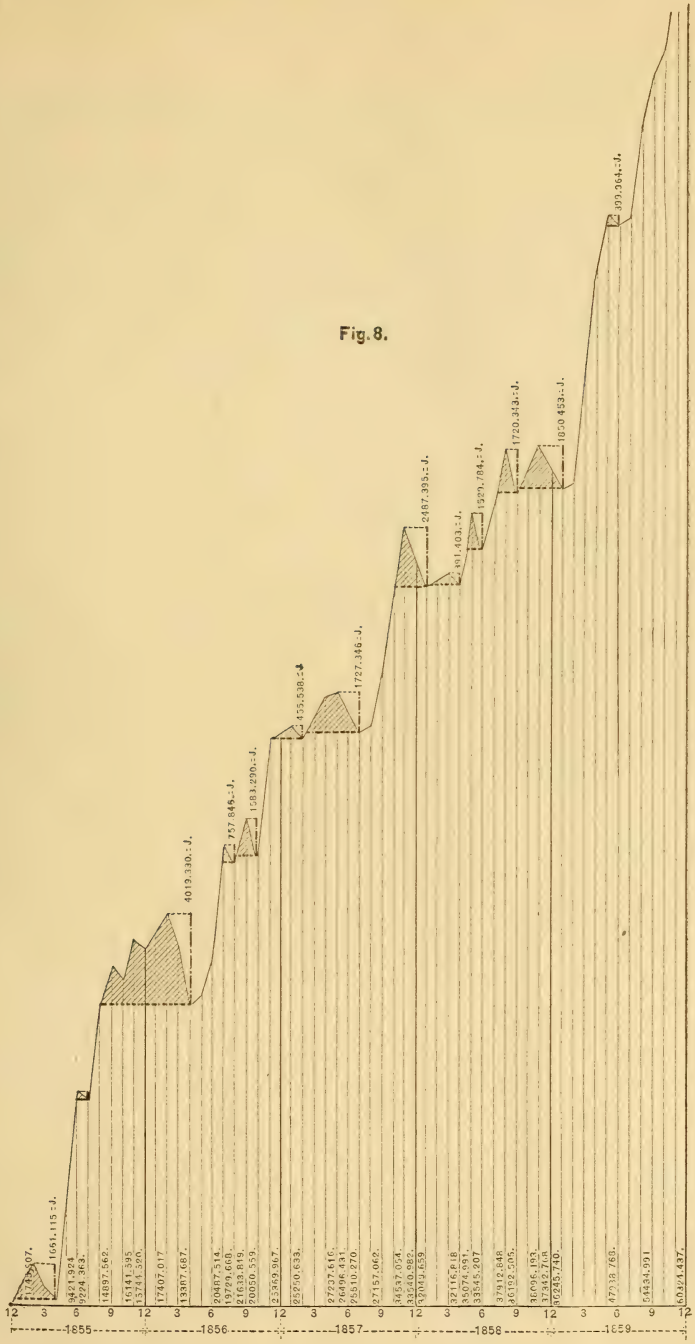
Fig. 8, represents part of a mass-curve based on observations of the rainfall in the district of the Wiener Wald, and comprises those portions of the curve only which required to be taken into detailed consideration. The curve was drawn on the method described above, and by examining it, it will be seen that a storage capacity of 4,019,330 cubic meters is required, to equalize the greatest fluctuation of supply and demand

Fig. 7.



10. The mass-curve becomes a straight line parallel to the axis of abscissas for any periods such as *m n*, *o p* (Fig. 7), during which the supply and demand are exactly equal. Such cases, however, occur rarely in practice.

Fig. 8.



during a period extending from the end of 1854 to the end of 1859. It will be seen also that it is not in the driest year, 1858, that the greatest fluctuation occurred, but in a period extending from the end of August, 1855, to the end of April, 1856. It is this period, therefore, which is the critical period, and which determines the capacity required for storage.

THE MASONRY AND CARPENTRY OF THE GREEKS AND ROMANS.

From "The Builder."

Professor C. T. Newton, C.B., delivered the fourth of a course of eight lectures on the useful and decorative arts of the Greeks and Romans, on Friday, the 11th inst., taking as his subject "Masonry and Carpentry." He observed that in the present lecture he had to deal with two of the most useful arts,—arts which enabled people to obtain shelter from the weather, viz., the arts or handicrafts of masonry and carpentry. Those arts, as he need hardly say, were closely connected with acrhitecture on the one hand, and with purely decorative work on the other. In the first instance he desired to say a word or two as to the condition of artisans generally in Greece, and as to the estimation in which artisans were held. In the poems of Homer the few artisans or skilled workmen who were noticed at all were put rather prominently forward, as if they were highly esteemed in their several crafts. We found mentioned by name several craftsmen, such as workers in metal and workers in wood. The lecturer was not aware, however, that any masons were mentioned by name in the Homeric poems; but Paris was described as building his own house with the assistance of masons, while the walls of Troy were believed by the ancients to have been built by two of the gods, and it was recorded that a disagreement about this work was the original cause of the quarrel between the Trojan dynasty and some of the gods, for it was contended that the two gods who built the walls were not paid their proper wages. We obtained in Homer the idea that the craftsman was a man very necessary to the community, and that he was regarded more or less as a *demiurgos*, or public servant, just as the physician in those days was a public servant who undertook to look after the health of the army. Of course it was to be concluded that in an heroic age such as Homer described the craftsmen would be very few in number, although their services would be very necessary. It was remarkable that Ulysses himself took rank as a carpenter, for Homer described how the king made his own bedstead, which was spoken of as being inlaid with ivory and other ornaments, and supported by the stem of an olive-tree. In tracing the history of the Greeks down from the Homeric age to, say, the time of Pericles, we found in Attic writers of the period a fine distinction drawn between the craftsman and the full citizen, *i.e.*, the citizen who enjoyed the fullest privileges. The full citizen was a member of the Athenian public who was not only endowed with great privileges, but he had to fulfil certain high and imperative duties, such, for instance, as taking part in the defence of his country, and to prepare himself for this particular work by carrying on his gymnastic exercises. On the other hand, the man who was perpetually working at his trade could hardly be said to take any part in public affairs, or to have any forensic life. He stood at his stall and worked at his trade while those who were not obliged to maintain themselves by working as craftsmen took part in the public assembly. Probably in the greater number of cases the craftsmen or skilled workmen were not full citizens. They were either slaves or strangers whom the Greeks allowed to live in Athens and practise their callings, but without giving them the privileges of citizenship. The extensive works which were going on in the Periclean age naturally attracted large numbers of these foreign workmen, including both masons and carpenters. The trades followed by these artificers descended from father to son, but the hereditary transmission of trades was not so exclusive as to involve anything like caste, neither did we find that marked tendency to form corporations or guilds of craftsmen which we saw in Roman times, and which went on throughout the Middle Ages. Proceeding with the immediate subject of his lecture, Professor Newton said that unfortunately there was very little to be found in Greek or

Roman writings as to masons and carpenters. He presumed that the German works which dealt with trades and crafts had brought together every passage from the ancient authors that could throw any light upon the subject. Such passages, however, were very scanty and meagre. There were, however, other sources of information, as in the mouments themselves. From the writings of Homer it was quite clear that there must have been workmen at a comparatively early period, capable of making the structures of polished marble and the furniture, which were described by the poet. That the Homeric descriptions were trustworthy had been proven by the researches of Dr. Schliemann, whose discovery of the Cyclopean walls of Mycenæ and of the neighboring fortress of Tiryns afforded a remarkable confirmation of some of the poet's statements. Cyclopean walls of this character were noticed by Pausanias as being regarded, in his time, as of remote antiquity. They derived their name from the belief of the ancients that they were built by the Cyclopes, a one-eyed race of beings of gigantic stature. These walls were really specimens of military architecture, those at Tiryns being as much as 27 ft. in thickness. Such walls generally contained internal covered galleries within their thickness, with loopholes from which to take aim at an enemy. Pausanias said of the blocks of stone of which the walls of Tiryns were built, that there were none so small as to be conveyed by less than a pair of mules- He described stones 10 ft. and 12 ft. long throughout these walls, and the immense size of the blocks had been urged as proof of the great age of the walls. Modern writers on prehistoric man had invented the word "megalithic," and spoke of the "megalithic period" as that during which structures like Stonehenge and the walls of Tiryns were built. The lecturer thought it very probable that the earliest walls built in Greece were composed of very large stones. But on examining the examples of Cyclopean Pelasgic or other early masonry extant, it would be found that they were of two kinds, first of all, those in which rectangular blocks of stone were used, laid in regular courses, though some what irregularly ;and secondly, those which were of what might be called polygonal masonry which was composed of stones with many sides and with many angles. At Mycenæ both kinds of masonry existed, and in both kinds the enormous size of the blocks was apparent. To convey those huge blocks up mountain sides, and to place them upon their proper beds, must have been a very difficult operation mechanically, especially in those times when, it might be assumed, the art of road-making was not much understood. The large blocks were often found fitted together imperfectly, leaving interstices in which other and smaller blocks were fitted. Walls of this character were regarded by Dr. Schliemann as belonging to the earliest period. After this early period of imperfect joints or interstitial stones, we came to a later masonry, in which the blocks were carefully and solidly fitted together, so that the wall presented a perfect face. With this, however, there was a diminution in the size of the blocks. The polygonal masonry of the Greeks no doubt owed its distinguishing characteristic to the application of the law of cleavage, with which the Greeks must have been familiar. The lecturer said he referred to these points because they had an immediate bearing on the question of the age of the things discovered by Dr. Schliemann at Mycenæ and Tiryns and at Orchomenos in Bœotia. He (Professor Newton) had ventured to ascribe those antiquities to a very early time,—as early as the year 1200 B. C. It might be said that it was only arguing in a circle to say that because at Mycenæ and Tiryns there were specimens of very old masonry, therefore the things found by Dr, Schlieman were old; but the lecturer contended that the opinion of the ancients themselves was worth a good deal, for they had a range of observation over deserted and ruined cities which we never possessed. But those of his hearers who cared to pursue the question would find it very ably discussed from the negative side in an excellent dissertation on the subject in the fifth volume of the "Classical Museum," by Mr. Bunbury. In the later period of masonry the chisel was very skillfully used. In the harbor of Cnidos are the remains of a mole, the blocks of stone composing which, the lecturer believed, from his personal observation, to have been fitted together in the same way as the walls of Mycenæ

and Tiryns. When he (the lecturer) was at Cnidos, he was accompanied by an engineer, who assured him that one block on the top of the mole weighed at least 50 tons. To get such an enormous block into position with the simple mechanical means which the ancients had at command was no small undertaking. They had no hydraulic jacks, no steam power, but only such simple appliances as the pulley and wheel. Coming to the masons' workmanship exhibited in Greek architecture of the best period, the lecturer observed that the perfection of the joints was a marvel to behold, even at the present day. In building up a column composed of a number of blocks of marble or stone, the problem was how to place each drum, weighing perhaps, 2 tons or 3 tons, upon the bed prepared for it, with absolute exactitude; another problem was how to place in position a piece of architrave, say 18ft. long (for such dimensions were adopted) without injuring it at the edge? Such feats were accomplished by the Greeks, and wherever one looked, whether at Athens, at Priene, or at any other of the sites famous for temples or other buildings, the fragments of architecture which remained exhibited the same beautiful perfection of the joints, called by the Greeks *harmos*. On careful examination of these joints it would be found that the outer edges or margins of the surfaces which were to come in contact were polished. These polished margins inclosed the rougher surfaces of the stones, which were sunk a little so as to allow the polished surfaces to come close together. Illustrations of this were to be seen in the British Museum in the marble tiles from the temple of Artemis at Athens. Mr. Penrose, in his valuable work on the Parthenon, referring to the accuracy of the joints, expressed his inability to tell how the builders could have got the joints so perfectly true, but he suggested that a pin or pivot was fixed in the lower of two drums and a hole to receive the pivot made in the lower surface of the uppermost of the two drums, which was made to revolve upon the lower drum, and so grind the surfaces true. But, whatever might be said in favor of that theory, it obviously would not explain how the Greeks managed to get such perfection of jointing in such features as architraves, &c. Since Mr. Penrose's book had been published, an important inscription, in the shape of an order or law giving directions as to the construction of a temple, had been discovered. This inscription was really the architect's specification, and in it the point was very strongly insisted upon all through that every block of stone used for the temple should be prepared and dressed in a particular manner, and be subject to the approval of the surveyor appointed by the city. Constant reference was made in this specification to the polishing of the joints by the application of vermillion and a particular sort of oil to the surfaces of the joints. Although that inscription had been published in Germany by Fabritius, he had not so far as the lecturer knew, proposed any explanation of that combination of oil and vermillion. A very interesting translation of the inscription was published in the *Builder* a few years ago, probably written by Mr. Watkiss Llyod, who was a very ingenious commentator upon Greek architecture. The writer threw out the hint that the vermillion was used for the purpose of ascertaining whether the two surfaces of marble, which were to be contiguous, were perfect planes. The mode of application was this,—taking two drums of columns, which had to fit truly together, if one of the intended contiguous surfaces were covered with some conspicuous wet color, with which the other contiguous surface were brought in contact by placing the stones one above another in their intended position, it would be readily apparent on separating the stones whether there were any inequalities in their contiguons surfaces. A similar process of working was followed, he found, by masons in the present day, so that here was an illustration of a method of work, probably invented by the Greeks, coming down to the present time from generation to generation of masons. The Greek builders used no mortar, but where the work was of the best character they used cramps either of stone or copper dovetailed into the work. The copper so used being of considerable value, there was no doubt that many ancient buildings had been ruined simply for the sake of obtaining the copper cramps. It was difficult to arrive at any conclusion as to the rate of wages paid to Greek masons, inasmuch as the evi-

dence afforded by inscriptions, &c., did not throw any light on the question whether the rates mentioned included food and lodging. Proceeding to speak of Roman masonry, the lecturer alluded to the way in which the Romans used brick and rubble as a backing or filling-in to their masonry. Their mortar was of very great strength, due to the fact that in its composition they used *pozzuolana*, an earth found near Puteoli. The Romans built walls of brick, sometimes laid in regular courses, sometimes in a kind of *opus reticulatum*, contenting themselves with a mere veneering of marble or stone. With regard to the carpenters' work of the Greeks and Romans, there was very little that could be said, for hardly any of their woodwork had come down to us. It was evident, however, from passages in Homer's works, that even in the times of the poet there must have existed some very good specimens of inlaid woodwork, prototypes of that of which we saw representations on the earliest vases and on various ancient monuments. In the Archaic Greek room at the British Museum there were some chairs which were more elegant in form and more honest in construction than some modern chairs, for they were constructed with mortise-and-tenon joints. The Greeks were certainly acquainted with glue from the time of Homer, but, wiser than modern furniture-makers, they preferred to use the mortise and tenon. With regard to the coarser work of the carpenter, such as that required for the roofs of buildings, a good idea of it was to be gained from the roofs of the Etruscan tombs, many of which had been carved in the rock in evident imitation of timber structures.

HARD ARMOR.

From "The Engineer."

For many years the only kinds of armor that found favor in any country consisted of wrought iron in some form, whether laminated, plate upon plate, or solid. Now a change is taking place, the magnitude and importance of which we venture to think is not realized as yet by the authorities of any country. It is taking place gradually, and for this reason, perhaps, its full significance has escaped notice. We believe it will, in a measure, re-shape what has latterly been laid down as to guns and projectiles, and perhaps ships also. If we are right, we are speaking of a question of unusual interest and importance. We will endeavor to be clear and concise. The matter may be briefly put as follows: Until comparatively recently wrougnt iron armor—which for the sake of distinction we call "soft" armor—was the only kind employed. The method almost universally adopted of attacking it was to perforate it—that is, to drive projectiles through it. Now armor of a harder nature has largely come in—that is, chilled iron shields for coast batteries, and steel and steel-faced armor for ships. This can be made to resist perforation; it must be destroyed by breaking into fragments. Such armor then, for the sake of distinction, may be called "hard" armor. It will not perhaps come in universally, but we think, as time goes on, it will become almost universal, for reasons to be given hereafter. And now comes the important point. The operation of breaking up plates and that of perforating them are of a sufficiently distinct and separate character to be best performed by different natures of guns and different kinds of projectile; and to such an extent is this the case, that we believe it may affect the class of ships employed. This is not yet clearly recognized, as we think we can show by the fact that experiments in breaking up plates are still brought to the standards, or based on the data, which apply to perforation—which, as we have said above, are, we believe, liable to give very wrong results when so used.

The long courses of experiments against soft armor have furnished data which have been employed in various ways by different authorities in drawing up formulæ by which to calculate perforation. In no case has a perfect sys-

tem been drawn up. All require a certain amount of empirical correction, but with this very good results can be obtained on several systems. In each of them the result depends on the amount of energy, or stored-up work, in the shot being divided by either the area or the diameter of the whole made in the plate; the empirical correction varying according to which system is employed. It naturally must follow, both in theory and practice, that a shot of small diameter has a great advantage in only having to make a small hole; hence the remarkable results which have been obtained by new type guns firing projectiles of small diameter at high velocities. In the work of perforation so great is the advantage that may be obtained by this class of guns, that they naturally have found their way into our most important armaments. If a long 18-ton 9.45 in. gun is able to perforate as much armor as one of 38 tons and 12½ in. calibre, it is natural that the former would be preferred, or, at all events, some approach to the type which gave such results will be preferred. This has greatly strengthened the desire for moderately large guns instead of those of extreme size. If a 43-ton gun could be made having a power of perforating 24 in. of armor, it might naturally be preferred to the Inflexible 80-ton guns, which perforate little more than 25 in. Of course, the latter would fire a larger and more formidable shell; still, this advantage would be dearly bought by so enormous an increase of weight. The ships, to a great extent, took their shape on the same general lines. The Italians were, as it were, bid welcome to their monster ships and 100-ton guns, which, after all, could only perforate about 28 in. of armor. The 43-ton guns of the Colossus or Edinburgh ought to pierce their sides at close quarters, and the additional power seemed dearly bought in the heavier guns. Even in the future, perforation was expected to effect great things. Steel shells could be driven clean through thick armor, not only without fracturing, but without setting up materially; so that there was a good prospect of their carrying bursting charges into the interior of armored structures, and thus, in a great measure, destroying the value of armor—for it is naturally held that so long as dead metal only is driven through the side of an armor-clad she maintains her chief advantage over wooden ships.

Before this had taken place, however, success had been achieved in another direction, of such a character as to make it impossible, in our opinion, for these steel shells containing gun-cotton to have any great future importance, and also such as to upset previous calculations as to guns and armor generally. This, perhaps, is not yet recognized; indeed, quite recently we heard one of the very highest authorities complaining that experiments on what appeared to him so important a subject, namely, the carriage of gun-cotton bursting charges through armor by steel shell, were not pushed on. Now we believe that while the action of steel shell containing gun-cotton bursting charges may be very powerful, and well worth investigation, its application to armor will be very limited indeed, owing to the introduction of the hard armor of which we have now to speak. In 1873 Grusen's *chilled iron armor* had so far showed its powers of resistance that its adoption became only a matter of time. It has now taken its place as the only kind of coast armor employed by almost every nation except England. Russia, Germany, France, Austria, Italy, Spain, Portugal, Holland and Belgium, and Denmark, have all in some shape adopted it. This is the hardest kind of armor extant. It is impossible to perforate it. It must be gradually broken up, which can be done with steel projectiles. Then again, in 1876, the first Spezia trials with the 100-ton gun established the power of *steel* to stop the passage of a projectile whose power of perforation would have been far in excess of the plate had the latter behaved like wrought iron. The steel, it is true, effected this result, by transmitting the shock through its mass, and so suffering wholesale destruction. This, however, is preferable to perforation if there is any possibility of the latter taking the form of a steel shell passing intact into the vessel, and then bursting under the force of explosion of an enormously powerful gun-cotton charge. Steel was, at that time, adopted by Italy, and soon after *steel-faced armor* was so far successfully made and perfected in this country that this material is now adopted

for most of the powerful ships built in England or abroad.

Now the conditions of attack are changed. Chilled projectiles have, in this country, been declared almost useless against steel-faced armor, for reasons dwelt on in the report on the Spezia trials of *The Engineer* of December 1st, 1882. More than that, it appears probable that our basis of calculation is no longer right. One shot can hardly claim an advantage over another in the circumstance that it requires a smaller hole to enable it to pass through a plate, if, as a matter of fact, neither of them is able to make any hole at all. Hence it appears probable that the effect of a shot against hard armor is not proportional to its power of perforation, but simply in proportion to its total energy or stored-up work, coupled with its power of holding together, so as to deliver the maximum amount of such work possible before breaking up. It is true that the shot's point penetrates a certain distance into steel, but in most cases of steel-faced armor, and in all cases of hard steel, the plate keeps out the shot, and only yields by breaking up and becoming dislodged. In such cases we believe that the effect is proportional chiefly to the stored-up work; and we believe that certain artillerists, Captain Andrew Noble for one, have long recognized this, yet in every experiment, British or foreign, the authorities seem to match the shot against the armor, whether hard or soft, according to its power of perforation.

The total amount of energy contained in the blows delivered on any shield have been noticed especially by Colonel Inglis in his reports, but no systematic use of such statements has been made as far as we know. This is easily accounted for; for in the case of wrought iron plates, when the iron was not actually perforated, it would bear so large a number of blows that destruction by such means was not generally practicable; now it is otherwise. We have returned to the days of "racking." Hard armor must be destroyed in this manner, and it seems reasonable to think that measuring the effect of a blow on hard armor by its power of perforation into soft armor is grossly wrong. We will give a few examples. On July 21st, 1880—*vide Engineer*, July 30th, 1880—a $12\frac{1}{2}$ in. chilled shot weighing 828 lb. struck an 18 in. steel-faced plate with 1504 ft. velocity. Its striking energy was 12,980 foot-tons; its calculated power of perforation equal to 18.57 in. of iron. The shot broke up against the plate producing some insignificant face cracks and other injuries. At Spezia in November last—*vide Engineer*, November 24th, 1882—chilled shot from the 17.72 in. muzzle-loading 100-ton gun struck steel-faced plates of Cammell and Brown 18.9 in. thick, with velocities of 1219 ft. and 1222 ft. respectively. The shot weighed about 2000 lbs. each, hence the calculated perforations should be 19.26 in. and 19.33 in., the energies being 20,600 and 20,710 foot-tons. The Cammell plate had a large piece separated by a through crack or fracture, and the Brown plate had long cracks, through which the plate gave way on the next round. It is difficult to compare these cases fairly, but it may be assumed that Cammell's plate in 1882 was at least as good as that in 1880, if not better; yet the difference in the fracture in the thicker plate is much greater than that shown by the relative figures 18.57 and 19.26. We have suggested in the report on Spezia that the Italian shot held together better. This it might do, from the metal being softer and more tenacious, as well as from the striking velocity being lower; but, on the other hand, it may well lead us to notice that the shot which produced so little effect had under 13,000 foot-tons, while the others had over 20,000—a fact that we are tempted to forget from the pernicious habit of matching the shot against the plate by giving its perforation through wrought iron as a standard. The energy per ton of metal in the plate might appear to furnish a sort of standard of comparison. This amounts to about 541 foot-tons per ton of metal for the Shoebury 1880 plate, and 654 foot-tons for the first blow at the Cammell plate at Spezia, the latter plate weighing 31.5 tons, and the former about 24 tons probably.

To get the full difference between probable smashing power and penetration, we have to compare a new-type gun with some piece fired at a comparatively low velocity. Take, for example, the 43-ton gun at the muzzle and the 100-ton muzzle-loading gun at 2,200 yards range.

Their perforations will be seen to be about the same, namely, 24 in. The 100-ton gun shot, however, has 31,200 foot-tons energy, while that of the 43-ton gun is only 19,800—that is, in larger proportion than 3 to 2—and this is with a velocity of 1,500 ft. against 2,000 ft.; from which it probably follows that the larger shot will break up less, and therefore impress more of its energy on the plate than the smaller one. Possibly, then, the smashing effect of the big shot will be nearly double that of the less one. Yet, if the official diagrams or rules be taken, the perforation is seen to be the same, and no other method for estimating the probable effect of shot is generally suggested. And this brings us on to the question of the best gun for perforation through soft armor and for racking.

Against wrought iron—that is, soft armor—as we have said, the new-type guns perform wonderfully well. There, for example, is the 43-ton gun which at the muzzle compares with the 100-ton gun at 2,200 yards in power to perforate. On the other hand, it may probably effect only something between one-half and two-thirds of what the latter can do in smashing. In the case of chilled iron Colonel Inglis has suggested that ruin might be effected by a very heavy shot, whose energy, when transmitted through the mass of shield, would be sufficient to destroy it wholesale, while smaller blows might be absorbed without much injury. When we know that every foreign iron fort that our ships might engage is made of chilled armor, and that hard armor is rapidly coming in on foreign ships, we see that very heavy guns have a great deal to recommend them; and very heavy guns carry us in the direction of very large ships, if they are to be sea-going. This is the direction in which we think things are again moving.

Without, however, jumping at such conclusions yet, we may surely plead a case for a thorough trial of hard armour in this country. With the exception of two miserable blocks of metal cast before chilled iron had ever been successfully applied to armor, we have never fired once at this class of shield in this country. If our guns ever engage with foreign forts they will fire at this class of armor only, for nothing else exists—at all events to any extent. A good programme, consisting of trials against chilled iron and steel, or hard steel-faced armor, ought to go far in enabling us to succeed in the following objects: First, to establish a basis on which to calculate the effect of fire against hard armor; secondly, to learn the relative value of guns, and make up our armaments accordingly; thirdly, to learn the effect of our present service of projectiles against hard armor; fourthly, to develop the best description of projectiles for this work. Sooner or later something of this kind must be done, and surely the sooner the better.

THE WORTHINGTON PUMPING ENGINE AT BUFFALO.

[An article on the trial of this engine appeared in our April number (p. 287). As the report on which the article was based was not accepted, and as the boilers used on that trial proved insufficient to supply the amount of steam guaranteed by the city, another trial was ordered by the Water Commissioners. In justice to the reputation of the engine, we now publish a report of this second trial. On this report the engine was promptly accepted.—Ed.]

To the Water Commissioners of the City of Buffalo, and Henry R. Worthington, of New York:

In accordance with the request of both parties to your contract, dated the 18th day of November, 1880, the experts appointed under that contract report, in addition to their formal report of the sixth instant, as follows:

The engine was run uniformly, for determining duty, from 10.30 A.M. to 10.30 P.M. of the twelfth of January, 1883. The number of gallons displaced in that time was 9,661,682.$\frac{321}{1000}$, or say 19,323,365 gallons per day of twenty-four hours. The contract required a delivery of fifteen million gallons of water in twenty-four hours of 77.6 per cent. only of the quantity actually displaced, or in other words the displacement was 28.8 per cent. greater than the required delivery.

The average pressure, above the delivery valves against which this quantity of water was pumped, was 74.90 pounds. The pressure required by the contract

was 70 pounds or 93.46 per cent. of that attained during the trial. In addition to this, the engine worked against a lift of nineteen feet from the surface of water in the pump well to the delivery valves.

The duty performed by the engine during the twelve hours of trial, without any allowance for wet steam supplied by the boilers, and to which the engine is entitled, was 70,331,274 pounds of water raised one foot high by one thousand pounds of steam. The contract required that seventy million pounds be raised one foot by this weight of steam.

The performance of duty, increased by the allowance to which the engine is entitled for extra wet steam, and decreased by the maximum slip of the pump, was above the contract requirement when taken on delivery, instead of displacement.

The duty performed by the engine as reckoned from the average of the steam cards, taken at intervals during the trial, and based upon the terminal pressure in the high pressure cylinder in the usual way, was 78,319,545 foot pounds or 11.72 per cent. above seventy millions. The duty based upon the cards at cut-off, which could not be so accurately determined, was a little over seventy-nine million foot pounds.

At the request of the Water Commissioners, an extra pressure was put upon the force main of the engine, after the trial for duty, and for nearly an hour the engine ran steadily, and pumped directly into the high service distribution against an artificial pressure of eighty pounds per square inch above the delivery valves, and held that pressure with remarkable regularity throughout the run. So far as could be seen, this pressure might be held indefinitely.

The engine is of excellent material and workmanship, and is larger than the contract requires. The net displacement per count is 972.09803 U. S. gallons or 129.9506 cubic feet. The average number of counts per minute during the trial was $13.\frac{8042}{10000}$. The number of plunger displacements in twelve hours was 39,756 and the total amount of shortages observed would be made up by the running of the engine about one-twenty-third of a second, a period of time much too small for notice.

The number of observations made during the time when forty shortages were observed was 1368, and if we assume that in the unobserved strokes the same ratio of shortages occurred, then the whole amount of shortages occurring in twelve hours, would be made up in running $1\frac{1}{4}$ seconds, an amount still too small for recognition in the records of the engine as taken by the observers.

J. Herbert Shedd,
John F. Ward.

Providence, R. I., April 19, 1883.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia. — Meeting, June 2, 1883. — Mr. Carl Hering read a short article on electrical units and formulæ, giving a list of the units and the relations existing between them; showing how these relations can be combined into formulæ containing any two or more of the units, for convenience of calculations. The units are: *ampire*, the unit of current strength; the *volt*, the unit of electromotive force, the *ohm*, the unit of resistance; the *coulomb*, the unit of quantity; the *farad*, the unit of capacity; the *volt-coulomb* (or *vomb*), the unit of work; and the *volt-ampire* (or *waht*), the unit of power. The relations are that an ampire is a volt divided by an ohm. An ampire is a coulomb per second. A farad is a coulomb divided by a volt. A volt-coulomb is a volt multiplied by a coulomb. A volt-ampire is a volt multiplied by an ampire.

As these relations contain no coefficient other than unity, they express the relations between any quantities measured in terms of those units as well as the units themselves.

Prof. L. M. Haupt exhibited a drawing of the Phœnixville Bridge which was built by Mr. Moncure Robinson, C. E. (Honorary Member of the Club), in 1836, for the Philadelphia and Reading Railroad, over the river Schuylkill. It is an instructive and enduring monument of successful construction of cut-stone masonry. There are four segmental arches 72 feet clear span and $16\frac{1}{2}$ feet rise. Radius of arch, $47\frac{1}{2}$ feet. Voussoirs 2 ft. 9 in. thick. One end abuts against a rocky bluff, whilst the other is supported by a heavy abutment with an earthen filling. It is believed to be one of the lightest and cheapest bridges of its kind in this country, having cost but $48,000.

The Secretary exhibited samples of Japanese paper which he had obtained through Mr. J. A. L. Waddell. Many Japanese papers are of excellent quality and could probably be used with great advantage in engineering practice. The method of manufacture is kept secret to some extent, as there is no patent law in Japan, but the paper is made of new material instead of old rags, as in America. The best qualities are made of silk; the others probably of bark fibre. The sheets exhibited are about $35'' \times 24\frac{1}{2}''$, and would cost but a few cents each to import.

ENGINEERING NOTES.

The New Tay Bridge.—A fair start has been made with the construction of the new Tay Bridge. Designed by Messrs. Barlow, C.E , London, the new girder bridge is to span the river about 30 yds. to the west of the ruined structure, no part of which is to be utilized. The piers, erected in pairs, will number 86, the distance between them varying from 66 to 245ft. —the long spans being in the middle of the river, where the bridge will have a headway of 77ft. above high water. This is 11ft. lower than the extreme height of the old bridge. One important difference between the old and the new bridges is, that whereas in the former the piers were of cast iron, in the latter most of the iron will be malleable, which of course, will add materially to its strength. On the north side the new bridge will begin at the bowstring girder of the old structure, which carried the railway over the road leading to Magdalen Green. This girder and its supports will be removed, new piers erected upon the same site, and a fresh girder provided. Counting from this point, the foundations of three new piers have been put in. Each of these piers has a coping of granite and atop of it will stand four cast-iron columns, 3ft. 6in. in diameter, and about 15ft. high, firmly held down by sixteen bolts, built several feet into the brickwork. The girders will rest on the top of these columns. The next three piers will be of skew pattern, and are designed so as to facilitate the extension of the Esplanade in a westward direction. These skew piers, which will be constructed from their base to the girders entirely of brick, will carry the bridge out beyond the low-water mark. For the foundations of all other piers, save four at the south side, massive iron cylinders will be used. In the shallow water off the north shore these will be of cast iron, and in the deeper water beyond of malleable iron. Between the cylinders which constitute the base of each pier an iron girder about 2ft. in depth will be laid; above this the pier will be continued in blue Staffordshire brick for 7ft.; and from this point a superstructure of malleable iron, braced with angle and T bars, will be carried up to meet the girders. This superstructure, beginning at the seventh pier from the Esplanade, will vary in height according to the gradient of the bridge, which averages 1 in 114ft. Blue brick is being used, as it is harder and better fitted to resist the action of the water than the common red variety. The girders will be of the ordinary lattice description, and of the same depth as those on the old bridge. The floor of the bridge will be wholly of iron. The rails will be laid on the top of the girders, except at the part of the structure opposite the gap. There, they will be fixed upon the bottom of the girders. It is intended to erect wind screens on the bridge, but what shape these may take will be a matter for further consideration. In connection with the works at the Forth Bridge experiments with screens are being conducted to determine the velocity of the wind; and the engineers will probably be largely guided by the results there obtained. On the south side of the Tay, where the river has high banks of rock, the bridge is carried out to the deep water on four brick arches, supported by brick piers rising from the rock below the bed of the river. These piers are protected by cutwaters with granite copes—the current at this side being often very strong. The abutments and wing walls of the first of these arches have been built. The officials responsible for the construction of the bridge are Mr. Kelsey, M.I.C.E., the resident engineer for Messrs. Barlow; and Mr. W. Inglis, C.E., the resident engineer for the contractors. The bridge, which is to cost about three-quarters of a million sterling, is expected to be finished about the end of 1885.

Proposed New Suez Canal.—A preliminary meeting was held at Cannon Street Hotel, on the 10th May, to consider the possibility of constructing a second Suez Canal, at which there were present representatives from the Peninsular and Oriental Company, the British India Company, the Ducal Line, the Orient Line, the Anchor Line, the Harrison Line, the Clan Line, the Eastern and Eastern Extension Telegraph Company, the Shire Line, and the Glen Line. It is estimated that the tonnage passing through the Canal represented by the gentlemen who were present in the room was not less than 3,000,000 tons.

In the course of the proceedings, it was stated by Mr. Stephen Ralli that even if the Suez Canal was not already inadequate to the requirements of the trade, it would soon become so. The trade between the far East and Europe had been of late years increasing at a very rapid and unexpected rate. There was, moreover, every probability that it would continue increasing. Taking it for granted that the concession given to M. de Lesseps prevented for 99 years from the opening of the present canal, the constitution of another company having for its object the making of a new canal through the Isthmus of Suez, it must be borne in mind that treaties between nations, although in most cases made in perpetuity, were altered as soon as the circumstances which brought them about were altered. Mr. Ralli thought that the proper steps to take must be to try first to come to terms with the present company of the Suez Canal, asking from them what was wanted by the requirements of the rapidly increasing trade between the far East and Europe. He thought what should be asked could be summed up in the following points:—(1.) In consideration that the amount of British shipping passing through the Suez Canal was equal to four-fifths of the whole, and that the trade was principally carried on between India (which is a British dependency) and Europe, they could fairly ask from the French company that its Board should be reconstituted on a different basis, and that, instead of having three English directors against twenty-one French, the number of English directors should be equal to that of the French. (2.) That the annual meeting of the company should be held alternately in Paris and in London. (3.) That either the present canal should be very much widened, or another canal made. If a large widening of the present canal would cost nearly as much as the con-

struction of a second, the construction of a second would be preferable, as it would allow steamers to come up through one canal and go down through the other.

Resolutions were unanimously carried in favor of another canal, and an executive committee was appointed to take the necessary steps in furtherance of this object.

The first two resolutions were as follows; "That having regard to the great increase of traffic, to the insufficiency of the present canal, even for the present traffic, and to its further certain increase, the time has arrived when arrangements should be completed for making a second canal;" and "That a committee be appointed to examine in detail the best course for such additional canal to take through Egypt, with authority to employ whatever professional assistance may be necessary for that purpose." A further résolution had reference to the appointment of an executive committee, and to the immediate formation of a guarantee fund to cover preliminary expenses. Among the gentlemen who were selected to act on this committee were Mr. J. Laing (President of the Chamber of Shipping of the United Kingdom), who was appointed chairman, Mr. Thomas Sutherland (Chairman of the Peninsular and Oriental Steam Navigation Company), Mr. John Glover, Mr. Pender, M.P. (Chairman of the Eastern and Eastern Extension Telegraph Companies), and Sir George Elliot, M.P.

It was stated in the discussion, that those who are most competent to judge were of opinion that steamships in the Indian trade were increasing at such a rate, that the canal traffic was likely to exceed ten million tons before a second canal could be built, and it was contended, having regard to the serious inconveniences which are experienced with the present traffic, that the conduct of the business will become almost impossible when it grows to ten million tons, unless there be a second canal by that time. It was argued that if the present traffic is paying the shareholders of the existing Suez Canal from 15 to 20 per cent., another canal would pay, even if the dues were lowered to five francs a ton.

THE NEW BRIDGE AT SPANDAU, NEAR BERLIN.—According to the *Central Blatt fur Bauverwaltung*, the above bridge over the Havel is about 500ft. in length, and is distinguished from other pontoon bridges by the fact that the expensive chains and cables usually employed are here replaced by wire-ropes for the purpose of resisting the pressure of the wind, only a small portion of the bridge rests on piles, while eighteen pontoons support the footway. This portion of the bridge is placed high, and is so arranged that boats can pass at two places, and in the middle vessels with upright masts; two pontoons being movable. Special precautions have been adopted to allow of the bridge being used without inconvenience at any height of the water in the river. The cost is said to have been £1,050.

LAKE WINNIPEG.—Recent exploration and levelling shows that the surmise of General G. K. Warren to the effect that Lake Winnipeg once discharged itself into the Mississippi on the south instead of Hudson's Bay on the east, is correct. Mr. J. D. Dana, the well known geologist, in a paper on the *American Journal of Science*, fully discusses the evidence and shows that the change was due not to a barrier of ice or earth, but to a change of level over a wide area. The geological facts he adduces point to the following succession of events: the lake deposits being underlaid by unstratified drift show that before the great lake existed, a glacier had moved southward over that region and deposited moranic material. The high level prairie on either side of the lake region and of the Minnesota valley is made up of this unstratified drift; but the generally level surface in the part next the lake valley and the stratification in the material show that the floods from the melting ice levelled it. This period of floods was followed by the era of the great lake, that is to say of quiet waters and gentle deposits, with a slow discharge over the Lake Traverse region, which appears to have been brought about by a diminution in the slope of the general surface, which was part of a great change of slope which went on, as General Warren has explained, until the land was reduced to its present inclination and the stream to its present courses.

RAILWAY NOTES.

RESISTANCE ON RAILWAY CURVES.—At the meeting of the Institution of Civil Engineers on April 24, a paper was read on "Resistance on Railway Curves as an Element of Danger," by Mr. John Mackenzie, Assoc. M. I. C. E. It was stated by the author that when a six-wheeled engine with parallel axles was running round a curve, the tendency which the outer leading wheel-flange had to mount the rail was evidently caused by its adhesion to the side or rounded corner of the rail, and that this adhesion was the result of a side pressure which, at low speeds, was principally caused by the resistance the treads of the wheels offered to the sliding motion that took place in going round a curve. He contended that this side pressure increased with increased adhesion of the treads of the wheels to the rails, and that the adhesion of the flange itself to the rail also increased with the increased ratio of adhesion, so that the tendency of the flange to rise increased in something like the duplicate ratio of the fraction representing the coefficient of adhesion. As the point of contact between the flange and the rail was in advance of the center of the axle, the motion of the flange at that point was downwards, imparting a downward pressure to the rail, and an upward pressure to the wheel, so that when the flange adhered to the rail the wheel rose. Thus the pressure which would cause the flange to mount the rail was not that which, with the wheel at rest, would force it over the rail in opposition to friction as well as to gravitation; but the very much smaller pressure which, when the wheel was at rest and the tread raised slightly above the rail, would cause friction

sufficient to prevent its falling into its place again. It had been found by actual experiment, that the adhesion between wheels and wet rails with sand sometimes rose above 40 per cent. of the weight; and it might be found, by calculation, that with this proportion of adhesion, the side pressure on the flange of the outer leading wheel of many six-wheeled engines of not unusual proportions might, under certain circumstances, be so great as to cause the flange to adhere and mount the rail; and that, as regarded running off the rails, six-wheeled engines generally had a very narrow margin of safety.

Some interesting statistics have been published relating to recent railway extension in India. At the end of the year 1882-3 there were open for traffic 10,251 miles of railway, and in course of construction 2,332 miles. There has been during the year an addition of 290 miles of completed line, and an increase of the railways sanctioned or actually begun of 1,030 miles. This increase indicates vast benefits conferred on India. In the year 1860 the Indian railways carried under four millions of passengers; in 1881 they carried over fifty-two millions. In 1860 the merchandise carried was 632,613 tons; in 1881 it had risen to 11,637,000 tons. The traffic receipts in the earlier year were £586,000; in the later they were £13,726,000. These figures, says the *Times*, are remarkable in themselves, but their full significance may very easily be missed, unless we take the trouble to picture to ourselves what was the condition of India in respect of intercommunication before English capital and enterprise provided railways. In India there were practically no means of intercommunication, except in the vicinity of navigable rivers, until we provided them. No Roman conqueror had bequeathed to his abandoned provinces his all but imperishable roads, nor had any vigorous or progressive race constructed highways for itself. Thus, it happens that 10,000 miles of railway in India have a significance which no conceivable mileage could have in England. More lines are, of course, required, since each new one that is opened actually creates a demand for more.

IRON AND STEEL NOTES.

Magnetization of Iron and Steel by Breaking.—At a recent meeting of the Society of Physical and Natural Sciences, Karlshruhe, M. Bissinger made a communication on the magnetization of bars of steel and iron when broken on the machine serving to test them. The phenomenon is not due to elongation of the bar but to the actual breakage; and both parts are converted into two magnets of sensibly equal power. The shock and trembling of the metal on breaking is probably the cause of magnetization, and here we are reminded of Professor Hughes' recent experiments. In the testing machine the bars are placed vertically, and the south pole is formed at their upper part. The different iron objects near the machine at the moment of rupture and vibration are also magnetized, but to a less degree.

In an article on the use of new iron, and the effect on it of the use of scrap for founding car wheels, the *Railroad Gazette* says: "The diminution of the silicon increases the amount of combined carbon, and consequently, up to a certain point, if the iron had considerable silicon to start with—i. e., before repeated cupola meltings—increases the strength of the metal. At the same time the increase in the amount of foreign substances, such as slag and oxides, continually weakens the metal by interfering with its continuity. The phenomena resulting from successive remeltings of the same iron can be almost entirely explained in this way. Starting with an iron pretty high in silicon, and consequently low combined carbon and not much strength, the first remelting diminishes the silicon, increasing thereby the combined carbon, and consequently the strength, the increase of foreign substances, slag and oxides not being sufficient to counterbalance the increase in strength due to increase in combined carbon. Each successive remelting does the same thing, until finally the amount of silicon has become so small and of the foreign substances so large that the metal has reached its maximum of strength, and each subsequent remelting diminishes this valuable property. It is possible that the amount of slag and oxides may reach a maximum before the maximum of strength is obtained, since, if at each melting the metal is allowed to stand quiet in the molten condition for a period of time before casting, a portion at least of these foreign substances rises to the top and is removed. In this case, the ultimate diminution in strength arises from the diminution of the silicon, as has already been explained. In the case of car wheels, the number of remeltings that the metal can endure without too great injury is undoubtedly small—perhaps none at all."

The Iron and Steel Institute.—The concluding sitting of the Iron and Steel Institute's annual spring meeting was held on May the 11th, in the Hall of the Institution of Civil Engineers, Westminster, Mr. B. Samuelson, M. P., presiding.

Mr. J. E. Stead, of Middlesbrough, read a paper on "A New Method for the Estimation of Minute Quantities of Carbon, and a New Form of Chronometer." The author said it was impossible to determine with accuracy minute quantities of carbon by the ordinary color method, owing to the color of the nitrate of iron present in the material. Investigations on the coloring matter produced by the action of dilute nitric acid upon white iron and steel had shown that it possessed the property of solubility in potash and soda solutions, and that the alkaline solution had about two and a half times the depth of color possessed by the acid solution. That being so, it was clear that the color matter might readily be separated from the iron, and obtained in an alkaline solution, by simply adding an excess of sodic hydrate to the nitric acid solution of iron, and that the color solution thus obtained might be used as a means of determining the amount of carbon, even when only so small a quantity as 0.03 was present. The paper then went at

length into the method at present in use, the writer stating that he was engaged upon investigations which, he hoped, would throw some light on the true constitution of hard and soft steel, and he believed he would soon be able to lay the results before the Institute. The second paper went into the subject of a new form of chronometer, which he had used in the process described above, and which, he said, had proved very efficient, and had the merit of being extremely simple and easily constructed.

In the next paper, Mr. W. S. Sutherland, of Birmingham, described a process of utilizing gaseous fuel in the manufacture of iron. Whilst fitting on a feed valve to a locomotive about 1866, the idea struck him that it would be advantageous to weld the seams of boilers instead of riveting them; and he set about to consider how best to obtain the requisite heat and apply it economically and of uniform quality and concentration upon the parts to be welded. The required conditions, he found, were best fulfilled by the gas produced from a modification of Story's American water-gas producer. The gas is no more poisonous than coal gas, and the advantages claimed for it are its superior heating power, cheapness, and simplicity of application. It may be produced at a cost of 3d. per 1,000 ft., after paying for coal, labor and plant, and a net profit of from 2s. to 3s. per ton can be made from the residuals. By mixing the gas and air thoroughly a powerful concentrated flame is secured, which, when it contains an excess of gas, has no tendency to oxidize the iron, and gives a soft mellow heat. The effects of explosions of mixed gas and air are prevented by simple means, and the method has been at constant work for six years without accident. The system, causing air and gas to mix and diffuse more perfectly, produces a higher temperature. The full effect of the heat can be produced directly it enters the boiler, so that complete combustion is secured at once. The first essential is that there should be an excess of gas in the mixture. The slightest excess of air, of fully combined carbonic acid or steam, infallibly burns the iron, makes it rotten, and the weld becomes a bad one. The experience of the author led him to get as much carbonic oxide as possible in his gas, and as little hydrogen and oxygen. If the process of making steel in the Bessemer process could be stopped just when the carbon becomes carbonic oxide and remained in the metal, a soft ductile metal just like wrought iron might be produced without having to add any speigel. Accepting Professor Graham's definition that steel is iron containing unoxidized carbon, and wrought iron is iron in which carbonic oxide is occluded, the author drew the conclusion that to produce a good, true wrought iron which would weld well, and in all particulars take the place of the best puddled iron, it would only be necessary to use the Bessemer converter, and instead of blowing raw air into it, blow Siemens' gas, which contains an excess of carbonic oxide. This process would be simply to produce wrought iron with all its good qualities increased, and certain dynamic considerations make it probable that this process would greatly facilitate the elimination of sulphur and phosphorus.

Taking the consumption of coke in the production of pig iron in the United Kingdom in 1882, at 23 cwt. per ton of iron made, it is estimated that Cleveland used 3,091,947 tons, West Cumberland used 1,151,358 tons, South Wales used 1,015,801 tons, North Wales used 56,020 tons, South Staffordshire used 458,209 tons, North Staffordshire used 364,684 tons, Lincolnshire used 231,795 tons, Lancashire used 900,150 tons, Northamptonshire used 220,932 tons, West and South Yorkshire used 321,141 tons, Derbyshire and Notts used 512,595 tons, Shropshire used 92,546 tons, Gloucestershire, Wiltshire, etc., used 55,200 tons; total of coke, 8,472,378 tons, representing about 14,120,627 tons of coal, to which must be added coal consumed in Scotland, say, 2,300,000 tons, or a total of coal, 16,420,627 tons. The British iron trade report, however, adds that it is probable that the average yield of the United Kingdom will be nearer 56 to 57 per cent. of coke per 100 of coal, 60 per cent. being, indeed, about the best average result that is obtained in the coke manufacture. It is probable also that the average consumption of coke per ton of pig made will, in the country generally, be nearer 25 than 23 cwt. The foregoing figures are, therefore, subject to these two modifications.

ORDNANCE AND NAVAL.

Marine Signalling Apparatus.—Sailors have in the "International Signal Book" a most complete code for the interchange of signals, but as the signs are given by flags the system is useless in the dark, or in fogs. To remedy this defect, Mr. C. Vreede, an officer in the Royal Dutch Navy, has invented an apparatus, using signals based upon the same system, whereby information may be transmitted to a distance by means of light flashes, or the blasts of a whistle or horn. According to his invention, a metal ring is caused to rotate at a uniform circumferential speed of one inch in five seconds by clockwork, and to carry removable cams or toothed projections, which pass under a lever, and so effect the flashing of a lamp or the sounding of a steam whistle. There are eighteen signals in the code, which, in Mr. Vreede's system, are represented by intervals and flashes. The intervals are all the same length, but the flashes are of various lengths of 4, 8, 12, and 16 seconds, thus the signal representing C is given by 4 seconds flash, 4 seconds interval, and 4 seconds flash again; Q is given by 12 seconds flash, 4 seconds intervals, and 8 seconds flash, and so on. The message is first arranged on the metal ring, which is put into the apparatus, and the information is then automatically signalled. A record is obtained by the observer by aid of an apparatus, in which a paper tape is kept moving at a uniform rate. When he sees the flash he presses upon a pencil, and maintains the pressure as long as the light is visible, and when it disappears he removes his hand. The

result is that the tape bears a record in strokes of various lengths, separated by intervals, and these can, if the observer have no skill in reading the signals, be interpreted afterwards by comparison with the instructions of the code.

THE NEW EXPERIMENTAL FIELD GUNS.—The two breech-loading field guns recently brought to the notice of H.R.H. the Duke of Cambridge are shortly to be sent to Dartmoor for trial under conditions as nearly as possible approaching those of service. Both guns and carriages are remarkable. The light field gun, which is intended for both horse artillery and field batteries, weighs 7 cwt.; it fires a 12lb. projectile from a 3in. bore, 27½ calibers long, rifled with ten grooves. The heavier gun, which replaces the old 16-pounder muzzle-loader, fires a 22lb. projectile from a 3.5in. bore 28 calibers in length. The power of this gun is enormously beyond that of its predecessor. The old 16-pounder muzzle-loader had an initial velocity of 1355ft. Its projectile weighed 16lb. 1½ oz. This gave nearly 205 foot-tons muzzle energy. The new 22-pounder gun fires its projectile with a velocity of 1750ft. The energy is, therefore, 467 foot-tons—that is to say, that this new heavy field battery gun, while no greater in weight than our old one, fires a projectile wtth more than double the stored-up work. In fact, this gun might be fired at the earlier made armor-plated ships, if necessary, for its power of perforation is about 7in. of wrought iron. The lighter gun, that is the 12-pounder, has an initial velocity of 1725ft. The gun being capable of performing this work, we naturally are brought to consider the question of the carriage. This is a modified form of Albini carriage, with a powerful brake. The action of the buffer employed in the Albini construction is rather calculated to save the carriage from fracture than to diminish its recoil. The latter unchecked is naturally enormous, a slow-burning charge and other causes favoring this. Without any brake the recoil of one gun fired before the Duke was about 50ft. The action of the brake brought this down to 10ft. Some experimental corrugated steel ammunition boxes are also to be tried at Dartmoor, with the ammunition in trays which draw out. The steel boxes are not, we think likely to do well, unless certain obvious objections can be got over. Difficulty of repair may be met of course by the fact that an ammunition box may be exchanged very easily. The noise we think must be intolerable. However, Dartmoor is the very place to try all these matters practically. It is very important that the question of our field guns should be decided without further delay. We have a certain number of 13-pounder guns in use, but most of our field batteries still have the old 9-pounder and 16-pounder muzzle-loading guns. These feeble and ridiculous pieces were always discreditable. To keep them longer than necessary would be a disgrace. We may add, by the way, that in these new field guns, and in all future breech-loaders, the steel cup obturator is replaced by Dubange's "asbestos pad." On this system a mushroom head has behind it on a spindle an annular pad, formed of asbestos chopped up small and brought into shape by hydraulic pressure. This is held between two tin plates with copper washers next to the asbestos. The obturation is wonderfully perfect. It is only necessary at considerable intervals of time to moisten or change the pad.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

JOURNAL OF THE MILITARY SERVICE INSTITUTION OF THE UNITED STATES. Vol. IV., No. XIV.

THE ECCENTRIC. Published by students of Stevens Institute.

PROCEEDINGS of the Institution of Civil Engineers, (from Mr. James Forrest, Sec.,) the following papers:

Economical River Training in India. By Carlton Fowell Tufnell, A. M. I. C. E.

Mild steel for fireboxes of locomotives. By John Fernie, M. I. C. E.

Repairing slipways for ships. By Thomas Bell Lightfoot, M. I. C. E., and John Thompson.

The Antwerp Water-works. By William Anderson, M. I. C. E.

The Iquique Railway. By Henry Sadleir Ridings, M. I. C. E.

ABSTRACT OF PROCEEDINGS OF THE SOCIETY OF ARTS.

GAS AS AN ILLUMINATING AGENT COMPARED WITH ELECTRICITY. By William Sugg, A. I. C. E.

PRACTICAL CARPENTRY. By Fred. T. Hodgson, New York: The Industrial Publication Co.

This convenient little book is designed for the workman who is inclined to study up the mysteries of mitering and joinery. The author has supposed the student but little acquainted with geometry, and has therefore commenced with practical geometrical problems.

The drawing of arched doors and windows then follows, and immediately thereafter roof designing is introduced, requiring a knowledge of descriptive geometry, for a knowledge of which the reader is presumably dependent on other books. The illustrations are numerous and good.

MANUAL OF TAXIDERMY. By C. J. Maynard, Boston: S. E. Cassino & Co.

The preparation of birds, mammals and reptiles for collections is the subject of this little manual.

As the directions are quite specific, and aided by an abundance of illustrations, the book will probably satisfy a demand that has lately come from students of zoology for a suitable guide to the proper preservation of specimens.

L'ELECTRICITE COMME FORCE MOTRICE. Par Th. du Moncel, et M. Frank Geraldy, Paris: Librairie Hachette & Co.

This last contribution to the literature of electrical science will be found to be an excellent compend of historical facts, as well as of the scientific principles successively developed in

the attempts to employ electricity as a motive power.

The work is divided quite properly into two distinct parts, treating of the first phase and second phase respectively of the development of the modern electro-motor.

In the first part are grouped the historic motors of Jacobi, Elias, Froment, Page, Wheatstone, Davidson and Pacinotti, each carefully described and illustrated. Then a distinct class is described as "electro-motors founded upon dynamic reactions of currents," including the engines of Bourbouze and Du Moncel, and the tilt hammer of Desprez.

The electro-motors depending upon magnetic attraction are quite numerous, and are here conveniently divided into "oscillating" and "rotary" engines.

The locomotives of Militzer, of Bellet and of Gaiffe form the subject of the next section. Then come special applications of electricity as a source of dynamic energy, including Edison's pen, Lacour's phonic wheel and Trouvé's Gyroscope.

The second part devoted to the second phase of development begins with an elucidation of the principles of *induction* and a definition of the *magnetic field.* The following order of presentation of topics is observed:

Reversible Machines, General Principles of the New Motors, The Small Motors, Applications of the Small Motors, First Attempts at Transportation of Force, First Traction Engines, The Motors at the Exhibition of 1881, Later Applications, Distribution of Electricity.

Although the descriptions of special devices are numerous, they are not given at the expense of an elucidation of the fundamental principle of each and every example. So compact an essay combining the philosophical and historical accounts of the development of a scientific principle is rarely seen.

FORMULAIRE PRATIQUE DE L'ELECTRICIEN. Par E. Hospitalier, Paris: G. Masson.

This is intended as the first of a series of annuals similar to engineers' table books which appear yearly, only slightly changed from their previous editions, but presumably brought up to the times.

The following abstract of the author's preface will explain the scope of the work:

"The title of this book briefly expresses its character. It is designed to furnish to professional electricians as well as to amateurs, those formulas for which they would have to search through a large number of works, and the fundamental notions upon which are based the different electrical processes.

"The first part contains definitions, principles and general laws. The second part contains the units of measure based upon the C. G. S. system, and gives also the numerical relations to the other systems in use.

"In the third part are described the machines and methods employed constantly by electricians. Algebraic formulas, trigonometrical tables, barometric, thermometric and specific gravity tables are also added.

"The fourth part presents a compendium of the applications of electricity to metallurgy, telegraphy, telephony and transmission of force. The production of electricity receives also its share of attention, and primary and secondary batteries, thermopiles and mechanical generators are all discussed."

It is clearly a pocket table-book designed to serve the daily wants of the practical electrician.

The matter as well as the size of the book are well adapted for his convenience.

THE MATERIALS OF ENGINEERING, IN THREE PARTS: PART II. IRON AND STEEL. By Robert H. Thurston, A.M., C.E., New York: John Wiley & Sons.

To engineers especially the knowledge of the nature of materials is of no less importance than knowing how to use them. No substances employed for engineering purposes rank higher in importance than iron and steel.

The work before us, containing as it does a summary of the latest and most valuable information about these metals, prepared by a prominent engineer, will be everywhere considered a valuable contribution to technical literature.

How exhaustive is the treatment of this extensive subject by Prof. Thurston may be inferred from the following analysis of its contents:

Chapter I. Qualities of the metals including alloys, and including also such physical properties as specific heat, fusibility, malleability, etc.

Chapter II. History and materials of metallurgical work.

Chapter III. Historical sketch of iron manufacture, including the later conclusions respecting the earlier work of the Greeks, Egyptians and Hindoos.

Chapter IV. The ores of iron classified and described.

Chapter V. Reduction of the ores and complete description of the manufacture of cast iron.

Chapter VI. Manufacture of wrought iron, including descriptions of the puddling and rolling processes.

Chapter VII. Manufacture of steel, direct process; crucible steel; Siemens' process; Bessemer process; chrome steel.

Chapter VIII. Chemical and Physical properties of iron and steel, including also the theory of hardening and tempering.

Chapter IX. Strength of iron and steel, including descriptions of testing machines and methods of applying tests.

Chapter X. Effect of temperature and time on resistance; flow of metal; Wöhler's law.

Chapter XI. Specifications; tests; inspection; this refers to preparation for use in structures of various kinds, and is of great importance to engineers about to employ iron or steel of a new brand for an important purpose.

In view of the facts that this treatise contains such an array of the latest and best authenticated information regarding these most important materials, it will doubtless be regarded by practical engineers as an indispensable reference book.

MISCELLANEOUS.

FRENCH ACADEMY PRIZES.—The French Academy of Sciences have recently published a list of the prizes offered by them for essays on scientific subjects during this year, and until 1886. In applied mechanics the Fourneyron prize will be given for the best "study, both theoretical and experimental, of the different methods of transporting force to a distance." The papers must be lodged before the 1st of June next. A grand prize will be awarded in 1844 for a mathematical solution of the problem "to perfect in some important point the theory of the application of electricity to the transmission of power." The prize will consist of a medal valued at 3000 francs. The memoirs must be submitted to the secretary of the Academy before June 1, 1884, and should be anonymous, but accompanied by a sealed envelope with the real name and address of the author. The Bordin prize, which was not awarded this year, is carried on to 1885, and memoirs must be lodged before June 1 of that year. The subject is a "research into the origin of electricity in the atmosphere, and the causes of the great development of electric phenomena in storm-clouds." The prize is a medal worth 3000 francs.

BASIC FURNACE LININGS.—Kutscha, Oelwein & Mertens recommend for furnace linings the use of the mineral agalmatolite, occurring at Dilln, near Schemnitz, in Hungary. Its composition is:—Silicic acid, 30.40; alumina, 52.68; iron oxide, 0.80; manganese oxide, 0.30; lime, 0.89; magnesia, 0.39; sulphuric acid, 0.80; water, 11.88; alkalies, 1.50; total, 99.64. By mixing two parts of burnt agalmatolite with one of the raw mineral and moistening the mass with water, the mixture may be pressed into briquettes which on burning at a white heat become hard and adhesive, and do not shrink. For the preparation of basic linings it is proposed to add to lime or dolomite a flux in such proportion that the mixture after twelve hours' burning at a white heat forms a slag which is pulverized and worked up with suitable binding agents. For the manufacture of such basic refractory masses, dolomite of the following composition is used:—Silicic acid, 0.7; alumina, 0.5; iron oxide, 0.6; lime, 31.5; magnesia, 20.0; carbonic acid, 46.7; mixed with 12 per cent. of talc of the composition:—Silicic acid, 62.0; magnesia, 31.0: iron oxide, 2.0; water, 5.0. The mixture is formed into bricks, heated for twelve hours at a white heat, pulverized, treated with from 5 to 8 per cent. of tar, from 3 to 5 per cent. of pitch, or from 5 to 10 per cent. of resin, pressed whilst hot in heated moulds and burnt at a high temperature. Bollinger recommends the use of a mixture of asbestos, chrysolite and magnesium chloride for the preparation of refractory basic linings.

ARTIFICIAL LEATHER FROM LEATHER WASTE.—An artificial leather, consisting of leather waste mixed with from five to ten per cent. of sinew, and pressed in sheets like ordinary card-board, has recently been made in Germany. Both materials are prepared separately; the leather pieces are washed, cut, boiled in alkaline lye, torn, neutralized with hydrochloric acid, and finally carefully washed once more to remove all traces of acid; the sinews are treated similarly, but steamed in an acid bath until they begin to assume the consistence of glue. The materials are then mixed, pressed into sheets, moistened on both sides with a concentrated solution of alum, and the upper surface is finally treated with a thin coat of a solution of caoutchouc in carbon bisulphide to increase resemblance to leather.

M. NEYRENEUF has communicated to the French Academy of Sciences the results of experiments made by him on the intensity of sonorous vibrations transmitted through different gases. He placed a sound source on one side of the gaseous chamber, and a sensitive flame on the other, and observed the action of the flame. The gases tested thus far are air, carbonic oxide, lighting gas, and carbonic acid. Air aud carbontc oxide have a transmissive power about equal. Air and lighting gas give very unequal results, probably because of the hydrogen in the latter. The results vary much with the chemical constitution of the coal gas employed. The transmissive power through carbonic acid is much greater than through air. The results show that Hauksbee's law is not correct, and the author is continuing his researches with a view of throwing further light on the dynamical theory of gases.

THE RADIATION OF SILVER IN SOLIDIFYING.—At the international congress of electricians in 1881, M. J. Violle proposed, and M. Dumas, the famous chemist, seconded, the use of an absolute unit of light consisting of the radiation emitted by a square centimeter of platinum in melting. At the instance of M. Cochery, the French Minister of Posts and Telegraphs, an investigation of the subject has been begun by M. Violle, and his first experiments have led him to some observations on the radiation of silver in solidifying. A bath of pure melted silver was placed under a thermo-electric pile connected with a mirror galvanometer. The radiation from the bath fell normally on the battery through an aperture in a double-walled screen kept cool by circulating water. As the bath cooled the pile showed that the radiation slowly decreased until the instant just before solidifying, when there was a slight increase, preceding the final decrease after solidification.

A LACQUER of great elasticity, perfectly supple and not liable to peal off, is made in the following manner:—About 120lb. of oil varnish is heated in one vessel, and 33lb. of quicklime is put into 22lb. of water in another. As soon as the lime causes an effervesence, 55lb. of melted india-rubber are added. This mixture is stirred and then poured into the vessel of hot varnish. The whole is then stirred so as to be thoroughly mixed, then strained and allowed to cool, when it has the appearance of lead. When required for use it is thinned with the necessary quantity of varnish and applied with a brush, hot or cold, preferably the former. This lacquer is useful for wood or iron and for walls; it will also render waterproof cloth, paper, &c.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXVI.—AUGUST, 1883.—VOL. XXIX.

THEORY OF RAILWAY TURNOUTS.

By J. R. STEPHENS.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

1. IN *Trautwine's Pocket Book* (page 401, edition of 1883) it is stated that the *modern practice* of laying out turnouts is to curve the switch rails so as to form part of the turnout curve.

The object of this article is to show that the *modern theory* of locating turnouts depends on a single, simple and exact proposition. (See Art. 4.)

2. Fig. 1 represents a turnout from a straight track. A J and A′J′ are the two rails of the main line; H and H′ are the points from which the switch rails BH and B′H′ spring, when actuated by the switch lever at S. The distance B*b* is called the throw of switch, and (for the ordinary stub switch here considered) is usually 5 ins. for a $4'8\frac{1}{2}''$ gauge.

It should be borne in mind that the switch rail, HB, when thrown over to the turnout, is not straight from H to *b*, but curved somewhat in the form of an elastic curve. For purposes of calculation, however, it is assumed to be a portion of a simple circular curve extending from H to F. In order to satisfy this condition, B*b*, or the throw of switch, must equal the tangent offset to the curve HF, corresponding to a length of tangent HB; and, as B*b* and the degree of curve HF are usually fixed, HB must be calculated accordingly. The difference HJ, between the length of switch rail and the total length of the rail, is spiked down to the ties.

At F, the intersection of the turnout curve with the main track, a frog is placed.

Frogs are usually designated by numbers which express the ratio $\frac{FC}{AB}$, Fig. 2.

It is sometimes difficult to determine the point F exactly, owing to the rounding off shown at S. In such cases the frog number may be found from the ratio $\frac{CG}{AB+DE}$; for, by similar triangles,

$$FG : FC :: DE : AB$$

$$\therefore \quad FG+FC : FC :: DE+AB : AB$$

or
$$\frac{CG}{AB+DE}=\frac{FC}{AB} \qquad \text{Q.E.D.}$$

Another common way of determining the frog number, is to slide a rule along the frog until it measures any whole number of inches across, (say 4″, as shown in Fig. 2). Mark the place and then slide the rule along until the breadth becomes 5 inches. The distance *d*, between the two cross lines in inches, will be the number of the frog.

If the frog number be known the exact point F or intersection of the gauge sides of the rails may be readily found by determining the point p, Fig. 2, when the distance across is exactly 1 inch. Then measure from this point to F as many inches as there are units in the number of the frog.

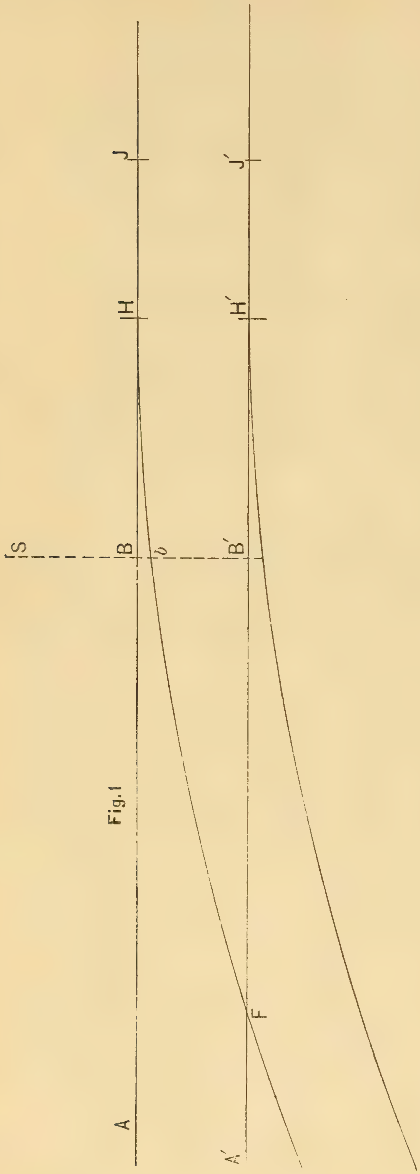


Fig. 1

To find the frog angle from the number of the frog.

In the triangle AFC, Fig. 2,

$$\frac{\text{FC}}{\text{AC}}=\cot.\tfrac{1}{2}\,\text{AFB}.$$

Put n=number of frog, f=frog angle then

$$n=\frac{\text{FC}}{\text{AB}}=\frac{\text{FC}}{2\text{AC}}=\tfrac{1}{2}\cot.\tfrac{1}{2}f\ . \quad (1.)$$

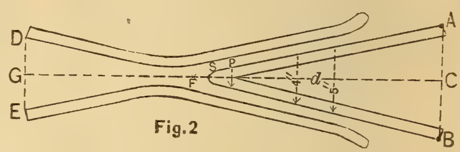


Fig. 2

3. Theorem I.—*The angle included between lines drawn from the point of frog to the heels of the two switch rails, is equal to one-half the frog angle,* or HFH' in figures 3, 4 and 5 equals $\frac{1}{2}f$.

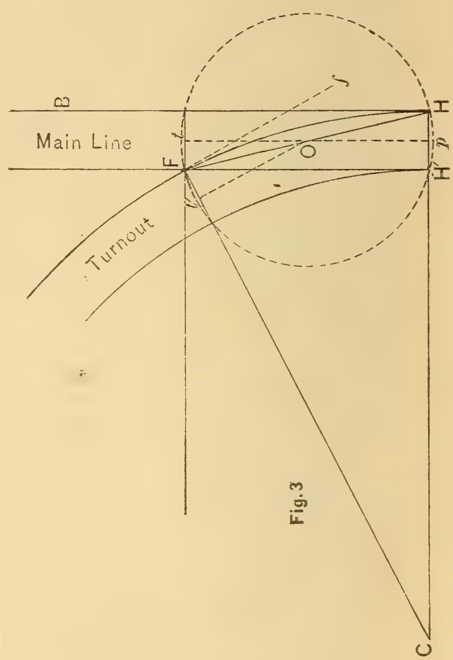


Fig. 3

Case 1.—*Turnout from a tangent.*

In Fig. 3 the frog angle fFH=FCH, since Ff is perpendicular to FC and FH' to CH.

Also H'FH = FHB = the deflection angle of turnout curve, and hence is $\frac{1}{2}$ the intersection angle FCH.

$\therefore$ H'FH=$\frac{1}{2}$ H'Ff Q. E. D.

Case 2. *Turnout from inside of curve.* Fig. 4.

In Fig. 4, C and C′ are the centers of the main line and turnout respectively, CFC′ = frog ang. $fFf,'$ since CF and C′F are perp. to Ff and $Ff.'$

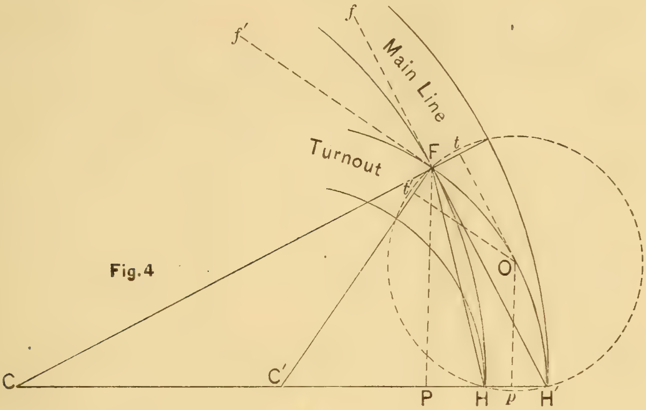


Fig. 4

Let fall the perp. FP upon CC′ then

$$
\begin{aligned}
& H'FP = \tfrac{1}{2}\, FC'H \\
& H\,FP = \tfrac{1}{2}\, FC\,H \\
\text{Subtract}\quad & \overline{HFH' = \tfrac{1}{2}\, CFC'} \qquad \text{Q. E. D.}
\end{aligned}
$$

Case 3. *Turnout from the outside of a curve.* Fig. 5.

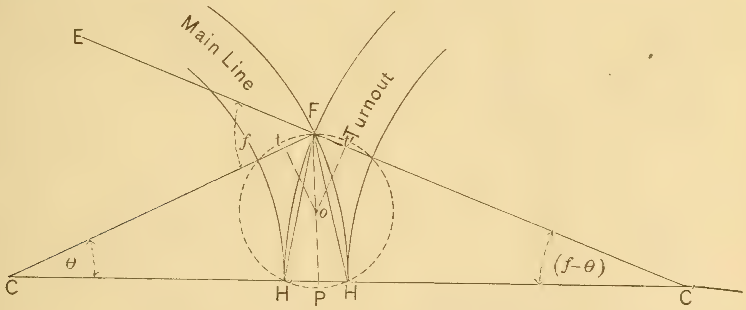


Fig. 5

Let fall the perp. Fp upon CC′; then

$$
\begin{aligned}
& H'FP = \tfrac{1}{2}\, FC'H' \\
& H\,FP = \tfrac{1}{2}\, FCH \\
\text{adding}\quad & \overline{HFH' = \tfrac{1}{2}\, EFC} \qquad \text{Q. E. D.}
\end{aligned}
$$

Corollary.—Since the angle HFH′ is constant, the gauge and frog angle remaining the same, it is evident that the locus of the point of frog is the circumference of a circle passing through HH′ and F as shown by dotted circles in Figs. 3, 4 and 5. The diameter of this

$$\text{circle} = FH\ (\text{Fig. 3.}) = \frac{\text{gauge}}{\sin.\ \frac{1}{2} f} \quad . \quad (2.)$$

4. Theorem II.—*Those parts of the center lines of the main and turnout tracks extending from the heel of switch to the point of frog have a common intersection point* (O Fig. 6) *and their tangents* (Op, Ot *and* Ot' Fig. 6) *are equal to the product of the gauge into the frog number.*

In Fig. 6, O is the center of the circle passing through the points F, H, H.′ C and C′ are the centers of the main line and turnout.

Produce CF, and take $Fm = H'H =$ gauge. The triangles OFC and OH′C equal, hence, $< OFC = < OH'C$ and $< OH'H = OFm$. Hence the triangles OFm and OH′H have two sides and the included angle of one equal to corresponding parts of the other.

Hence, $Om = OH$, and m is a point on the circumference $HH'F$. The point n on Fc' so taken that $Fn = HH'$ is also on this circumference. Therefore lines drawn from O to the middle points of the chords Fm, Fn and HH' at t, t' and p will be equal to each other, and also perp. to the

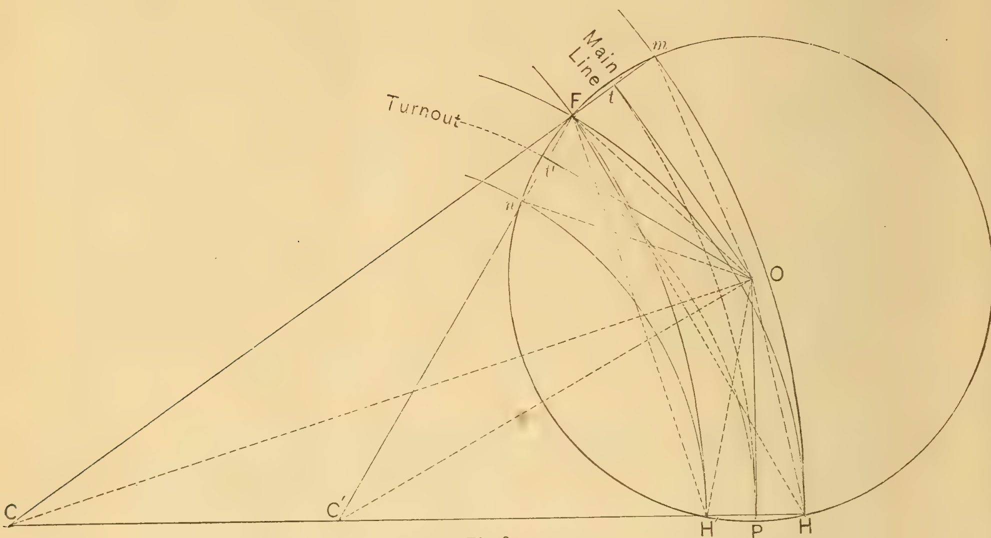


Fig. 6

radii (Ct $C't'$ and Cp) of the center lines of the main and side tracks. Therefore Ot Ot' and Op are the tangents of the curves pt and pt', and O is the common intersection point.

By theorem I, ang. $H'FH$ equals $\frac{1}{2}$ the frog angle; but $H'OH = 2H'FH$.

$$\therefore\ H'OH = f = \text{frog angle.}$$

$$\therefore\ \frac{Op}{H'H} = \text{no. of frog} = n,$$

$$\text{or}\ \frac{\text{Tangent}}{\text{gauge}} = n$$

$$\therefore\ \text{Tangent} = g \times n \quad (3)\quad \text{Q. E. D.}$$

5. Problem.—*Given: the frog number n, and the gauge of the track g, to find the radius of curvature of a tangent turnout.* (Cp, Fig. 3.)

By Theorum II, the tangents of the turnout curve $= g \times n$ ($= Op = Ot = Ot'$). But tan. of curve $= R \times \tan.\frac{1}{2}f$ ($f = < t'Ot$)

$$\therefore\ g \times n = R \times \tan.\tfrac{1}{2}f = \frac{R}{\cot.\frac{1}{2}f} = \frac{R}{2n} \text{by (1).}$$

and $\qquad R = 2gn^2 \quad . \quad . \quad . \quad . \quad (4).$

Hence the

Rule.—*The radius of a turnout from a tangent, equals twice the gauge into the square of the frog number.*

$H'F$ (Fig. 3) $= pt = 2gn$, since

$pt = pO + Ot$,

also $\quad t'Ot = f =$ frog angle.

This affords a most convenient way of staking out turnouts by setting the transit at the intersection point O and centering t and t' at a distance from $O = g \times n$.

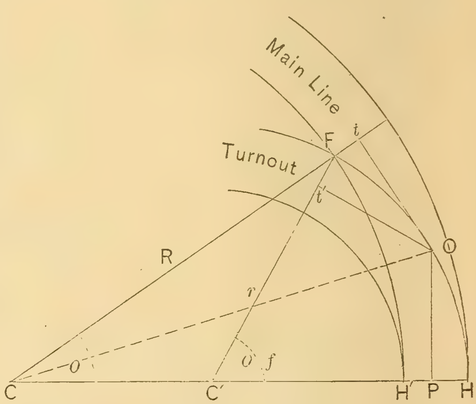


Fig. 7

6. *Given: a main track on a 4° curve* (R=1432.7), *and a number 9 frog* (*ang.*=6°22′), *to locate a turnout from the inside of the curve.* See Fig. 7.

Put θ=FCH—the total angle of the 4° curve from heel of switch to point of frog.

$$g = \text{gauge} = 4.71 \text{ ft.}$$
$$n = \text{frog No.} = 9.$$

By Theorem II,

$$\text{tangents} = g \times n = 4.24 \text{ ft.}$$

But $\quad \text{tangent} = \text{radius} \times \tan.\tfrac{1}{2}\theta$

$$\therefore\ 42.4 = 1432.7 \times \tan.\tfrac{1}{2}\theta$$
$$\theta = 3°24'$$

Hence, $\theta + f$=intersection angle of the turnout curve=9°46.′

To find r, the radius of the turnout curve,

$$Ot' = gn = r \times \tan.\tfrac{1}{2}(\theta + f)$$
$$r = \frac{gn}{\tan.\frac{1}{2}(\theta+f)} = \frac{42\ 4}{.08544} = 496.5$$

corresponding to a 11°34′ curve.

These values of θ and $\theta + f$ may be found more quickly by using the table of tangents to a 1° curve in Shunks' *Field Engineer*. Thus $42.4 \times 4° = 169.6$, the corresponding tangent of a 1° curve. Referring to the table we find that an angle of 3°24′ corresponds to a tangent of 170 ft., which is sufficiently exact.

It may be here remarked that if a 50 ft. chord be used, Shunks table of functions for a 1° curve is practically exact, even when applied to sharp curves, as for this chord length the radii are very nearly in the inverse ratio of the degree of curve.

The turnout, Fig. 7, may be located on the ground, by setting the transit at p and running the 4° and 11°34′ curves to t and t'; or lay off the tangent pO, then from O set off O$t(=g \times n)$ making an angle with Op=3°24′ and O$t'(=g\times n)$ by turning an additional angle tOt'=6°22.′

Similarily, the conditions of a turnout from the outside of a curve may be determined. In this case, however, the intersection angle of the turnout will equal the difference of θ and f instead of their sum.

7. In practice calculations may be much shortened by using convenient approximations, thus let $2gn$ (the sum of the tangents) be taken as the length of both main line and turnout. This assumption is nearly true for ordinary curves less than 100 ft. long. Let θ = intersection angle of the main line, D its degree of curve, and f the frog angle; then,

$$\theta : D :: 2gn : 100$$

or $$D = \frac{50\theta}{gn} \quad . \quad . \quad . \quad . \quad (5).$$

If the turnout be from the inside of the main line, the intersection angle will be $f + \theta$. (See Art. 6.)

Put D′=degree of curve of turnout; then $\quad f + \theta : D' :: 2gn : 100$

or $$D' = \frac{50(f+\theta)}{gn} \quad . \quad . \quad (6).$$

Subtracting (5) from (6)

$$D' - D = \frac{50\,f}{gn} \text{ or } D' = D + \frac{50\,f}{gn} \quad . \quad (7).$$

If the turnout be from a tangent, make θ and D=zero, and equation (6) becomes

$$D' = \frac{50\,f}{gn} \quad . \quad . \quad . \quad . \quad (8).$$

It is seen by comparing (7) and (8) that *the degree of curve of a turnout from the inside of a curve, equals the degree of curve of the main line, plus the degree of curve of a turnout from tangent, the gauge and frog angle remaining the same.*

Example, Given: g=4.71, n=9, f= 6°22′; to find D′ for a turnout from tangent, and also from the inside of a 4° curve.

By equation (8)

$$D' = \frac{50 \times 6°22'}{4.71 \times 9} = \frac{19100'}{42.4} = 450' = 7°30'$$

which is the degree of curve of a turnout from a tangent.

Again from equation (7)

$$D' = 4° + 7°30' = 11°30'.$$

Compare with the result in Art. 6.

8. For a turnout from the outside of a curve, the intersection angle of the main line being θ, that of the turnout will be $f - \theta$. See Fig. 5.

Let D′=degree of turnout curve, and D degree of curve main line, then

$$f - \theta : D' :: 2gn : 100$$
$$D' = \frac{50(f-\theta)}{gn} \quad . \quad . \quad . \quad (9).$$

Adding (5) and (9)

$$D + D' = \frac{50f}{gn}\ .\ D' = \frac{50f}{ng} - D\ .\ (10).$$

Hence the degree of curve of a turnout from the outside of a curved main line equals the difference between the degrees of curve of the main line and the corresponding tangent turnout.

EXAMPLE, Given: $g = 4.71$, $n = 9$, $f=6°22'$, to find D′ for a turnout from the outside of a 4° curve.

By eqs. (8) and (10) $D'=7°30'-4°=3°30$. In this case the main line and turnout will curve in opposite directions.

EXAMPLE, Given: $g=4.71, n=9, f=6°22'$, to find the degree of curve of a turnout from the outside of a 10° curve.

Here $D'=7°30'-10=-2°30'$, and the main line and turnout will curve in the same direction.

Then, degree of curve $ca=7°30'+3°=10°30'$. If ba and ca each equal 50 ft., then ang. $dab=\frac{3°}{4}$ (da being the common tangent at a).

$$\text{ang. } cad=\frac{10°30'}{4}$$

$$\therefore \text{ ang. } cab=\frac{7°30'}{4}=1°52\tfrac{1}{2}' \text{ as before.}$$

For a turnout from the outside of a curve, the sum of the deflection angles of the main line and turnout, for any given distance, equals the deflection angle of the turnout from tangent for the same distance.

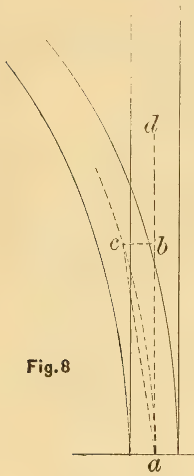


Fig. 8

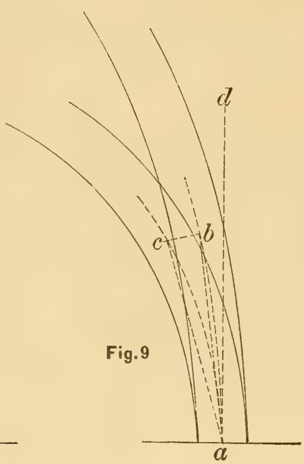


Fig. 9

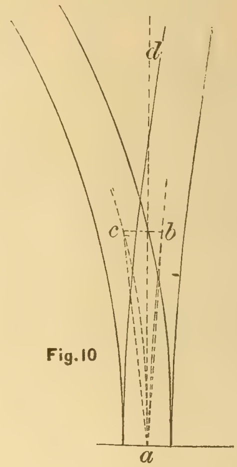


Fig. 10

9. Since [eq. (7)], for a turnout from the inside of a curve, the difference of the degrees of curvature of main line and turnout is constant, and equal to the degree of curve of a turnout from tangent; it follows, that the difference of the deflection angles of the main line and turnout, for any given distance, will equal the deflection angle, for the same distance, of the turnout from a tangent.

Thus let Fig. 8 represent a turnout from tangent, $g=4.71$, $n=9$. The degree of turnout curve will be 7°30.′ Let a be the heel of switch, and take ab and ac each 50 ft. Then the angle $cab=\frac{7°30'}{4}=1°52\frac{1}{2}'$. Fig. 9 represents a turnout from the inside of a 3° curve, g and n remaining the same.

Thus, Fig. 10, if ab be a 3° curve ac will be a 4°30′; hence

$$\text{ang. } dab=\frac{3°}{4} \text{ (}ab\text{ being 50 ft.)}$$

$$\text{ang. } dac=\frac{4°30'}{4}$$

$$\text{adding, } cab=\frac{7°30'}{4}=1°52\tfrac{1}{2}' \text{ as before.}$$

Since in figures 8, 9 and 10, the angle

$$bac=1°52\tfrac{1}{2}'$$

and $$ac=ab=50 \text{ ft.}$$

$$cb=2\sin.\frac{1°52\frac{1}{2}'}{2}\times 50;$$

or, in general, $cb=2\sin.\frac{1}{2}\,bac\times ac$. (11).

A table based on (11) would afford a ready means of laying out turnouts. Assuming convenient values for ab the corresponding values of cb are calculated for the proper degree of curve (ac Fig. 8). In practice a cord or tape should be held at a (the heel of switch) and the proposed diatance laid off to b, then the tape is swung around a as a center, until the corresponding value of cb is obtained.

The distances cb may be set off from either rail of the main line to the corresponding rail of the turnout, but this presupposes that the main line is laid and approximately lined.

This method of putting in turnouts has two advantages:

1st. It is uniform in its application to tangents and curves of all kinds, but one table being required (g and n remaining the same).

2d. Since turnouts are laid on switch ties, it is of but slight importance that the main track be in perfect line before putting in the turnout, for the off-sets determine the correct position of one rail with reference to the other, so that when one is brought to line the other must necessarily follow.

By modifying the value of the gauge in the formulæ a short tangent may be obtained through the frog, which is the customary practice.

10. Switch Rails. — It was before stated that the length of switch rail was usually so taken that the throw of switch, Bb, Fig. 1, is the corresponding offset from a tangent HB to the turnout curve at b.

Assuming the tangent offsets to vary as the squares of their distances from the tangent point, we have, Fig. 1,

$$HH' : Bb :: \overline{HF}^2 : \overline{HB}^2.$$

Whence

$$HB = H'F\sqrt{\frac{Bb}{HH'}};$$

but $$H'F = 2gn. \quad \text{(Theorem II.)}$$

and $$HH' = g. \quad Bb = t = \text{throw}.$$

$$\therefore \quad HB = \sqrt{4gn^2 \times t} = \sqrt{2r \times t} \quad . \ (12).$$

$$\text{since } 2gn^2 = r.$$

Rule.—*To find the length of switch rail for a tangent turnout, multiply twice the radius of the turnout by the throw of switch and extract the square root of the product.*

By (12) $\quad HB = \sqrt{4gn^2 \times t},$

or $\quad HB = 2n\sqrt{gt},$

Hence the length of switch rail is directly proportional to the frog number, the gauge and throw remaining the same. Thus for a 10 frog, $g = 4.71$ $t = 5$ in. $=$ .417 ft.

$$\therefore \quad HB = \sqrt{4 \times 471. \times .417} = 28 \text{ ft.}$$

For a 9 frog $HB = \dfrac{28 \times 9}{10} = 25.2$ ft.

For an 8 frog $HB = \dfrac{28 \times 8}{10} 22.4$ ft.

For a 10 frog, the gauge being 3 feet, $t = .333$ ft.

$$HB = \sqrt{1200 \times \tfrac{1}{3}} = 20 \text{ ft.}$$

For an 8 frog $HB = \dfrac{8 \times 20}{10} = 16$ ft.

If the turnout be from a curve the length of switch rail will not be changed, for it has been shown in Art. 9 that ab being constant the values of bc are the same in Figs. 8, 9 and 10; hence if bc be taken as the throw in the three figures, the values of ab, or the length of switch rail, will be the same for the turnouts from curves, Figs. 9 and 10, as for the tangent turnout, Fig. 8.

11. It has been assumed that the switch rail springs to a circular curve of the same radius as the rest of the turnout.

Putting t = throw.
l = length of switch rail.
S = switch angle;

or the total curvature of the switch rail from heel of switch D, Fig. 11, to the head block A, then

$$\tan. S = \frac{t}{\frac{1}{2}l} = \frac{AC}{AB}, \text{ Fig. 11.} \quad . \ (13).$$

If the rail be influenced by the switch lever alone, neglecting the friction on the ties, the curve assumed will be the elastic curve.

Hence $$\tan. S = \frac{t}{\frac{2}{3}l'} = \frac{AC}{AB'}, \text{ Fig. 12} \quad . \ (14).$$

Equating (13) and (14),

$$\frac{t}{\frac{1}{2}l} = \frac{t}{\frac{2}{3}l'},$$

$$\therefore \quad l' = \tfrac{3}{4}l \quad . \quad . \quad . \quad (15).$$

Thus l for a switch rail sprung in a circular arc, when $g=4.71$, and $n=9$, will be 25.2 ft.

If, however, the switch rail be bent to an elastic curve, the switch angle being the same, then Eq. (15),

$$l'=\tfrac{3}{4}\times 25.2 \text{ ft.}=18.9 \text{ ft.}$$

The true curve lies somewhere between these extremes, and, in actual practice, the switch rail is spiked up until the proper switch angle is obtained. Hence all calculations may be made for simple circular curves, the length of switch rail being adjusted experimentally to fit the rest of the turnout.

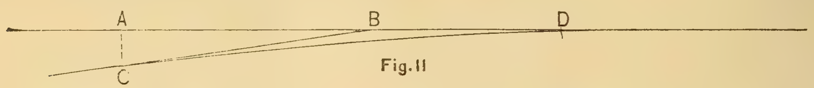


Fig. 11

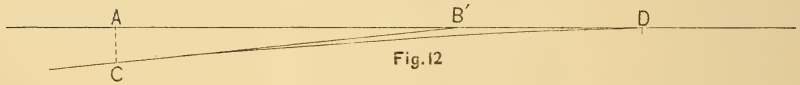


Fig. 12

12. Double Turnouts.

A double turnout from a tangent is shown in Fig. 13.

Here three frogs are used, two of which F and F′ are alike, and the angle of the middle frog F_m should be so taken that the turnout curves HF_mF, and $H'F_mF'$ will be simple circular curves.

Let D $=$degree of curve of main line.

D′ $=$degree of curve of turnout to right.

D″$=$degree of curve of turnout to left.

Then by Art. 7, Eq. (7):

$$D''=D+\text{constant} \quad . \quad . \quad (16).$$

By Eq. (10),

$$D'=-D+\text{constant} \quad . \quad . \quad (17).$$

Adding

$$D''+D'=2 \text{ constant} \quad . \quad . \quad (18).$$

The constant is the degree of curve of a turnout from a tangent with the given frogs F or F′.

Now, if D″ be considered as a turnout from the outside of D′ as a main line, then by (10),

$D'+D''=$degree of curve of turnout from tangent corresponding to frog F_m (19), (number$=$N).

Comparing (18) and (19) it is seen that F_m will be the proper frog for a tangent turnout whose degree of curve is double that of a similar turnout for the frogs F and F′.

Putting R$=$radius of tangent turnout for the frog F_m,

and $r=$radius of tangent turnout for the frog F.

and assuming that the radii are inversely

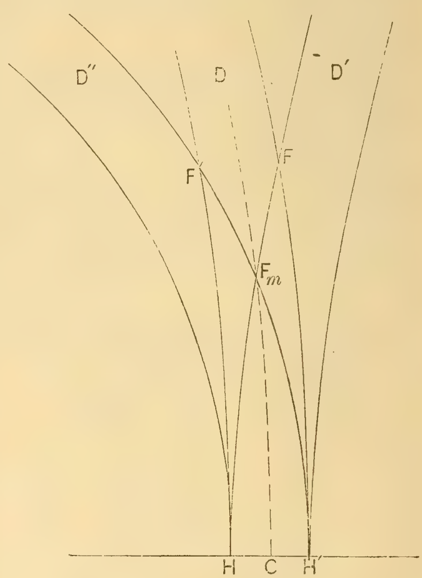


Fig. 13

proportional to the degrees of curvature, then

$$R=\tfrac{1}{2}r \quad . \quad . \quad . \quad . \quad . \quad . \quad (20).$$

Put $n=$No. of frogs F and F′.

N$=$ " " frog F_m.

then by (4),

$$R=2gN^2 \quad . \quad . \quad . \quad . \quad . \quad (21).$$

also $$r=2gn_2 \quad . \quad . \quad . \quad . \quad . \quad (22).$$

Comparing (20), (21) and (22),

$$gn^2=2gN^2,$$

or $$N=\frac{n}{\sqrt{2}}=.707n \quad . \quad . \quad . \quad (23)$$

The frogs F and F′ may be located as though they were single frogs.

To locate F_m the center line CF_m, Fig. 13, is assumed to represent the gauge side of a rail. A glance at the figure will show that the point F_m is the proper location of a frog whose number is 2N, the gauge of track being $\frac{1}{2}g$. Hence, approximately,

$$CF_m=2\times\tfrac{1}{2}g\times 2N=2gN\;;$$

but (23) $$N=.707n,$$

$$\therefore \quad CF_m=.707\ (2gn) \quad . \quad . \quad . \quad (24).$$

Or the point of crotch frog is located at a distance from the heel of switch equal to $\frac{7}{10}$ the distance of the main frog from the same point, the distances being measured along the center line of the main track.

From the foregoing it will be remarked that a three-throw set of frogs consists of two frogs of like number (n), and a third or crotch frog, whose number $N=\frac{7}{10}n$ nearly; and, further, that this set is just as applicable to a curved main line as to a straight one.

13. Side Tracks.—*Given: a turnout from tangent with a frog angle* f (*No.*=n), *and the perpendicular distance* (p) *between the center lines of the main and side tracks, to join the turnout with the side track, by a curve reversing at the frog point.*

An inspection of Fig. 14 will show that $mn=(p-g)$ may be taken as the gauge of a turnout, m being the heel of switch, F the frog, and mF the outside rail of the imaginary turnout.

By (4)

$$R=2gn^2$$

$$\therefore \quad CO=2(p-g)n^2,$$

and the required radius:

$$CS=CO+\tfrac{1}{2}p=2(p-g)n^2+\tfrac{1}{2}p \quad . \quad . \quad (25).$$

The total angle of the curve will equal the frog angle.

It is customary, however, to insert a short tangent between the frog and the

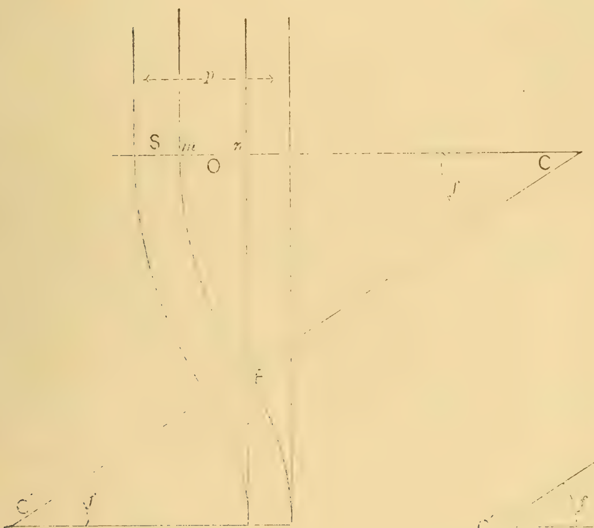


Fig. 14

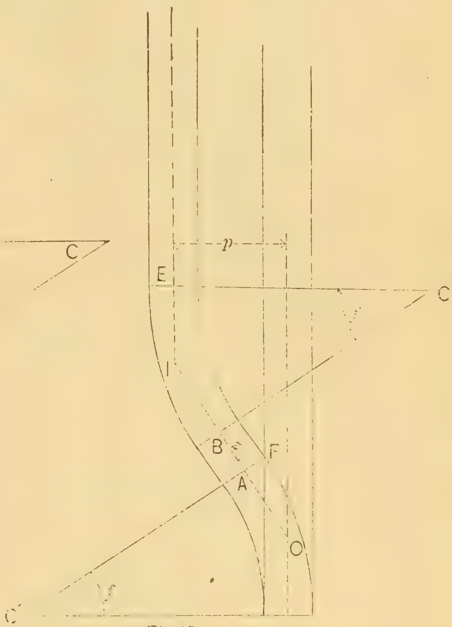


Fig. 15

P. C. of the connecting curve, as AB, Fig. 15.

By Theorem II. $OA = gn$;

also, $$OI = \frac{p}{\sin. f}.$$

But $BA = l$ = length of tangent.
But $BI = t$ = tangent of curve BE.

Then $$t = \frac{p}{\sin. f} - (gn + l),$$

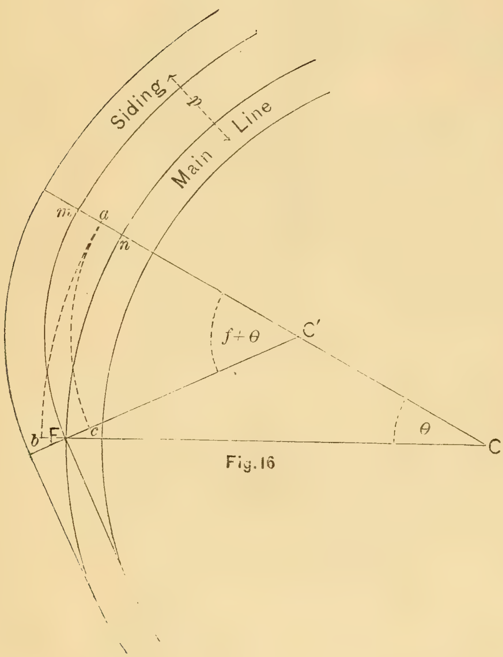


Fig. 16

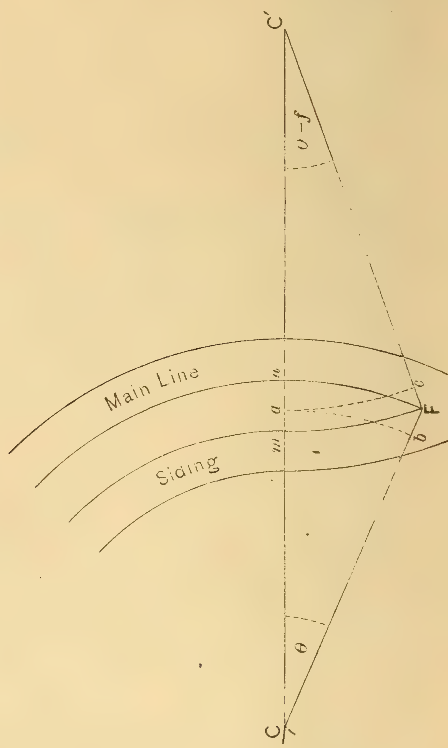


Fig. 17

and since the angle at $I = f$,

$$CB = R = \frac{\frac{p}{\sin. F} - (gn + l)}{\tan. \frac{1}{2} f}$$

$$= \left(\frac{p}{\sin. f} - (gn + l)\right) \cot. \tfrac{1}{2} f.$$

But by (1) $\cot. \frac{1}{2} f = 2n$.

$$\therefore \quad R = \left(\frac{p}{\sin. f} - (gn + l)\right) 2n \quad . \quad . \quad (26).$$

or since $\frac{p}{\sin. f} = pn$ nearly,

$$R = [(p - g)n - l] 2n \quad . \quad (27).$$

14. *Given: a turnout from a curved main line, to connect the turnout curve with a side track, by a simple curve beginning at the point of frog.*

Put p = distance between centers of main and side tracks, R = radius of main line, r = radius of the required connecting curve.

Case I.—*The siding outside the main track.* Fig, 16.

Let mF, as before, be taken as the outer rail of a turnout curve, m being the heel of switch and F the point of frog, $mn = (p - g)$ being the gauge.

Then, by theorem II., the tangents of the arcs ab and ac are equal to $(p - g)n$;

also $$\tan. \tfrac{1}{2} \theta = \frac{\text{tangent}}{\text{radius}}$$

$$\therefore \quad \tan. \tfrac{1}{2} \theta = \frac{(p - g)n}{R + \frac{1}{2} p} \quad . \quad . \quad . \quad (28).$$

Also $$\tan. \tfrac{1}{2}(f + \theta) = \frac{(p - g)n}{C'a},$$

where $C'a = r - \frac{1}{2} p$.

$$\therefore \quad r = \frac{(p - g)n}{\tan. \frac{1}{2}(f + \theta)} + \tfrac{1}{2} p \quad . \quad (29).$$

CASE II.—*The siding inside of the main line.* Fig. 17.

As before, take mF as the outer rail of a turnout curve, m being the heel of switch and $p-g$ the gauge.

$$\therefore \ C'a+\tfrac{1}{2}p=r=\frac{(p-g)n}{\tan.\frac{1}{2}(\theta-f)}+\tfrac{1}{2}p \quad . \ (31).$$

In Fig. 18 the siding is on the inside of the main line, but θ is greater than f, and the centers of main line and connect-

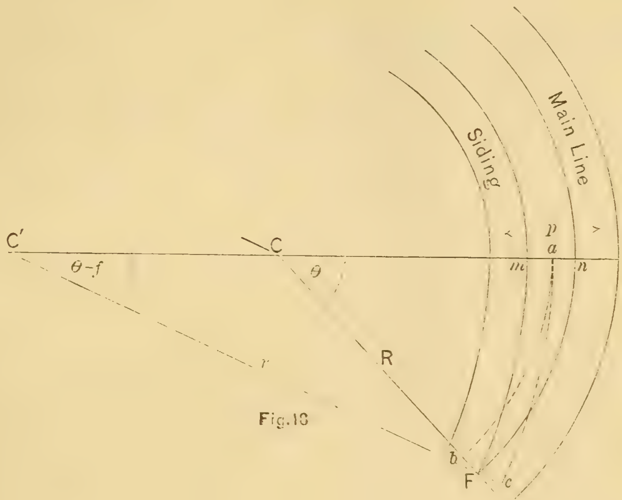


Fig. 18

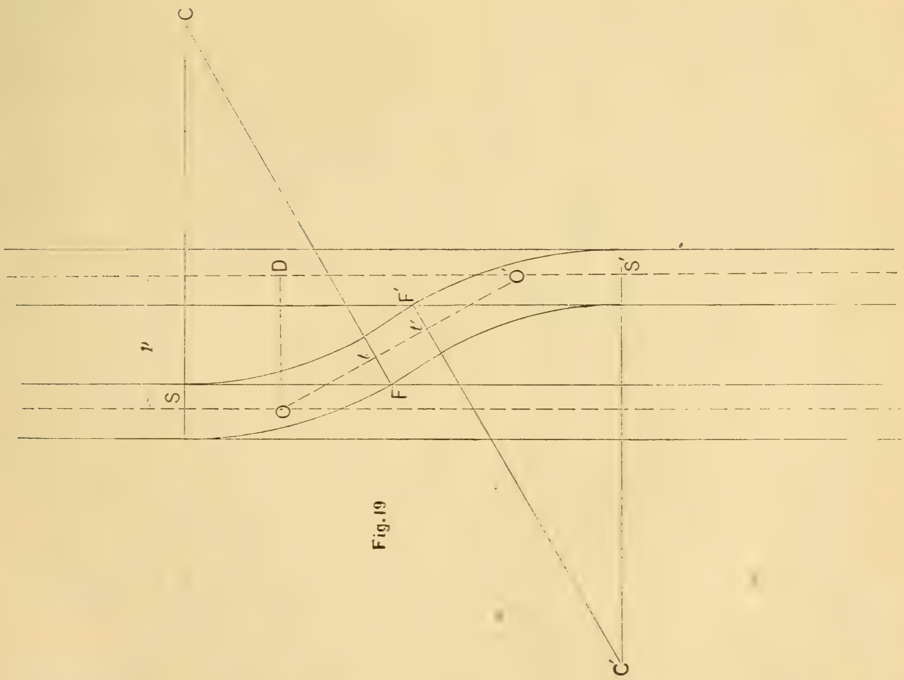


Fig. 19

$$\tan.\tfrac{1}{2}\theta=\frac{\text{tangent}}{\text{radius}}=\frac{(p-g)n}{\mathrm{R}-\frac{1}{2}p} \quad . \quad . \ (30).$$

Also $\quad \tan.\frac{1}{2}(\theta-f)=\dfrac{(p-g)n}{C'a}.$

ing curve are on the same side of the main line. The preceding formulæ then become

$$\tan.\tfrac{1}{2}\theta=\frac{\text{tangent}}{\text{radius}}=\frac{(p-g)n}{\mathrm{R}-\frac{1}{2}p} \quad . \quad (32).$$

$$\tan.\tfrac{1}{2}(\theta-f)=\frac{(p-g)n}{C'a}$$

$$\therefore\ C'a-\tfrac{1}{2}p=r=\frac{(p-g)n}{\tan.\tfrac{1}{2}(\theta-f)}-\tfrac{1}{2}p \quad (33).$$

15. THE LINK.

A link, or cross over track, is represented in Fig. 19, the parallel tracks being straight. The two frogs F and F′ are alike, and it is required to determine the distance tt'.

Taking g=gauge, n=No. of frog, and p=perp. distance between parallel track centers, then

$$\text{ang. } OO'D=\text{frog angle } f$$

$$\therefore\ OO'=\frac{p}{\sin.f}$$

also $$O't'=Ot=g\times n$$

$$\therefore\ tt'=\frac{p}{\sin f}-2gn \quad . \quad . \quad (34).$$

or approximately $OO'=pn$

$$O't'=Ot=gn$$

$$\therefore\ tt'=pn-2gn=(p-2g)n \quad . \quad . \quad (35)$$

EXPLOSIVES.

By M. BERTHELOT, PARIS.

Translated by M. BENJAMIN, Ph. B., F. C. S.

I.

GENERAL OBSERVATIONS IN THE FORCE OF EXPLOSIVES.

1. The force of an explosive may be understood in two ways according to the different senses in which the word is applied, that is, it may be considered either as the pressure developed or as the work accomplished. It frequently happens that the word force is used to represent the *pressure* resulting from the explosive, or (to put it more definitely) that produced by the gas arising from the decomposition.

It is this which is the cause of the bursting of hollow projectiles and the breaking down of walls in mines.

But this definition is not complete, for the reason that hydraulic pressure would effect similar results without producing any notable ultimate effects, whereas certain mechanical results follow as the result of explosives, such as the noise, or the extended fracture of rocks, the projection of balls, of bullets, and of fragments from hollow shells shattered by the explosion.

2. The following detailed list includes the principal applications of explosives in industrial or military arts.

First.—The bursting of hollow projectiles by black powder or its substitutes.

Second.—The breaking of masses of cast or wrought iron, such as bears, which accumulate below the tap hole of cupolas, or which form in the crucibles of blast furnaces, causing the suspension of all work. Black powder is almost entirely without action on such material, and it becomes necessary to use some stronger agents, such as nitro-glycerine, or dynamite, or even compressed gun-cotton, in order to break into pieces the cast iron or wrought iron.

Third.—The destruction of metallic bridges by twisting, tearing, or otherwise removing them from their location, to prevent their use in times of war, with the utter ruin of their fragments either on land or under the water, so that the bridge cannot be reconstructed.

Fourth.—The breaking, rupturing and piercing of rails and metallic plates, such as sheeting and the like.

Fifth.—The bursting or putting out of service of pieces of ordnance, steel, cast iron or bronze, either by exploding them with dynamite placed in their boxes, or else by similar treatment externally so as to destroy the trunnions.

Sixth.—The blasting of rocks by means of dynamite, gun-cotton, or any of the various forms of black powder (gun powder, mine powder, etc.).

This blasting may be for the simple dislocation of the rocks, or else for their reduction into smaller or larger pieces, which remain *in situ* or are trimmed and piled in heaps for some industrial usage, or which may be employed for

some military operation. Finally, it is possible to pulverize the rocks into powder or into very small pieces when it is desired to dig a hole or an opening in the ground. The differences in rocks, on account of their hardness or tenacity, their aquiferous or fissure-like character demand a great diversity in the use of explosives, which are necessary to produce any given result.

Some very interesting applications of both dynamite and gun-cotton have resulted from submarine blasting and by their employment, it becomes possible to make constructions, which thus far have been deemed impracticable.

Seventh.—The destruction and excavation of clay banks or earth works by dynamite.

The digging of chambers and passages in clay or earth.

Eighth.—The demolition of all kinds of masonry, the destruction of bridges, tunnels, constructions of all sorts, galleries in mines, etc.

Ninth.—The breaking of ice and the removal of icicles by extensive displacement of the material; for this variety of work dynamite is especially adapted.

Tenth.—The breaking of wood by splitting, cutting or tearing, such as the removing of standing timber by using dynamite in land clearing or in war; the breaking down or overthrowing of palisades; the demolition and breaking up of piles under water; the tearing up and breaking of buried stumps of trees.

Eleventh.—The destruction of floating vessels, the breaking up of stranded ships or submarine wrecks.

Twelfth.—The destruction of torpedoes, of submarine or subterranean mines from a distance.

Thirteenth.—The projection of balls, bullets, shells, etc, from various weapons, guns, cannon, etc.

Fourteenth.—The projection of rockets by the composition of an internal charge of powder.

Fifteenth.—The ignition by primers or detonators, which determine explosions in the main body of gunpowder or dynamite.

We shall not at present refer to the pyrotechnic displays, that is to say, the use of powder on the producing agent of light and fireworks. The theory of these displays is an entirely different one from those which we are about to consider.

3. The different applications of explosives which we have just enumerated are produced equally by the pressure and the effect of these substances.

The *pressure* depends principally upon the nature of the gases formed, on their volume and on the temperature.

The *work* on the other hand is principally dependant upon the amount of heat given off, which is the criterion of the energy developed. In other words, the *maximum work* which an explosive substance is capable of producing is proportional to the quality of heat given off in consequence of the chemical decomposition of the explosive, this matter being taken at the existing pressure and temperature, and its theoretical products reduced to the same conditions.

4. Let Q be this quantity of heat, expressed in calories, the corresponding effect translated into kilogrammeters would be according to the mechanical equivalent of heat, 425 Q.

This figure expressed the *potential energy* of the explosive.

Without a doubt, this is a limit that is never reached in practice, but it is necessary to be familiar with it as the only absolute term for comparison.

5. The effective transformation of this energy into work depends upon the volume of gas, the temperature, and the law of expansion. The transformation is never complete; worse than this, only a portion of the work itself is utilized in the application. For instance, in guns, the work which communicates to the projectile its living force, is the only one which is taken into account; it represents the actual amount utilized, while the work used up at the expense of the walls of the weapon, and by the gases and the air projected, is lost. An important fraction of the energy always remains unused in the form of heat locked up in the gases, or else communicated to the projectile, to the gun, etc.

The calculation of the proper distribution of energy between the heating property so called, the mechanical result accomplished, the living force communicated, the vibrating movements of the ground and of the air, is a most complicated affair.

6. The following are some general ideas in this connection, ideas which we think it best to present at this juncture. In consideration of the work for which explosives are to be employed, they are distinguished as *strong powders* and *weak powders*, *rapid powders*, and *slow powders*.

7. *Strong and rapid powders.*—The materials whose chemical decomposition takes place very rapidly, such as mercury fulminate, produce principally the effect of crushing rocks *in situ*, or breaking the shells of hollow projectiles into a multitude of small fragments, the elasticity of the entire mass not having had time to come into play; they constitute what are called the *breaking powders*.

Furthermore, the living force of translation communicated to particles contiguous to the powder, becomes predominant in consequence of the sudden production of the enormous pressures which is particularly characteristic of this class of powders. However, its influence exerts itself in a special manner on the surrounding gases, the molecules of which find themselves thrown out all of a sudden with a rapidity very much greater than that of their actual change of place, which is, as is known, comparable to the exact rapidity of sound in gases; in consequence, the molecules of gas tend to accumulate, the one on top of the other, and to produce the effects of a shock, and even of rupture, which may be compared to those which result from the shock or the pressure of an extremely hard, solid body.

Such are the extreme effects produced by the almost instantaneous explosion of a breaking powder.

But if the decomposition is retarded a little, and if the potential energy is considerable (*strong powders*), the explosive has a tendency to produce a tearing or shearing in the lines of the least resistance, even of metals having the greatest resisting powers.

These results extend for some distance into the mass of compact substances of moderate tenacity; they are the effects of dislocation. They produce the results without projection, provided that the masses to which the movement is communicated are of sufficient size.

In the employment of strong and breaking powders, it is possible to suppress or lessen the tamping, the communication of the presure taking place by mere contact before the substances have time to be driven away by the compressed air.

It is thus, that a feeble charge of dynamite placed in the open air on a hewn stone, and covered by a simple sand-bag, is sufficient to break that stone into small pieces. A single cartridge containing 150 grains of dynamite (of 75 per cent.), will, in this way, break a block having a surface equal to 60–80 square decimeters and a thickness of 40 centimeters. The piece will be broken up according to the cracks which radiate from the center of the explosion, and is analagous to that which would be produced by the falling of an iron beam from a great height. In a word, the effect is that of a gigantic shock, and is extremely sharp. Besides this dynamite may be used to break a block according to a given design, as if by a wedge. All that is necessary is that a furrow shall be marked along the surface, with a center drill-hole into which the charge is placed.

It is in consequence of this means of propagating the pressure, that the depth of a blast-hole may be made much smaller when dynamite is used. This is not all. In a blast-hole, the effect of the layers and crevices in the rock do not seriously influence the action of such a powder, providing, however, that the layers or crevices are not directed towards the center of the shock. These powders are also greatly preferred for displacing fissured and aquiferous territory; they excel all others in tearing down a sand bank, or for enlarging a stony excavation. The conditions under which they act, are such that they may be employed equally as well for drilling an opening in the ground, of medium size, perpendicular to the surface, always in the direction of the least resistance, and without paying any attention to the breaking of rocks that may lay in their pathway.

With such powders, the effect of successive explosions in the same chamber are accumulative, that is to say, the fissures produced by the first shock are increased with the second; by taking advantage of this circumstance, it is possible to obtain pieces of much greater dimensions than would have been obtained by one single operation, using the entire quantity of dynamite.

These different characteristic properties showing the action of dynamite, lead us to regard it as the type of the strong and rapid powders.

8. *Strong and Slow Powders.*—Black powder or gunpowder is also a strong powder, although for equal weight, of considerable less power than dynamite; but at the same time it is a slow powder. In consequence it produces a pressure which increases more slowly, and is of longer duration. It does not break the material *in situ* into small fragments, a quality which is very essential for certain purposes, for instance, in the mining of coal, where it is desirable to break the material into pieces as large as possible, although the substance is quite brittle and easily cracked. Black powder will break an empty projectile into a smaller number of pieces with less effort, and which can then be driven further for the same expenditure of energy, less of it having been consumed in the work of crushing.

On the other hand, black powder has less effect, and does not break rock in a mine according to the direction where the mass is very compact and strongly adherent. It is easily turned aside, especially if the tamping or charge has not a greater resistance than that of the direction in which the rock has the least resistance. On this account it is necessary to make the drilling holes very deep and sometimes inclined at an angle of 45° for the purpose of giving a convenient length to the tamping, which is an element of expense.

The masses which become detached in the direction of the least resistance are frequently thrown to some little distance by black powder.

The cracks and cleavages adjoining the charge lessen the effect of the explosion. They weaken it until the effect is nullified; if these cross the drilling the expansion of the gases from the powder frequently takes place in the interior cavities, it is then that the mine *blows.* Also, in fissured rocks much time is often lost in closing up with rammed clay the cracks which are connected with the drill hole, whereas this work is useless when dynamite is used. For similar reasons it is of little value in blasting argillaceous or aquiferous rocks and calcareous tufas, in conglomerates, in beds of sand of high resistance, in one direction, and slight resistance in others.

It has little effect, for directly opposite reasons, in very hard and tenacious rocks, such as quartzite and certain felspars.

These circumstances, in addition to the fact that black powder has less force (the effect of one part of dynamite is regarded in practice as equivalent to two and one-half parts of black powder), explain the preference given to dynamite in most mining operations.

Nevertheless, black powder possesses certain advantages due to the gradual increase of the pressure which allows it to transmit its effects to a distance, for instance, in beds of coal, or better still, in wood, following the direction of the fibers. In recent earthworks, the pressures, if too suddenly produced by the quick powders, will shatter the mass and expend themselves in local work without much effect, while the slower tension of black powder displaces the earth and throws it in the direction of the least resistance.

From these details and these examples, we see what part the rapidity of the explosion plays in the transformation of energy into work.

9. Finally, the force of the explosion may be expressed in terms of the pressures produced, and by the work which they perform. The pressure is produced from the volume which the gases occupy at the temperature of the explosion. The work is due to the heat produced, and to the rapidity with which the gases are developed. These fundamental conditions —volume of the gases and the heat—are the consequences of the chemical decomposition; any reactions which liberate gases, or which augment the volume of a previously existing gas, may be the cause of an explosion. Therefore, in consideration of the foregoing observation, to define the force of an explosive, the following data are necessary:

First. *The chemical composition of the explosive.*

Second. *The composition of the products of explosion.*

The latter may vary during the different periods of temperature which succeed each other from the first moment of

ignition; it is also necessary to take the *dislocation* into account.

These three elements, the chemical composition of the exploding substance, the chemical composition of the products of the explosion, and finally the dissociation will be studied under the general title of *chemical composition.*

Also, it is desirable to define the following:

1. The *quantity of heat* given off during the reaction.

2. The volume of the gases formed under normal pressure.

These propositions are also obtainable from a knowledge of the two first in all reactions, which are positively known.

3. The rapidity with which the reaction takes place gives rise to the following studies, which are essential in order to be able to furnish a complete definition of explosives.

Origin of the reaction—The rapidity of the increase of the reactions.

To this sequence of ideas there should be attached a collection of phenomena, which are designated by the name of *explosions by influence.*

These phenomena, which have only been known for a few years, have seemed to us of sufficient importance to be separately considered with their accompanying developments.

In the treatment of these questions, we shall endeavor to cover all of the general ideas known at present concerning explosive substances.

II.

The Duration of the Explosive Reactions.

§ 1.—*Origin of the Reactions.*

We shall now take up the consideration of the chemical transformation of explosives, from the point of view of their origin and rapidity.

1. We will at first treat of its origin, that is to say, of the conditions which determine the commencement of the reaction. This, once started, maintains itself, and increases either by a simple progressive burning or by an almost instantaneous detonation.

Thus far, artillerists have expressed this origin by the expression "*set on fire*," which implies that fire is applied locally to begin with; but the study of explosives shows us that the origin of the reaction may arise equally as well from a shock, from pressure, from friction, or from some other analogous mechanical force.

At first, suppose that it is necessary to refer all explosive reactions to an original heating, which is increased step by step, by successively bringing all the particles of the substance up to the temperature of its decomposition. The shock, the pressure, the mechanical conditions are not efficacious except as they determine this first heating, according to mechanisms otherwise different, and to which we shall return in the following paragraphs.

2. This being understood, the decomposition of the same material can take place at widely varying temperatures, but with equal rapidity, a material slowly decomposed at a given temperature being able to resist at much higher temperatures, though for a time continually decreasing as the temperature rises. Elsewhere I have explained all of this theory, (*Essai de Mecanique Chimique*, vol. II., p. 58, et seq.), and it is recalled only for the purpose of thoroughly fixing the ideas which are developed there. It plays a very important rôle in the explanation of the mode of formation of the secondary compounds produced in the explosion of powder; several of these compounds are formed all of a sudden, at a temperature which destroys them slowly, if they were maintained at that heat during a sufficient length of time; but the abruptness of cooling preserves the compounds, such as formene, ammonia, nitric acid from the destruction toward which they would hasten, because it brings them to temperatures at which they are perfectly stable.

3. This is the place at which it is desirable to introduce some expressions on the *sensibility of explosives.* This sensibility is equally dependent on the conditions of heating and of the method of propagating the reactions. It varies according to the conditions. Some substances are sensitive to the slightest elevation of temperatures, others to a shock, properly so-called, others detonate at the least friction. Silver oxalate detonates near 130°, nitrogen sulphide about 270°, mercury fulminate at about the same figure, somewhere near 90°, nevertheless,

the fulminate is much more sensitive to a shock and friction than the nitrogen sulphide and the silver oxalate. Thus we discern the special properties depending upon the individual structure of each substance, particularly for solids. But there also exist general conditions which it will be useful to define here.

4. The sensibility is greater for the same substance when operated on at a higher initial temperature, that is to say, at a temperature nearer to that which the substance begins to spontaneously decompose.

A fortiori the sensitiveness will be still further increased, if this limit is exceeded, that is to say if conditions occur where a slow decomposition may be transformed by the slightest heating into a rapid decomposition. A substance within these limits may be said to be in a state of *chemical tension*, an expression which is sometimes erroneously employed with reference to stable bodies, or for mixtures which have no habitual tendency to enter into a spontaneous reaction.

We have an example of such a case in *celluloid*, a body which does not detonate when struck by a hammer at ordinary temperatures, but it acquires the property of detonating when it is heated up to the point where it becomes soft, that is to say, up to about 160° to 180°, a locality which is near the temperature at which the substance decomposes.

5. When two different explosives are compared, which are decomposed at the same temperature, and with similar rapidity, their sensitiveness relative to shock and to friction at a lower temperature depends primarily on the quantity of substance on which the work of the shock expends itself; that is to say, it depends upon the cohesion of the substance which governs the transformation of the shock into heat. Cohesion, likewise, interferes with direct ignition, as the same quantity of heat produced by the combination of the first portions can elevate to the degree of decomposition, the temperature of a small quantity of water to which it is exclusively applied, which, if it is distributed over a larger mass, the temperature of that mass will not be brought up to the required degree.

6. The mass heated remaining the same, and the materials being different, the sensitiveness will depend upon the temperature of decomposition, which, for example, is lower for potassium chlorate than for the nitrate; the chlorate gunpowder is more sensitive than that made with nitrate.

7. The sensitiveness depends, furthermore, on the quantity of heat set free by the decomposition, that is to say, the sensitiveness will be greater, other things being equal, if the reaction gives off a greater amount of heat.

8. This same quantity of heat will produce different effects in acting on the same weight of substance according to its specific heat. For instance if potassium chlorate, whose specific heat is 0.209, be substituted for an equal weight of potassium nitrate, whose specific heat is 0.239, in the composition of an explosive mixture, a powder more sensitive than the nitrate powder would be produced. This condition acts in concert with the lower temperature of decomposition and with the absence of cohesion in chlorate powder, so as to render them particularly dangerous.

§ 2.—*Molecular Rapidity of the Reaction.*

1. The chemical transformation in a detonating mass is propagated with a certain rapidity, a knowledge of which is desirable for theory as well as in practice. In reality the rapidity with which the gases are given off depends on it, and in consequence the rapidity communicated to projectiles in guns, as well as the effects produced in mines at the expense of rocks to be thrown down or the obstacles which engineering desires to remove. For, the heat given off by a given reaction may be employed almost entirely to heat the gases and to increase the pressure, provided the reaction is very rapid, while if the reaction is made slower it is dissipated without effect by radiation or by conductivity.

A given quantity of an explosive may in this manner crush, *in situ*, such portions of rock as it comes in contact with, its energy being consumed without any result, from an industrial point of view, on account of its instantaneous decomposition. If the development of the gases is less rapid, but is still quite fast, an equal quantity of explosive may, on the other hand, dislocate the rock by de-

veloping extending fissures and sharply striking those portions of rock which are the nearest, a result which is sought for by miners. It may also produce elastic displacements and an undulatory movement of the soil without any local disturbance, if the pressures are developed sufficiently slow, so that the rocks shall have had time to be displaced *en masse*, in which case the explosive will be found to have produced scarcely any useful result.

This question of the quickness of reactions plays an important part in the studies relating to explosives, and therefore I am led at this point to collect the experiences and results which they produce.

2. The quickness of a reaction may be considered in two ways, if it is to act upon a homogeneous system, and especially a gaseous system, surrounded by conditions of pressure and temperature identical in all its parts; also, if the system is submitted at one point to an elevation of temperature or to a shock capable of determining an explosion, which is then propagated step by step.

It is desirable to begin with the examination of the first case, which serves as a basis for all the theory.

3. Having, therefore, a certain body, or a certain mixture, capable of undergoing a chemical transformation when the entire mass is placed under the conditions of temperature, of pressure, or of vibratory motions, etc., it appears as if the reactions should be instantaneously developed in all parts at once. The sudden explosion of nitrogen chloride and nitro-glycerine seem at first sight favorable to this conception. Nevertheless, a closer observation proves that the molecular reactions as a general thing consume a certain amount of time for their accomplishment, even when they are giving off heat.

Such, for example, is the decomposition of formic acid into hydrogen and carbon dioxide, which furnishes experiments that are easy to follow on account of the slowness with which their decomposition takes place. Operating in a closed vessel, and kept at a fixed temperature of 260°, it requires quite a length of time. And still this reaction gives 5.8 calories to each equivalent of formic acid, that is to say, 126 calories to the grain.*

4. The following are other examples of reactions which give off a great quantity of heat without being instantaneous. Thus acetylene, changed into benzine at a dark red heat by a slow reaction, gives off, without increase of volume, one and a-half times as much heat as a detonating mixture composed of oxygen and hydrogen in the proportions which form water, that is, 85.5 calories for 33.6 liters of acetylene (reduced at 0° and to 0m. .760) instead of 59 calories produced by the formation of vapor of water, by means of the same volume of detonating gas. It is about four times the amount of heat given off by chlorate powder for the same weight, that is 2,192 calories for each grain of acetylene transformed, instead of 590.6 calories for each grain of potassium chlorate powder.

The cyanogen gives off three times as much heat (1,435 calories to the grain) as the same weight of chlorate powder; or again, twice the amount of heat disengaged by its own volume of a detonating mixture formed of oxyhydric gas, such as 33.6 liters; 112 calories instead 59, when the so-called cyanogen is decomposed into carbon and nitrogen by the electric spark. Although the carbon begins to be precipitated almost immediately, still the cyanogen does not detonate in consequence of the spark, a fact which demonstrates the slowness of the reaction thus determined. Under other conditions, however, the cyanogen and the acetylene may be decomposed into their elements accompanied by detonation, but it is not by simply heating nor by the action of the electric spark.

I might go on multiplying such facts† which refer to the explosive bodies properly so-called when they are kept at a temperature slightly lower than that which determines the explosion. Silver oxalate, for instance, slowly decomposes at 100°, whereas, at a temperature a little above this it detonates strongly.

5. In brief, all molecular reaction, operated by simple heating at a constant temperature, in the midst of a homogeneous body, are surrounded by conditions which appear identical for all its parts, is

* *Essai de Mécanique Chimique*, Vol. II., p. 17.
† *Annales de Chimie et Phisique*, 4th series, 18, 142.

effected by a characteristic coefficient depending on the length of the reaction. This coefficient varies with the temperature, the pressure, the relative proportions; it plays an important rôle in the study of the powers of destruction among explosive compounds.

6. Let us follow out this explanation. The greater or less duration of a reaction does not change the quantity of heat given off by the total transformation of a given weight of explosive material. But if the gases which are formed expand in volume in consequence of the change of capacity caused by the escape of the projectile, or else by the cooling due to the contact with the walls; under such circumstances I say the initial pressures will be proportionally less than when the transformation of a given weight of an explosive will be of longer duration.

On the other hand, when a very rapid transformation of the entire mass in the midst of a closed vessel, added to the absence of the phenomena of dissociation, permits the initial pressures to reach the extent of their theoretical limits, or to approach them, it would be extremely difficult to make vessels strong enough to retain the gases of explosion.

7. The same state of affairs prevails, not only for an explosive body placed in a fixed and resisting volume, but also for the same body placed in a thin envelope, or beneath a layer of water, or even in the open air. In reality when the length of the reactions decrease beyond measure the gases given off develop pressures which increase with immense rapidity, so rapidly, indeed, that the enveloping bodies—solids, liquids, or gases—have not sufficient time to move and yield gradually to the pressure; these bodies oppose the pressure of the gas with a resistance comparable to that of a fixed wall. It is known that a pellicle of water on the surface of nitrogen chloride is sufficient to produce such results. The more instantaneous the reaction is the more the initial pressure, even in an open vessel, approaches the theoretical pressure, the latter being calculated for a case of decomposition under a constant volume, entirely filled by the explosive substance. It is in this way that we can explain the extraordinary effects of destruction produced by mercury fulminate, nitro glycerine, or compressed guncotton.

8. *As a general thing, any reaction that gives off heat is capable of producing explosive phenomena, provided, however, that it produces gaseous products,* and this for several reasons—First: *The rapidity of the reactions in a homogeneous system,* other things being equal, *increases with the temperature.** It even increases according to a very rapid law, as has already been shown by my experiments on the ethers; † hence the rapidity may be represented by an exponential function of the temperature, a function whose numerical value in the formation of acetic acid is 22,000 greater at 200° than when it is in the neighborhood of 7°. Secondly: *The temperature of the system increases, at least up to a certain limit, in consideration of the effect produced by the reaction.*

Let there be a system capable of giving off heat in consequence of its chemical transformation; if this system is confined in a locality where it can neither give up nor receive the slightest quantity of heat, the temperature of the system will continue to rise without stopping until it reaches a limit defined by a figure which is obtained by dividing the amount of heat given off, by the specific heat of the system. In addition, the rapidity with which this system tends towards this limit will increase in proportion to the extent of the elevation of the temperature already produced by the reaction, is greater.

In a gaseous system confined in a fixed space, the acceleration will become greater still, at least in the beginning, and that in consequence of the influence produced by the pressure, which pressure increases necessarily on account of the elevation of the temperature. For I have established the fact, that, all else being equal, and in operating at a fixed temperature the reactions take place more rapidly in liquid mixtures that in gaseous mixtures; it is especially noticeable that in gaseous mixtures the reactions are the more rapid according as the pressure is greater. ‡ In a word:

---

* *Essai de Méchanique Chimique*, Vol. II., p. 64.
† *Ibid*, p. 93.
‡ *Essai de Méchanique Chimique*, Vol. II., p. 94.

Third. The rapidity of the reaction in a homogeneous system increases as the condensation of the substance progresses, or more simply *with the pressure in the gaseous systems.**

Thus, in an enclosure supposed to be impermeable to heat, the elementary rapidity of the reaction continues to increase, for the double reason that the temperature is continually being elevated and that the pressure of the gas increases without stopping. Nevertheless, the influence of the pressure should be more sensitive at the beginning than at the end of the experiment; provided, however, that the part which is not in combination, continually diminishes until the moment arrives when the proper tension of this part, considered by itself, ceases to increase in consequence of the heating; from that time on, it tends to diminish until it becomes null.

9. *The rapidity of the reactions in a homogeneous system depends upon the relative proportions of the components.*—In operating at a *constant temperature* the combination is generally accelerated by the presence of one or the other of the components.

On the other hand, at a constant temperature, the reaction is retarded by the pressure of an inert substance which has the effect of diminishing the state of condensation existing in the substance.

At a variable temperature the reactions are retarded *a fortiori* by the presence of an inert body, such as for instance the nitrogen of the air, or the silica of ordinary dynamite, this body by absorbing the heat lowers the temperature of the system without producing any special influence to hasten it by its pressure.

The reaction is generally slower at a variable temperature in the presence of an excess of one of the components, than if the operation is effected with equal equivalents, the necessity of heating this excess is more than counterbalanced by its accelerating influence.

It is clear that if the proportion of inert substance is such that the temperature of the system cannot be elevated to a degree necessary for the combination to continue of itself, the reaction will cease to be explosive and perhaps will not be propagated.

By this means the character of an explosive body may be changed—simply mixing it with an inert body. We shall now give some important facts. A seventy-five per cent. dynamite is not as sharp as pure nitro-glycerine, nevertheless, such a dynamite cannot be used for charging shells, for they would explode in the mouth of the cannon by the influence of the initial shock of powder.

Fifty or sixty per cent. dynamite, on the other hand, may be used with empty projectiles and can be fixed without giving rise to any injury to the ordnance.

This is not all, in using sixty per cent. dynamite, the projectile may produce an expression at the point it reaches without requiring any special priming, as for instance when its progress is stopped by a body of considerable resistance, as for instance a plate of iron sheeting, the elevation of temperature caused by this sudden arrest is sufficient to determine an explosion. But if the charge of nitro-glycerine be reduced to thirty or forty per cent. charged with such a dynamite, necessitates the use of a percussion force in order to produce an explosion similar to such as are produced by black gunpowder. It is quite true that such a dynamite presents scarcely any advantages over ordinary gunpowder.

It is an important observation that the rapidity of the burning of an explosive substance diminishes considerably as the proportions of its mixing with an inert body approach the limits of inflammability. It follows then, that as these limits are approached the burning becomes uncertain and the explosive character of the phenomena ceases to be manifested.

10. These general relations were established for such a system that all the heat which it gives off should be used to increase the elevation of the temperature. Let us examine the real condition of affairs, one in which the system gives a portion of its heat to the surrounding bodies by either radiation or conductibility. The elementary rapidity of the reactions, and the mass of the substances used, play an important part in this connection. In reality, on all occasions, when the rapidity of the reactions was

* In liquid or solid systems, the process, on the other hand, exercises but little influence, that is according to my investigations. A circumstance which is explicable because it is produced in consequence of the state of condensation of the material.

not great, a portion of the heat would to a certain extent be dissipated, and the elevation of its temperature soon reaches a fixed limit.

This limit will be one at which the loss of heat produced by the external action is equal to the gain resulting from the internal reactions of the system; in this case the reaction takes place with certain rapidity, constant or almost so, without becoming explosive. Such is the case with priming substances.

This is also the case, in explosives generally slower, of a substauce which is spontaneously decomposed. But if the mass with which the operation is conducted is increased, and supposing it to be confined in a fixed space, the amount of heat lost by radiation or conductivity at a given temperature will vary but little, the entire amount of heat produced internally will be increased.

Thus, the temperature of the system should be higher than the preceding when it tends toward a new limit, or when its growth becomes more and more rapid, and finally explosive in consequence of the correlative growth of the pressures.

This same acceleration, depending on the pressures and the rapidity of the reactions, plays an important role in the interpretation of the effects produced by tamping.

Besides, it is in this means that all deflagrating mixtures may be changed into explosive compounds when the mass continued in a given space is increased.

The difference between the methods of decomposition of an explosive material, according as its mass is greater or less, deserves especial attention, for it is frequently referred to in practical applications.

11. This is observed, even in the case where an exit is opened to the gases of explosion. If the explosive mass is of sufficient size, the decomposition of a deflagrating substance. where gases are given off through a narrow opening, may be changed into an explosion when the opening is made narrower, in such a way that the pressure and the internal temperature may be increased towards a given limit.

The same remark may be applied to spontaneous decompositions, occurring with large masses of matter. Beginning slowly at ordinary temperatures, their rapidity increases under the influence of the temperature which they determine; besides it may happen that this will change the character of the decomposition, by causing a new reaction giving off more heat to follow the initial reaction. The elevation of the temperature of the mass increases and hastens until it produces a violent reaction and a general explosion.

12. These facts which are frequently observed in laboratories, have been called upon to explain the spontaneous explosions of gun cotton and nitro-glycerine. They lead to the belief that an explosive substance which has begun to decompose is particularly dangerous. Such general explosions are produced not only in explosives that are contained in very solid vessels, but also in those which are held in vessels that are slightly resisting, such as boxes of wood or their thin metallic cases and even on substances heaped up in the open air, when the accumulation of substances allows the temperature to be raised of itself and to become more and more accelerated.

They may take place equally as well on substances divided into very small quantities, provided that the particles are sufficiently near to each other so that the mechanical effects may be accumulated and produce a common result.

In their preservation and in use the same precautions should be followed, just as if all the portions of the explosive were collected in a single mass. These are consequences which are theoretically possible and which are often proven to be practically correct by the accidental realization of terrible catastrophes.

13. In fact, the experiments made by the Birmingham Chamber of Commerce relative to the transportation and storage of caps, showed that the capsules, each containing 15 mgrs. of fulminate, will not explode in mass, nor by the influence of a shock, nor when crushed by the wheel of a locomotive, nor when they are placed in the center of an incandescent muffle, or when in the midst of a burning hearth.

But if the weight of the fulminate contained in the capsule is considerably increased, the case becomes different. The security which the first trials excited has created even in England, in conse-

quence of an explosion on the Thames of a boat which was loaded with detonating caps.

Experience has shown that beyond a doubt that the explosion of a single powerful capsule of fulminate is sufficient to cause that of all the capsules placed in the same box; if the box itself explodes the neighboring boxes will detonate equally as well.

It is in consequence of similar phenomena that the small fulminating caps which are sold as playthings to children, have so frequently been the cause of serious accidents.

At Vannes, near Paris, a child was amusing itself by exploding such a cap between the blades of a pair of scissors, two packages of 600 caps each, which were laying on the table close at hand went off at the same moment; the child was killed, the chair destroyed, and the floor injured.

We also add the explosion that occurred in the Rue Beranger, at Paris, on May 14, 1878, which was produced by a mass of fulminating caps that were intended for children's toys. These caps had the following composition: one kind called single consisted of—

Potassium chlorate......	12 parts.
Amorphous phosphorus..	6 "
Lead oxide............	12 "
Resin	1 "

and those called double were made of a mixture of—

Potassium chlorate.....	9 parts.
Amorphous phosphorus	6 "
Antimony sulphide.....	1 "
Sulphur sublimed......	0.25 "
Niter	0.25 "

The latter were more sensitive to friction, and on an average weighed 10 mgrs. each. Six to eight million caps of this description done up in rows of five and pasted on strips of paper, were piled up in the store in boxes containing a gross in each. Some one of these individual caps was set off by an accident, whose origin was never known, and a general explosion ensued. Of a sudden the house was thrown down, its facade destroyed by hurling the trimmed stones out of their positions. A stone, a meter cube in size, was thrown to a distance of 52 meters, a large portion of the adjoining house was also ruined; fourteen persons were killed on the spot and sixteen wounded.

These terrible effects are explained when it is recollected that the explosive material contained in the cap weighed about 64 kilogrms., and according to the composition of the substance that its force was equal to 226 kilogrms. of black powder.*

It is, therefore, of the greatest importance that persons having explosives in their charge should be familiar with these truths and facts, and adopt such precautions as will prevent the explosion of the entire mass.

III.

§ 3.—*Rapidity of the Propagation of the Reaction.*

1. Let us now examine the case of a homogeneous system, but whose various parts are exposed to different condibeing such as those which arise from ock ignited at one point or from a local sh. In order to propogate the transformation in a mass which detonates, and which is not submitted to the same action at all of its points, it is necessary that the same physical conditions of temperature, of pressure, etc., which prevail at one point of action, should successively be produced and propogated, molecule by molecule, through all portions of the mass.

In this connection the numerous works of artillerists are well known † on the rapidity of combustion of ordinary gunpowder, and of that of gun-cotton, a capacity which varies according to the physical structure of the powders and their chemical composition. We shall presently examine these results as well as those observed in explosive mixtures of gases, that is to say the observations bearing on the rapidity of the combustion of mixtures of oxygen and hydrogen, or of nitrogen oxide or gaseous hydrocarbons.

Then we shall give some new and unexpected results furnished from the study of gun-cotton and of nitro-glycerine, the new theory of the functions of caps, the distinction thus far ignored between the

* These facts are taken from the report made the Inquest Commission.

† Piobert, Praste de artillerie, partie theorique.

simple ignition and the true detonation of explosives, a distinction which my recent investigations extend to even gaseous mixtures themselves, and we shall seek to harmonize their differences with theoretical ideas.

2. According to Piobert, the rapidity of the combustion of powder in the open air observed on prisms of known length, placed vertically, and whose lateral faces were greased in order to insure regularity in the phenomena. This rapidity, I say, has been found to be included between 10 and 13 mm. to the second in gunpowder. Otherwise it varies in inverse proportion to the apparent density of the powder.

3. The rapidity of combustion of powder depends to a great extent on the pressure of the air or on the surrounding gases.

Near the end of the seventeenth century Doyle made some experiments on the combustion of powder in vacuo, and observed that grains of powder thrown on a red-hot iron in this condition, melt without detonating. If the operation is conducted with a sufficient number of grains, towards the end an explosion will take place beyond a doubt, because the conditions of pressure are changed.

Huygens repeated the same experiments by igniting the powder with a burning-glass which concentrated the solar rays.

If the heating is progressive, an effect which may be produced by a piece of glowing charcoal then at pressure, the sulphur will be sublimed and the homogeneity of the mixture destroyed, or else according to Hawksbee (1702) the powder will be melted.

These experiments have been frequently repeated with different modifications, such as the employment of a red-hot platinum wire, heated by electricity, and then used to ignite the powder in vacuo.—*Abel.* M. Bianchi has in this manner determined that gun-cotton is slowly decomposed in vacuo before its explosion, and a similar result with nitroglycerine has been reached by Messrs. Heeren and Abel.

Mercury fulminate, on the other hand, detonates in vacuo when brought in contact with a piece of brass wire which has been heated red-hot, but the detonation does not extend to the grains which are not contiguous to it, as it does when under atmospheric pressure.

4. Not only does a vaccuum reduce the explosive qualities of gunpowder, but any diminution in the pressure retards it. In 1855, Mitchell observed that fuses burned slower at high elevations; M. Frankland in 1861, at his laboratory, and then M. de Saint Robert on the Alps, have made very exact determinations in this line, under the pressures included between 722 m.m. and 405 m.m., according to the researches of M. de St. Robert, rapidity of combustion of the powder under less than atmospheric pressure would be represented for all practical purposes by a formule such as $V=Ap^{\frac{2}{3}}$.

A being a constant and p expressing the pressure. These results should be attributed to the greater or less rapidity with which the heated gases escape before having had time to heat the neighboring portions of the solid matter, which is equivalent to saying that the pressure diminishes the number of gaseous particles carried up to a high temperature, that come in contact at each instant with the solid particles not yet ignited, and share with them their living force in a way so as to produce an equilibrium of temperature.

Whatever may be the pressure, the initial temperatures of these particles is pretty much the same at constant volume, at least so much as has not been modified by the chemical reaction. But if one operates under a constant pressure, it is otherwise, for the temperature is lowered in accordance with the detention of the gases.

5. On the other hand, the quickness of combustion of powder increases with great rapidity, as soon as it attains the heavy pressures which are produced in cannons and in guns; thus, for instance, Captain Castan reckons the rapidity of the combustion of powder in the bore of cannons of large calibre at 230 mm. per second, instead of 10 mm. in the open air.

6. The rapidity of combustion of other explosives has not been the subject of experiments as exact as those applied to black powder; it likewise suggests new observations and a theory of an entirely different kind, as we shall immediately explain. We confine ourselves to the statements that Piobert determined the rapidity of combustion of gun-cotton (not compressed) as eight times that of

gunpowder, a value which may be applied to a progressive combustion taking place without detonation.

7. These same studies were extended to explosive mixtures of gases. In 1867, M. Bunsen * determined the rapidity of combustion to be 34 m. to the second for detonating gas (hydrogen and oxygen) and of one meter only per second for a mixture of equivalent parts of carbon monoxide and oxygen. These mixtures being taken at atmospheric pressures. He determined the delivery through a small orifice by igniting the jet, and determined for what rapidity as a limit of flow the flame remained stationery at the opening without going back into the interior. M. Mallard † has made similar experiments in different mixtures of marsh gas or of illuminating gas and of air; he found that the rapidity of combustion, defined as above, rapidly diminishes in proportion as the amount of gas having no part in the combustion increases. The maximum rapidity corresponding to 0.560 m. each second for a mixture of eight parts of air and one part of marsh gas by volume. It lowers itself to 0.04 m. with a mixture containing twelve parts of air to one of marsh gas. With illuminating gas and air, the maximum rapidity has almost reached double this amount mm. Mallard and Le Chatelier have restudied this question by other processes which have given them results entirely different according to the method of combustion. They will be referred to presently and the causes of these differences will be shown.

8. In reality the study of the new explosives, gun-cotton and nitro-glycerine, lead to a better understanding in the knowledge of the means of propagation of the chemical reaction in the midst of a mass during combustion, and it has greatly modified the ideas which have been held on this subject. Formerly, when black powder was the only known explosive, its ignition was all that demanded attention, the effects of the explosion that followed did not appear depependent on the process of ignition. But nitro-glycerine and dynamite have shown singulur differences in this connection.

9. In order that these may be perfectly understood, it is necessary to first consider those phenomena of shock and other analogous causes capable of producing a deflagration.

The shock will hardly produce by itself the decomposition if a substance absorbs heat, unless we refer to colossal masses animated by enormous living force, and which are concentrating all their action on a very small quantity of matter, something which is very difficult to produce. For instance, the living force of a weight of 1630 kilograms falling from the height of a meter, would be necessary to decompose one grain of water, that is by supposing that it would be possible to transmit to a grain of water, by any means, the entire amount of this living force.

On the other hand, if the decomposition of the substance gives off heat, one would suppose that a living force which was limited would be sufficient to produce it, provided that it was in condition to be applied completely to a very small quantity of substance which it raises to the degree of temperature necessary in order to determine the reaction.

Thus, for instance, several strokes of a hammer violently struck on some powdered potassium chlorate, wrapped up in a sheet of platinum and placed on an anvil, is sufficient to produce traces of potassium chloride that are quite perceptible; while potassium sulphate will give no indications of decomposition under the same conditions. But it must be remembered that the decomposition of potassium sulphate into potassium sulphide and oxygen absorbs heat, while the decomposition of potssium chlorate into potassium chloride gives off heat, (11,000 calaries for potassium chlorate).

10. This condition is, however, not sufficient to cause a shock to produce detonation. It is still necessary that the live force, developed by the decomposition of the first portions, should be communicated to adjoining portions, in such a way as to determine, step by step, the decomposition of the entire mass. The shock from the hammer which is not sufficient to produce these conditions with pure potassium chlorate is, on the other hand, efficacious with nitro-glycerine. The fall of a weight 4.7 kgrams. from a height of 0.25 meter is sufficient to cause the explosion of a single drop of nitro-

* *Annales de Physique et de Chimie*, 4 Serie t. 14, p 449.
† *Annales des Mines*, t. 8, 3c. lierason, 1871.

glycerine occupying a surface of 2 c.cm. square.*

Nitro-glycerine mixed with infusorial earth constitutes dynamite, a substance which is but slightly sensitive to shock because of the porous and cellular structure of the silica prevents to the immediate and local communication of the living force of a very slight quantity of nitro-glycerine separated from the rest.

Besides, the explosion of black powder will cause the nitro-glycerine to detonate, but it will not lead to any explosion of dynamite, at least in the open air and in weak charges. But this inertia disappears under the influence of certain shocks, particularly violent, such as that of mercury fulminate. The explosion of nitro-glycerine varies according as it is pure or mixed with some other body, whether it is effected by a simple shock, by the contact of a body in feeble ignition, or in strong ignition, or an ordinary match, or else by the contact of a strong mercury fulminate cap.

IV.

1. According to the process used for igniting, the dynamite may be decomposed quietly and without flame, or else it may burn with considerble vivacity, or else produce a detonation, properly so called, which may be sometimes moderate, sometimes capable of dislcoating rocks, sometimes even of destroying them *in situ*, and producing the most violent effects.

2. The substances, which are the cause of these last-named results, have been specially designated as *detonators.* M. Nobel was the first to observe these effects on nitro-glycerine (in 1864), and he then deduced a suitable process by which it could be made to detonate with certainty by means of a cap containing mercury fulminate. Gun-cotton does not show any less difference. M. Abel has published in this connection since 1868, many very curious experiments, and which tend to establish a great diversity between the conditions of deflagration of this substance varying with the method of ignition.† M. Roux and Sarran have generalized these phenomena by distinguishfng what are known as *explosions of the first and of the second order.*

3. However strange this diversity may appear at first sight, I nevertheless believe that the thermo-dynamic theories are capable of accounting for it, by a suitable analysis of the phenomena of the shock.

In truth, the diversity of the explosive phenomena depends upon the rapidity with which this reaction propagates itself, and the more or less intense pressure which results from it.

Let the case be a more simple one, such as an explosion caused by the fall of a weight from a certain height. At first one would be disposed to charge the effects observed to the heat given off by the pressure due to the shock of the weight suddenly arrested. But calculation shows that the arresting of a weight of several kilogrammes, falling 0.25 m., or 0.50 in height, would not be capable of raising the temperature of the explosive mass more than a fraction of a degree, if the resulting heat was dispersed uniformly throughout the entire mass; this would not then attain an elevated temperature, that of 190 degrees, for instance, for nitro-glycerine, a temperature to which it appears necessary to suddenly raise the entire mass in order to produce an explosion.

It is by another mechanism that the living force of the weight which is transformed into heat becomes the origin of the observed effects.

It is sufficient to admit that the pressures which arise from the shock exercised on the surface of the nitro-glycerine being too rapid to become uniformly dispersed throughout the entire mass, the transformation of the live force into heat takes place especially among the first layers reached by the shock. If it is sufficiently violent it may thus be rapidly elevated to 200°, and they will be immediately decomposed and producing a large quantity of gas. The production of gas is in its turn so violent, that the shocking body has not had time to displace itself, and that the sudden detention of the gases of explosion produces a new shock probably more violent than the first on the layer situated below. The living force of this same shock is changed into heat in the layers

* Ch. Girard, Millot et Vogt, *Comptes Rendus de Seances de l'Academie des Sciences*, tome 71, p. 391

† *Comptes Rendus des Seances de l'Academie des Sciences*, tome 69, p. 105–121. 1869.

which it first reaches. It produces the explosion and this alternative between a shock, developing a live force which changes into heat and a production of heat which elevates the temperature of the heated layers up to the degree of a new explosion capable of reproducing a shock; this alternative, I say, propagates the reaction, molecule by molecule, through the entire mass. The propagation of the deflagrating takes place this way in consequence of phenomena comparable to those which produce a sonorous mass, that is to say, by producing a real explosive which advances with a rapidity incomparably greater than that of a simple burning provoked by the contact of a body in ignition and operating under conditions where the gases expand freely in proportion to their production.

4. This is not all. The reaction started by the first shock in a given explosive material is propagated with a rapidity which depends upon the intensity of the first shock, provided that its living force, changed into heat, determines the intensity of the first explosion, and in consequence that of the entire series of consecutive effects. For the intensity of the first shock may vary considerably, according to the method by which it is produced. The effect of a blow from a hammer may vary in its duration—for example—from the one-one-hundredth to the one-ten-thousandth of a second, according as one strikes with a hammer having a flexible handle, or with a block of steel, that is according to the experiments of M. Marcel Duprez. From this it may be seen that the explosion of a solid mass or a liquid may develop itself according to an infinite number of different laws, each one of which is determined, all other things being equal by the original impulse. The more violent the initial shock, the greater will be the resulting violence of the decomposition and the greater will be the pressures which are exercised during the entire converse of this decomposition. One and the same explosive substance may produce very different effects according to the method of ignition.

5. The effects likewise differ as the substance is pure or mixed with a foreign substance, and in accordance with the structure of the latter. This feature is shown by dynamite, a mixture of nitroglycerine with silicon, which has lost the greater part of its sensibility to an ordinary shock, but remains explosive to the shock of a ball, and above all, to that of mercury fulminate.

The addition of a few per cent. of camphor to dynamite will still further diminish its explosive qualities to such an extent even, that it will no longer detonate except with strong fulminate caps.

6. Gun-cotton, impregnated with water, or with parafine, becomes likewise insensible to a shock; for its detonation, then it requires the use of a small supplementary cartridge of dry gun-cotton, itself charged with fulminate.

If several per cent. of camphor are mixed with nitrated cellulose, its susceptibility to explode by a shock is almost completely destroyed, at least at ordinary temperatures; to such are that this mixture forms a substance which is used to-day for many purposes in the arts under the name of *celluloid.*

7. The dynamite given, which results from the combination of nitro-glycerine with collodion, sometimes with camphor added, also forms an elastic mass which is only slightly sensitive to shock, and it also requires an auxilliary cartridge of dry gun-gotton, itself charged with fulminate.

8. The change brought about by the addition of camphor and resinous substances to the explosive power of similar substances, is the result of a modification brought about in the cohesion of the mass. This has acquired a certain elasticity and a solidity of parts, in consequence of which the initial shock of the detonator propagates itself at first in a much greater mass. Besides a portion of the effects are expended by the work of tearing up and separation, there still remains the smaller portion which is capable of producing heat in the parts directly struck, this heating, however, being dispersed through a larger mass.

Therefore, a sudden elevation of temperature at one spot, capable of producing consecutive chemical and mechanical action can only be produced with difficulty; it requires the employment of much greater weight of the detonator. This follows always in consideration of the preceding theory.

9. But camphor, on the contrary,

should not produce, and does not, as experience goes to prove, any specific action on a discontinuous powder, such as the potassium chlorate powders. It is on this account that it is necessary, equally as well to take into consideration that frozen dynamite jelly possesses a sensibility to shock comparable to that of nitro-glycerine if the solidity of the parts have become destroyed by the crystallization of that substance.

10. The importance of *caps* may be clearly seen from the foregoing. Up to the present time they have been regarded simply as agents, seeming to communicate the ignition to the powder. In reality, these caps, as long as they are of sufficient size regulate by their nature the character of the initial shock, and in consequence the character of the entire explosion. In this case they receive the name of *detonators*, properly so called. Mercury fulminate is used for this purpose particularly, on account of its being more powerful, that is to say, its shock is more violent and more sudden than that of any other substance, which is explained by the greatness of the pressure which it develops by detonating within its own volume (nearly 40,000 atmospheres).

We have given above a certain number of characteristics relative to the spinal influences of caps. We shall return to this subject.

V.

§ 4.—*Burning and Detonation.*

1. The term *burning* is specially given to progressive combustion, the expression *detonation* being reserved for rapid and almost instantaneous combustions. Hence we obtain the distinction proposed by M. Sarrau between *detonations of the first order*, such as those of black powder, whose detonation is the starting point of the series of burnings, and the *detonations of the second order*, or detonations proper, such as those of nitroglycerine induced by a powerful cap of mercury fulminate. However, the known facts do not, in my opinion, oblige us to admit of a difference of nature or of a sharp line of demarkation between the two varieties of phenomena. They tend rather to cause them to present an indefinite variety, included between the two extreme limits, as follows:

First. *The detonation of the explosive in its own volume*, reaching the maximum of temperature and of pressure, and in consequence, the maximum of rapidity of which the chemical reaction taking place under these conditions is susceptible. This detonation is specially induced by a very quick shock. The gases formed at the point where the shock is produced has not, so to speak, the time to be displaced, and so communicate their living force to the parts in contact; the action is thus propagated throughout the entire mass with a sort of regularity. It is to this order of detonation that the rapidity of propagation belongs, so different from that of the combustion of black powder, which has been measured in comparison with dynamite and compressed gun-cotton. For instance, Austrian artillerists have noticed a rapidity higher than 6,000 meters a second in causing the detonation of a cylinder of dynamite 67 meters long. Colonel Sébert has observed rapidities of 5,000 to 7,000 meters in gun-cotton, powdered and compressed in long tubes of lead. Further on it will be seen that I, myself, measured with M. Vielle rapidities of several thousands of meters per second in mixtures of detonating gases taken at the ordinary pressure and contained in tubes of iron, of lead, or even of rubber.

Second. *The progressive burning* propagating itself from particle to particle in conditions where the cooling due to external agents lowers the temperature to the lowest degree compatible with the continuation of the reaction.

It is to this order of burning that we refer the rapidity of combustion of the detonating gases to, as measured by Bunsen. In the case of solid or liquid explosions, the propagation of a simple burning is rendered more difficult than otherwise by the movements of the gases which expand to a considerable extent around the point which is set on fire; instead of acting in an equal volume or in one slightly different from that of the primitive body, their temperatures are thus reduced by distribution through a greater mass of matter. The temperature is frequently found to be dispersed by the gases without giving rise to a total combustion or even undergoing any change. This is particularly the case

with explosions which are not forced into an envelope to concentrate the action of the gases and give them a common resultant.

This is the case with the nitro-glycerine which is found unaltered almost *in situ* in progressive explosions; such is likewise the case with dynamite that is laid along the ground in a thin layer. Damp gun-cotton that is not inflammable in the cold, also furnishes a number of illustrations of this dispersion, resulting from the use of an insufficient detonation. In is, in consequence of this reaction of the gases, that it is deemed advisable to present the simple burning of dynamite in caatridges from preceding the action of the fulminate.

2. Between these two limits there is observed an entire series of intermediate stages of an unlimited number, as is shown by the different methods of burning of dynamite, and the influence of tamping which allows the transformation from burning into a real detonation, if the tamping is sufficiently resisting. Finally we may cite the inequality of the effects produced by the successive explosions of the charges of the same agent, which detonate by influence within limited distances beyond which the explosion will not propagate itself.

3. We must also refer to chemical phenomena. That of decomposition prevails when the explosive substance contains sufficient oxygen to experience a complete combustion as occurs for nitro-glycerine and nitro-dynamite; besides it is necessary that this total combustion shall have actually taken place, which does not necessarily happen, especially in slow burnings performed at a temperature as low as possible.

4. But it often happens that the oxygen is insufficient, or that the first reaction gives rise to a wasteful expenditure of this oxygen as is the case when nitro-glycerine burns slowly, with the production of nitrous vapors and of fixed or gaseous substances incompletely burned. Under these circumstances the possible decompositions are numerous; their number depends on the temperature, on the pressure, and on the rapidity of the heating. We have already remarked upon this in the case of ammonium nitrate; it may be observed in general inorganic substances, decomposed by heating. (*Essai de Mecan. Chimique*, t. II., p. 45.)

5. Among these decompositions those which develop the most heat are those which produce the most violent explosive effects, other conditions being equal. This fact is evident when the volume of gas (reduced at 0° and 0.76°) reaches in the same time its maximum value. But it is also verified in other cases, the dissociation giving rise always to a diminution of pressure, as I have shown elsewhere. On the other hand, this fact does not obtain as a general thing in reactions which are produced at the lowest possible temperature. If then, the explosive body receives in a given time a quantity of heat insufficient to carry the temperature up to a degree, which corresponds to the most violent reaction, it will experience a decomposition capable of giving off less heat, or even of absorbing heat; and it is capable of destroying itself completely by this decomposition without developing these explosive effects which are most energetic.

The opposite will take place if the body is quickly heated to the temperature corresponding to the most energetic reactions.

6. Finally the multiplicity of the possible reactions carries with it a series of intermediate effects, and this all the more, because, according to the mode of heating, it may happen that several decompositions will follow each other progressively. This succession of decompositions gives rise to effects which are very complicated, as has been noticed by M. Jungfleish, when the first decomposition, instead of producing a total elimination of the decomposed portions (changed into gaseous or volatile substances) produces a division of the primitive substance into two parts: the one gaseous, which passes away; the other solid or liquid, which remains exposed to the consecutive action of heating. The composition of this residue being no longer the same, as is the case, for instance, with nitro-glycerine which first loses a portion of its oxygen in the form of nitrous vapors, the effects of its consecutive destruction may become completely changed.

7. Such are the causes, some chemical, others mechanical, through which nitro-

glycerine and compressed gun-cotton produce all of these different effects, according as they are ignited by the aid of a body in feeble combustion, or by a flame, or by an ordinary cap, or else by means of a cap charged with mercury fulminate.

For example, M. M. Roux and Sarran have found that the charge necessary to break a shell, other things being equal, will vary in an inverse ratio of the following numbers; these numbers being referred to gunpowder, taken at unity—

	Detonation.	Burning.
Nitro-glycerine	10.0	4.8
Compressed gun-cotton	6 5	3.0
Picric acid	5.5	2.0
Potassium picrate	5.3	1.8

The weight of the breaking charge in the case of gunpowder itself, under the influence of nitro-glycerine primed with fulminate, is capable of being reduced in the ratio of 4.34 to 1.

This inequality in the force of different powders is partially attributed to the cooling which is effected by the walls during a slow reaction, and also in part to the changed chemical condition.

8. The diversity is less marked with the non-compressed gun-cotton, because the influence of the initial shock is exercised on a smaller quantity of matter, and above all, because the propagation of the successive reactions in the mass develops their initial pressures weaker, and a less direct transformation of living force into heat, is transmitted to the explosive body on account of the air which is interposed and in consequence the explosive wave can hardly be generated.

Compressed gun-cotton itself, is not so compact as nitro-glycerine, because of its structure; on this account the pressures due to shocks might be sensiby weakened by the existence of interstices. Gun-cotton also detonates with more difficulty than nitro-glycerine. The nitro-glycerine will detonate by the fall of a weight from an insignificant height, that is, provided a cap charged with gun-cotton or a mixture of fulminate and of potassium chlorate, etc., is used; while on the other hand, gun-cotton does not explode by the influence of nitro-glycerine, nor by the influence of a mixture of fulminate and of chlorate. It requires the more violent shock of pure mercury fulminate. Besides, this is less efficacious if it is used unenclosed than when confined in a thick envelope of copper or tin plate; it is less powerful in an envelope of paper or tin-foil than it is when wrapped in copper: it is still less powerful if the cap is not in direct contact with the gun-cotton. Finally, if it is placed in the tube of a feather its effect will be annulled. Nitro-glycerine does not detonate as well when exposed to the influence of a fulminate force, if it is ignited before the explosion of the fulminate, this preliminary ignition having the effect of producing a certain vaccuum between the two.

The absence of an immediate contact between the dynamite contained in the cartridges and the fulminate cap is objectionable for the same reason, the shock being weakened in part by the interposed air. The sensitiveness, the action of the fulminate which contains the liquid nitro-glycerine, than in that which contains frozen nitro-glycerine which may also be explained as due to the lack of homogeneity of the frozen dynamite in which the nitro-glycerine is partly separated from the porous silica in consequence of its solidification.

9. All these phenomena may be explained by the more or less important value of the initial pressures, and by their more or less rapid development; that is to say, by the conditions which regulate the living force transformed into heat within a given time, into the midst of the first layers of the explosive substance reached by the shock.

The quantity of living force thus transformed depends first upon the sharpness of the shock and also upon the amount of work which it may produce; these are the factors which vary with every explosive material. For instance, it is not always that the most sensitive caps are those which produce the most instantaneous explosion. M. Abel has observed that nitrogen chloride is not particularly adapted for igniting gun-cotton; nitrogen-oxide, so sensitive to the least friction, remains entirely powerless to explode gun-cotton. Now, nitrogen chloride is precisely one of these explosive bodies which we are now discussing, which develops less heat and in consequence less work for a given weight. We conclude, then, that it cannot be advantageously employed for caps or priming. As

to nitrogen-oxide, according to the analogies drawn from the iode substituted compounds (see *Annales de Chemie et de Physique*, 4 series, vol. 20, p. 449) its explosion should develope still less heat and work for the same weight than the nitrogen chloride. Its weakness is therefore easy to comprehend.

VI.

1. The phenomena which we have described has had reference to solid or liquid explosives, but the gaseous compounds and the detonating mixtures of gases give similar results, these throw a still greater light on the theory. In reality the chemical transformation of such a gaseous mixture may act with very different rapidities according to the mode of propagation of the decomposition or of the burning.

2. Let us begin with a compressed gas formed with the absorption of heat according to its elements, such as hypochlorous and ($Cl+O=ClO$ absorbs -7.6), acetylene ($C_4+H_2=C_4H_2$ absorbs—61.5) or cyanogen ($C_4+N_2=C_4N_2-74.5$.

Such a gas is decomposed in inverse proportion to the liberation of heat. This, for instance, is what we obtained with the hypochlorous gas, heated below 100° or traversed by an electric spark, or placed in contact with a body in ignition; the gas detonates at once, reproducing chlorine and oxygen. But it is not the same with acetylene nor with cyanogen. These gases neither detonate by the influence of heating nor by the influence of the electric spark, although they are decomposed by them, little by little, and without exploding. On the other hand, I have found that a quick shock of mercury fulminate will cause them to detonate rapidly with considerable flame, separation of their elements, carbon and hydrogen in the case of acetylene, and carbon and nitrogen in the case of cyanogen. The experiments have recently been described in the present work.*

3. The explosive mixtures formed by the union of oxygen with a combustible gas, may also burn with extremely varying rapidities, according to the method of the propogation of the chemical reaction.

We have cited the experiments of M. Bunsen who thought it possible to fix the rapidities of combustion of a mixture of hydrogen and oxygen gas, in equivalent proportions, at 34 meters per second, and that of a mixture of carbon monoxide and oxygen at only a meter. These are experiments based on the retrogradation of the flame back into the mixture, while flowing out into the atmosphere through a narrow opening. Having recently undertaken these experiments with M. Violle under different conditions, we have observed rapidities incomparably greater. Our method of operation was as follows:

On each occasion we filled, under atmospheric pressure with an explosive mixture, an iron tube 5 meters long, having an internal diameter equal to 8 mm., capable of being sometimes kept open and sometimes closed at its extremities, or else a lead tube 40 meters in length, or sometimes an thick india-rubber tube 40 meters long. By means of a special arrangement we are able to register the passage of the explosive wave, first at the beginning of the tube, then further on, and finally at the end of the tube,* and also we were able to measure the time elapsing between these different passages. These experiments were made with the tubes which were sometimes opened at one extremity and sometimes closed; on some occasions they were placed horizontally, and on others they were arranged vertically, and under different pressures. They had proved to us that the detonation is propagated with a rapidity equal to thousands of meters per second, as well for the mixture of hydrogen and oxygen as for the mixture of carbon monoxide and oxygen.

4. The difference between these results and those which were obtained by M. Bunsen may be explained by the variance in conditions. The gases burned in the earlier experiments were cooled by contact with the air, and the explosive wave was not produced. The difference between the two kinds of combustion appeared analogous to that which exists between the simple burning of explosive substances, an operation in which the movement of the different particles takes place confusedly and in an independent manner, and their sudden detonation, provoked by a fulminating cap, an opera-

* See *Comptes Rendus*, 93, p. 240.

* *Comptes Rendus*, 93, p. 19.

tion in which the movements become coordinated. Hence the effects of temperature and of pressure attain their maximum, and are propagated with an incomparably greater rapidity. Some of these observations made with the fire-damp of mines seems to bear an analogous interpretation.

5. The characteristic feature of this order of phenomenon is the production of an explosive ware, that is to say, of a certain regular surface where the transformation is produced and which brings about a similar state of combination, of temperature, of pressure, etc. This surface, once produced, propagates itself layer by layer throughout the entire mass, in consequence of the transmission of successive shocks of the gaseous molecules carried to a vibratory condition more intense in consequence of the heat given off in their combination and transformed *in situ*, or more exactly with a feeble relative displacement. Analogous phenomena may be developed with solid and liquid explosives in confirmation with what has been said above.

6. These effects are comparable to those of a sound wave; but, however, with this important difference, that the explosive phenomena does not reproduce itself periodically, that is to say, it starts a single and characteristic wave, whereas the phenomena of sound is reproduced by a periodical succession of equal waves.

There is, moreover, this important difference that the living force of a system of molecules, whose collection forms the sound wave, remains sensibly constant during the propagation of the wave, and that it is slight, whilst the living force of the system of molecules which constitutes the explosive wave is enormous, and that it begins at once by growing and tends towards a maximum, which is determined by the very highest temperature that the system can reach due to the chemical transformation actually realizable. In fact this maximum is never reached, in consequence of the conditions of cooling, but it is more nearly approached as the reaction is more rapid, is carried on in a medium more condensed and on a greater mass.

7. The propagation of successive shocks between the ultimate molecules of bodies leads us to push further the comparison of the mechanical effects and the thermal effects which are developed simultaneously. In reality the living force communicated to this order of molecules by the chemical combination is nothing else than the heat itself given off in the reaction and the pressure exercised on the molecules themselves and on the walls of the vessels, is the immediate phase of transformation according to present theories. We have reached, therefore, a point where the two orders of ideas tend to confound themselves.

8. It follows from these explanations that the rapidity of the propagation of the explosion becomes comparable to the rapidity of sound, which is also propagated in virtue of an undulatory movement, the rapidity of these two movements being of the same order as the rapidity of translations of the gaseous molecules.

9. It is possible to define this point of view by observing that the rapidity of the translation of the gaseous molecules is equal, according to the formulas of M. Clasius to 29.354 meters $\sqrt{\frac{T}{P}}$ per second. T in this instance represents the absolute temperature $(273+t)$, p the density of the gaseous mixture in terms of that of air. Let $T=3000°$, a temperature whose development* may be admitted in the gaseous mixtures which we are considering taken at the normal pressure. The actual rapidity of the translation of the gaseous molecules would be included between 1,300 meters and 1,600 per second, according as one operates on carbon dioxide or on a mixture of carbon monoxide and oxygen, or on a dissociated mixture containing these different compounds. It would be included between 2,000 and 2,500 meters per second for steam or its compounds. These figures may furnish the first term of the comparison, though we must not forget that explosive phenomena are more complex than a simple movement of translation, or even than the propagation of a sound wave.

VII.

Explosions by Difference.

1. Thus far we have regarded the development of the explosive reactions

* *Ann. de Chem. et de Phys.*, 5 Series 12, 309.

either from the point of view of their duration in an homogeneous system in which all the parts are maintained at the same temperature, or else from the standpoint of their propagation in a system equally homogeneous, to which fire is applied directly by means of a body in ignition or else by a violent shock. In these last years, however, the study of explosive substances has revealed the existence of another method of propagating the reactions in an explosive center, this propagation taking place at a distance and by the intermediation of the air or certain solid bodies which do not themselves participate in the chemical change.

We now wish to speak of what are called explosions by influence, whose existence was formerly suspected from certain known facts relative to the simultaneous explosion of several buildings separated by considerable space from each other, as in catastrophes occurring in powder mills.

Attention has been specially directed to this class of phenomena by the study of nitro-glycerine and gun-cotton.

2. We will begin by giving the most important characteristic facts. A dynamite cartridge made to detonate by means of a fulminate cap causes the adjoining cartridges to detonate, not only by contact and by direct shock, but even from a distance. In this way an indefinite number of cartridges, arranged in a regular course, may be made to detonate.

3. The distances to which the explosion may be propagated are relatively great. Thus, for instance, cartridges being contained in rigid metallic envelopes and placed on a resisting soil, the detonation produced by 100 grains of Vonges' dynamite (75 per cent. nitro-glycerine, 25 per cent. randanite, that is to say, very finely divided silica) communicates itself 0.3 meters of distance, according to the experiments of Captain Corille. D being equal to the distance in meters, and C the weight of the charge in kilograms, the experiments of this officer show that $D=3.0C$.

When the caps were laid on a rail, D was found to be equal to $7.0C$.

On soft or ploughed up earth the distances, on the contrary, are less.

When a cartridge is suspended in air there is no detonation by influence, perhaps because the cartridge not being fixed can recoil freely, which diminishes the violence of the shock. Nevertheless, there are experiments which show that the air suffices for the transmission of the detonation by influence, although with greater difficulty and requiring a greater mass of the explosive.

With a dynamite less powerful in nitro-glycerine (55 per cent. of nitro-glycerine and 45 per cent. of the argillaceous ashes of boghead coal), contained in similar cartridges, and placed along the ground, the experiments of Captain Pamard have given the smallest distances: $D=0.90c$. If metallic envelopes having less resistance are used, the distance at which the explosion is propogated is likewise diminished. Dynamite simply spread along the ground ceases to propagate the explosion. The experiments performed in Austria have given similar results. They have shown that the explosion is communicated either in the free air with intervals of 4 cm., or else through pine boards 18 mm. thick. In a lead tube with a diameter $=0.15$ meters and a meter in length, a cartridge that is placed at one extremity has caused the detonation of a cartridge at the other end. The explosion is still better transmitted through tubes made with wrought iron. Couplings of the tube diminish their aptitude for transmission.

4. An explosion which is propagated in this manner will go on weakening itself from cartridge to cartridge, and even change its character. Thus, according to the experiments made by Captain Müntz at Versailles, in 1872, a first charge of dynamite, exploded directly, excavated a funnel-shaped hole in the ground with a radius of 0.30 meters; the second charge, detonated by influence, produced an opening of only 0.22 meters; the effect of the detonation was then reduced. This reduction should manifest itself towards the limit of the distance at which the influence ceases. In the same way four tin sieves were taken and located at 40 mm. apart, and against each of them a small cylinder of gun-cotton was placed and the entire affair arranged on a board. 15 mm. in frout of the first sieve a similar cylinder was detonated. All of the cylinders detonated, but a progressive

diminution was observed in the excavations produced in the board below each cylinder. According to these facts the propagation by influence depends at the same time on the pressure acquired by the gas, and on the nature of the support. It is not even necessary that it should be rigid.

5. Finally, in operating under water at a depth of 1.30 meters, a charge of 5 kilograms of dynamite brought on an explosion of a charge of 4 kilograms, situated at a distance of 3 meters. The water then transmits the explosive shock, at least to a certain distance, as does a solid body. This transmission is so violent that the fish are killed in ponds without the sphere of a certain radius by the explosion of a dynamite cartridge, a process which is frequently employed to fish a body of water, but which is objectionable as depopulating the stream.

6. Similar experiments have been made by M. Abel, with compressed gun-cotton. According to his observations the explosion of the first block determines that of a series of similar blocks. The propagation under water has likewise been studied; the explosion of a torpedo charged with fulminating cotton caused the detonation of adjoining torpedoes placed within a certain radius of activity. The sudden pressures transmitted by the water when measured by means of the compression of lead at different distances, such as 2.50 m., 3.50 m., 4.50 m., 5.50 m; they go on decreasing, as would be expected. Besides, experiment has shown, that the relative position of the charge and of the "crusper" is of no consequence, which is unformidable to the principle of equal transmission in all directions of hydraulic pressures.

7. Explosions of fulminating substances belong to this same order of explosions by influence, which are rapidly propagated to a great number of caps. We have previously cited the explosion in the Rue Beranger. The experiments which M. Sarran made on that occasion showed that caps of the description which produced this catastrophe may be successively burned in a fire without giving rise to a general explosion; whereas the explosion of a few of these same caps, each containing 10 milligrammes of explosive material, if it is provoked by a rapid pressure, determines by influence the explosion of the adjoining packages, even when they are not contiguous and are situated at a distance of 15 centimeters apart. A general explosion may thus easily be produced by influence.

8. It follows then from these facts, and especially from the experiments made under water, that the explosions by influence are not due to a burning, properly so called, but to the transmission of a shock arising from the enormous and sudden pressures produced by the nitro-glycerine on gun-cotton.

Let us enlarge upon this explanation; it is the same fundamentally as that which we have already shown accounts for the influence of the shock which determines the direct detonation of explosive substances.

9. In an extremely rapid reaction, the pressures may approach to the limit which corresponds to the matter detonating in its own volume, and the commotion due to the sudden development of almost theoretical pressures can be propagated both through the ground and by supports as intermediary or through the air itself, projected *en masse*, as has been shown by the explosion of certain powder factories and of gun-cotton magazines, and even by some of the experiments with dynamite and compressed gun-cotton. The intensity of the shock propagated either by a column of air or by a liquid or solid mass, varies with the nature of the explosive body and its mode of inflammation; it is of greater violence according as the length of the chemical reaction is shorter and develops more gas, that is to say, a higher initial pressure, and more heat, that is to say, work, for the same weight of explosive material.

10. This transmission of a shock is conveyed better by solids than by liquids, better by liquids than by gases; with gases it is better, as they are more compressed. Through solids it is better propagated according to their degree of hardness, iron transmitting it better than earth, and hard ground better than ploughed soil.

All breaks of continuity in the transmitting material tend to weaken it, especially if a softer substance is inter-

posed. It is in this manner that the use of a tube as a receiver, made from a goose quill, stops the effect of mercury fulminate, while a tube or a capsule of copper transmits this effect in all its intensity.

The explosions by influence are the better propagated in a series of cartridges according as the envelope of the first detonating cartridge is the more resisting, which allows the gases to attain a greater pressure before the covering is destroyed.

The existence of an empty space, that is to say, filled with air, between the fulminate and the dynamite, on the other hand diminishes the violence of the shock transmitted, and in consequence that of the explosion; generally the effects of breaking powders are lessened when there is no contact.

11. To form a full conception of the transmission of sudden pressures which produce shock by the supporting medium, it is desirable to recall this general principle, in virtue of which in a homogeneous mass, pressures are transmitted equally in all directions and are the same on a small element of surface whatever its position. Detonations produced under water with gun-cotton show that this principle is equally applicable to the sudden pressures which produce the explosive phenomena. But it ceases to be true when one passes from one medium to another.

12. If the inert chemical matter which transmits the explosive movement is fixed in a given situation on the surface of the ground, or better, on the surface of the rail on which the first cartridge was placed, or better still, held by the pressure of a mass of deep water, in the midst of which the first detonation is produced, the propagation of the movement in this matter will hardly be able to take place except under the form of a wave of a purely physical order, and in consequence of a character essentially different from the first wave of a chemical and physical order, at the same time developed in the explosive body itself. This new wave propagates concussion away from the explosive center, all around it, and with an intensity which decreases inversely as the square of the distance. Even in the neighborhood of the center, the displacements of the molecules may break the cohesion of the mass and disperse it, or crush it by enlarging the chamber of explosion, if the operation is conducted in a cavity. But a very short distance, and of which the magnitude depends on the elasticity of the surrounding medium, these movements, confused at the beginning, regulate themselves in order to produce a wave, properly so called. Characterized by compressions and sudden deformations of the material, the amplitude of these oscillations depend upon the magnitude of the initial impulse. They move with a very great rapidity, and preserve their regularity up to the point where the medium is broken. Then these compressions and sudden deformations change their nature and are transformed into a movement of impulse, that is to say, that they reproduce the shock. If then they act on a new cartridge they may determine its explosion; the shock will be otherwise weakened by the distance, and in consequence the character of the explosion may be modified. The effects diminish in this manner up to a certain point, from which the explosion ceases to produce itself.

When this occurs on a second cartridge the same series of effects will be produced from the second to the third cartridge; but they depend on the character of the explosion of the second cartridge. And thus it goes on.

13. Such is the theory that appears to me to explain explosives by influence and the phenomena which accompany them. It depends, definitely, on the production of two orders of waves; one series represents the explosive waves, properly so called, developed in the midst of the matter which detonates, and consisting of a continual reproduction of the transformation of the chemical actions into caloric and mechanical action, which transmits the shock to the support and to the contiguous bodies; the other is a purely mechanical and physical series, and which also transmits the sudden pressures all round the center of the concussion to the adjoining bodies, and by a singular circumstance to a new mass of explosive material.

14. A theory differing from this was originally proposed by Abel; that is to say, the theory of *Synchronous vibrations*, to which we shall now direct our atten-

tion. According to this English savant, the originating cause of the detonation of an explosive lies in the synchronism, between the vibrations produced by the body which provokes the detonation, and those which the first body would produce in detonating, precisely as a violin string resounds at a distance in unison with another vibrating chord.

Prof. Abel has cited the following facts in support of his theory. To begin with, the detonators appear to differ with each variety of explosive. For instance, nitrogen, iodide so susceptible to shock or friction, cannot cause the detonation of compressed gun-cotton. Nitrogen chloride, so easily exploded will not produce the same detonation, except when ten times the weight of the necessary fulminate is used. In the same way nitro-glycerine will not produce a detonation of gun-cotton in sheets on which is placed the envelope in which it is contained. In this way nitro-glycerine up to 23.3 grains can be detonated without success. On the other hand, the inverse influence is proved; 7.75 grains of compressed gun-cotton having caused the detonation at a distance of 25 mm. of nitro-glycerine wrapped up in an envelope of thin sheet iron. A cap filled with a mixture of potassium ferrocyanide and potassium chlorate, similarly, (according to Brown) will not detonate gun-cotton. Finally, a cap consisting of a mixture of mercury fulminate and potassium chlorate, should be used of much heavier weight than if it was filled with the pure fulminate, (according to Franzl). Nevertheless, the heat given off by the same weight is greater by one-fifth than that with the first mixture.

15. Messrs. Chapman and Pellet have brought to the support of this ingenious hypothesis the following experiments: they attached to the strings of a double bass particles of nitrogen iodide, a substance which detonates on the slightest friction. Then they made the strings of a similar instrument vibrate at a short distance off; a detonation was produced, but only for sounds higher than a certain note, which corresponds to 60 vibrations per second. They also took two conjugate parabolic mirrors placed 2.5 meters apart and they arranged along the line of foci at different points several drops of nitro-glycerine or of nitrogen-iodide, these they detonated at one of the foci a large drop of nitro-glycerine; they observed that the explosive substances placed in the conjugated foci detonated in unison to the exclusion of the same substances placed at other points. A layer of lamp-black placed on the surface of the mirrors was designed to prevent the reflection and the concentration of the calorific rays.

16. As yet none of the experiments appear to me to be conclusive, and several of them seem even to be formally opposed to the theory. We shall begin by observing that the speciality of a given musical note, capable of determining each variety of explosion, has never been established; it is only below a certain note that the effects cease to be produced while they take place by preference and whatever the explosive bodies may be by the action of the most acute notes. Besides, these effects cease to produce themselves at distances which are incomparably less than the resources of the chords in unison, which goes to prove that the detonations are functions of the intensity of the mechanical action, rather than of the character itself of the determining vibration. Similarly, the detonation ceases to be produced when the weight of the detonator is too slight, and in consequence when the living force of the shock is weakened. Nevertheless, the special vibratory note which determines the explosions should always remain the same. For instance, cartridges filled with 75 per cent. of dynamite cease to detonate when the capsule contains a weight of fulminate less than 0.2 grains; the detonation not being certain in all cases except by the regulation weight of one grain. This confirms the existence of a direct relation between the character of the detonation and the intensity of the shock produced by one and the same detonator.

If it is true that gun-cotton will cause the nitro-glycerine to detonate in consequence of the synchronism of the vibration communicated, then we do not understand why the reciprocal action does not take place, while the absence of reciprocity can be easily explained by the difference of the structure of the two substances which play so important a part in the transformation of the living force into work.

17. This same diversity of structure and the modifications which it introduces into the transmission of the phenomena of shock and with the transformation of mechanical energy into caloric energy, may be called upon to explain the facts observed by Abel.

The difference between the energy of pure fulminate and of the fulminate mixed with potassium chlorate is no less easily explained; the shock produced by the first body being sharper on account of the absence of all dissociation of the product, which is no other than carbon monoxide, this absence should be opposed to the dissociation of carbon dioxide formed in the second case. Perhaps, also, the formation of potassium chloride disseminated through the gas produced with the aid of potassium chlorate weakens the shock, just the same as silicon does in the case of dynamite.

18. All the effects observed with nitrogen-iodide may be explained by the vibration of the supports and by the effects of rubbing which results therefrom, this substance being particularly sensitive to friction.

19. The experiment with the conjugate mirrors may also be easily explained by the concentration in the foci of the movements of the air, and therefore of the mechanical effects which result.

20. Besides, M. Lambert has proved by experiments made for the commission of explosive substances, that the explosion of dynamite cartridges being produced in tubes of cast iron of large diameter, it does not appear to have, from the standpoint of detonations provoked by influence, any difference between the vibratory motions and nodes characteristic of the tube.

21. Desiring to clear up this entire question by removing it from the influence of the supports and of the diversity of cohesion and physical structure of solid explosive substances, I undertook a series of special experiments on the chemical stability of matter in a sonorous vibration and especially on that of gaseous bodies such as ozone or hydrogen, arsenide or liquids, such as hydrogen feroxide, or persulphuric acid, all of the bodies being selected from among those which decompose or change spontaneously, and that at ordinary temperatures with the disengagement of heat, precisely similar to explosive substances. The description of these experiments may be found in the *Comptes Rendus* or in the *Revue Scientifique*, May, 1880.

They lead to the conclusion, that substances which are transformable with the disengagement of heat, are stable under the influence of sound waves, while they are decomposed under the influence of ethereal vibrations. This diversity in the mode of action of the two classes of vibrations is not surprising when we consider that the sharpest sonorous vibrations are incomparably slower than the luminous or calorific vibrations.

22. Nevertheless, it appears certain that the propagation of explosions by influence is not made in virtue of an undulatory movement, a complex movement of a chemical and physical order in the midst of the explosive substance which is decomposed, whilst it is purely physical in the midst of intermediary substances which suffer no decomposition. But that which distinguishes this sort of movement of the vibrations, properly so called, is, first of all, its extreme intensity, that is to say, the magnitude of the living force which it transmits; it is also the unique character of the explosive wave which is propagated in opposition to the multiplicity of successive sonorous waves. Finally it is essential to observe that the explosive material does not detonate because it transmits the movement, but on the contrary because it arrests, and because it transforms on the spot the mechanical energy into calorific energy, capable of suddenly raising the temperature of the substance up to the degree which will produce its decomposition.

Anemometric Observations.—M. Domojiroff continues to publish in the *Izvestia* of the Russian Geographical Society his anemometric observations on board the clipper "Djighit." In June, 1881, during the cruise from the Zond Strait to the Seychelles Islands, he met mostly with south-east winds, the velocity of which varied from 3 to 7.5 meters per second, with one exception, on June 9th, when it reached 15 meters. On the cruise from the Seychelles to Aden, from June 25th to 30th, the wind was mostly south-west, and varied from 5 to 12.7, reaching 14.3 meters per second on June 29th.

WHO DISCOVERED GUNPOWDER ?

A HISTORICAL CHAT.

By KARL BRAUN, Wiesbaden, Leipzig.—Nord und Süd, Juni, 1883.

Translated by LIEUT. JOHN P. WISSER.

THE question, "Who discovered gunpowder?" is usually answered to-day *unisono:*

"Berthold Schwarz, the Freiburg monk."

So our youth has been taught for two generations, and this is quite enough to make any doubt of this assumed fact appear as idle folly.

Nevertheless doubt is justified. The contemporaneous writers, the authors of the middle and of the latter half of the fourteenth century, knew nothing of the discovery of the monk of Freiburg. The name of Berthold Schwarz is first mentioned long after "Büchsen" and "Katzen" (small cannon or mortars) were used in firing, and after a "Katzenstadl," *i. e.*, a gun-foundry, as well as an arsenal existed in Augsburg, for instance.

But even those who grant Schwarz the honor of being the first to make use of the preparation of gunpowder in Germany, and to spread the knowledge of its use, deny him part of the merit of the discovery. They assert that he too belongs to the great number of those "who did *not* discover gunpowder;" at all events he could not have taken out a patent on his "invention," for it had been in use for centuries. The Chinese had been long acquainted with it; traces of it are found among the Saracens and the Byzantines; it may be assumed, say they, that the discovery is derived from the Chinese, and has passed by various, no longer accurately determinable, steps, to the Byzantines, and through them has arrived in Germany; although the Byzantine or "Greek fire" is not identical with modern gunpowder, it is of earlier date, and the latter bears the same relation to the former that an amendment bears to the principal motion, or an additional or improvement pattern to the main patent.

Occupied with these doubts, I find in the "Chronicles of Augsburg," composed by the learned Clemens Jager, about the middle of the sixteenth century, the notice that *a Jew, named Typsiles, discovered gunpowder in the year 1353, in Augsburg, and from Augsburg the preparation of gunpowder, its application to military purposes, and the manufacture of fire-arms, spread throughout Germany and over the rest of Europe.*

True, the chronicler Clemens Jager, writes two hundred years after the discovery and the propagation of gunpowder manufacture in Europe, and cannot therefore speak from personal observation or the observation of his contemporaries. But the same is true of the warranters and witnesses of the patent of the monk of Freiburg. Clemens Jager is, however, to be regarded as an earnest and authentic writer, who has studied his sources carefully. We are compelled to believe that, to make such an assertion with such apodictic certainty, he must have had his good sources and grounds therefor, and that he could assume belief and agreement in his assertion from his fellow-citizens in Augsburg, who were acquainted with his sources, and instructed by the traditions of their forefathers on the subject. Indeed, his statement not only remained uncontradicted at the time, but was confirmed and repeated by other chroniclers and other authors of later date.

We may therefore assume as authentic that it was believed in Augsburg, in the sixteenth century, that the discovery or re-discovery of gunpowder by the said Typsiles took place within the walls of that good city.

I acknowledge that this view is founded on a legend as well as that which asserts the authorship of Berthold Schwarz. In this respect one has not much preference over the other. We also know little more of Schwarz than of Typsiles in both cases we must be content with the mere names.

But here there is nevertheless a slight difference. "Schwarz" belongs to the names which are so common that they hardly bear the stamp of individuality. Schwarz is a name like Brown or White, like Smith or Jones, like Miller or Baker.

Typsiles, on the contrary, has a meaning. The name is not of Jewish, but of Greek origin, when we consider Typto or Psilos, or regard it as a compound of the two, or of two similar words.

The name points to the Levant, to the Byzantine empire—to Constantinople, which at that time not yet conquered from the Turks, had still an active inter course with the West; we find, for instance, Byzantine comes everywhere, from Hungary and Roumania to Denmark and Sweden, and thence to Portugal and Spain. The old German shrines of relics are of Byzantine origin. So also the old imperial crowns. And the Hungarian king's crown, so celebrated for its age and adventures (it was several times sold, stolen, pawned, conquered, robbed, hidden, and yet always reproduced), and regarded by every good Hungarian as sacred, is of Byzantine origin.

It is a fact that the Byzantines possessed an explosive substance closely related to modern gunpowder, as it came into use in the middle of the fourteenth century in Germany, and middle and Western Europe.

These circumstances lead us to the conjecture that the said Typsiles, be he of Jewish, or Greek Catholic, or Roman Catholic confession—for faith has nothing to do with gunpowder—came from the Orient, and brought thence a knowledge of the preparation of Greek fire into the free imperial city of Augsburg. the metropolis then of the Alemannic countries in Germany, where, by modifications of the technical methods employed, he effected the preparation of our gunpowder.

I do not intend to write an account of the Greek fire, or the science of gunnery in Constantinople, which passed from the Byzantines to the Turks (as did, for instance, the dome of the churches, and much else), but only, *en passnnt*, to insert two interesting notices.

The "Greek fire" played its part on into the nineteenth century.

During the Greek war for independence in the twenties, the Greeks obtained only occasional successes by land, and these did not prove to be lasting. The separate bands of the armatoli, klephts and palikari, brave as they were, soon dispersed again. The truly decisive triumphs of more permanent effect were gained at sea, where a Miavlis and a Sachturis delivered murderous battle to the fleets of Chosren and Ibrahim; and here it was that the activity of the Greeks triumphed over the lethargy of the Turks, the small vessels of the Greeks, so capable of manœuvering, over the colossal, unwieldy and heavy vessels of the Turks; *and principally by means of fire-ships and the Greek fire.*

These small fire-ships, furnished with this combustible, each manned by nine, or at most twelve men, swarmed about the large Turkish ships, surrounded them on all sides and endeavored to deprive them of wind. The Greeks were familiar with the seas and coasts, those of the mainland as well as those of the innumerable islands, which latter had furnished the trained mariners, men of bravery and skill, inured to the perils of war and the sea, whose wants were so few that a handful of black olives sufficed for a day's subsistence. They were versed in the wind and weather of these seas, and could anticipate their character for several days, so as to prepare combined plans of operations in advance. The Turks, on the contrary, generally rode at anchor. "To anchor suits best the believers in Fatalism," (Mouiller convient aux adeptes du Fatalism) says the French Vice-Admiral Jurien de la Gravière, in his highly interesting contribution to the history of the Orient, from 1815 to 1830, which he has furnished in his book "La Station du Levant."

When the Turkish ships, finding themselves surrounded by the Greek fire ships, overcoming their fatalistic lethargy, finally put themselves in motion, it was generally too late to escape them by a precisely executed manœuvre. The fire-ship knew how to attach itself and its fire—its Greek fire—which burned on and exploded even under water, so skillfully that it could not be gotten rid of. The nine or twelve men in the fire-ship pulled rapidly out of danger in a light boat, w e the Greek fire blew the Turkish

vessel into the air, or at least tore open a breach of several meters in extent, and thereby usually succeeded in sinking it.

In our torpedoes and torpedo-boats we observe a new form and application of the Greek fire-ships and Greek fire, which has possibly entirely changed naval warfare. At all events the above-mentioned Jurien de la Gravière thought it possible to predict as much. Meanwhile the Germans have every reason to be grateful for the invention of torpedoes, for in 1870 they successfully protected and defended their coasts and seaports.

So much for the notice in regard to *the history of the Greek fire.*

The second notice relates to *the artillery of the Turks.* In the palmiest days of the Turkish Empire, in the fifteenth and sixteenth centuries, the Turkish army excelled in cavalry and artillery. As early as the fifteenth century a gun-foundry existed in Constantinople. In Turkish it is called *top-hané.* In the ear of the Turk the cannon shot does not sound as in our own, "bang," but "top." *Top* is the gun, and *hané* the house. Hence, "gun-house"; and this is precisely what "Katzenstadl" signifies in Augsburg. In the sixteenth century this gun-foundry, this *top-hané,* lying in the suburb Pera, enjoyed an extraordinary celebrity, and the writers of the day (the Genoese Giovanni Antonio Menavino, for instance) do not fail to add to their notice of this gun-foundry, that they were Greek Jews or Jewish Greeks who conducted the entire establishment, namely, the casting of cannon and the preparation of gunpowder, thus furnishing the elements of war and destruction to the hereditary and arch-fiend of Christendom, as the Turks were called, although then and thereafter in alliance, or at least in most cordial harmony, with the "most Christian King" of France. Here certain remarks are pertinent, which I hesitate to communicate. They may be oil on the flames of our anti-semitics of to-day. At all events these remarks cast a peculiar, even somewhat comic, retrospective light on the fact that it was also, as Clemens Jäger informs us, a Greek "Jew named Typsiles," who furnished the Christians of the West their elements of destruction and war to be used against the Mohammedans of the East.

The subject is therefore compensated. Let us return, after the communication of these Greco-Turkish notices, from the East to the West, to Germany, to Augsburg. That this metropolis of the Alemanni, like Nürmberg, the metropolis of the Franks, stood then, in the fourteenth century, on the pinnacle of arts and manufactures in Germany, is an indisputable fact. Nürmberg was celebrated for the discovery of painting on glass, Augsburg for that of linen or rag paper (in contradistinction to the old parchment or the East Indian cotton paper). Even this claim is contested by other German cities—Ravensburg, for instance, which can produce a register of the year 1324 written on rag paper, and a linen paper mill of the year 1412. But the claims of Augsburg rest on older documents, namely—city accounts of the year 1320, undoubtedly genuine, and written on linen paper. It also possesses such a document of 1330, and many from 1360 on. In short, there is no doubt that Germany can produce the oldest documents on linen paper—older than those produced by Spain and Italy—and that, among the competing German cities it is Augsburg again which contains the oldest of these possessions. The importance of this discovery is apparent when it is remembered that the art of printing would not have spread so rapidly so soon after its discovery, had not linen paper, which surpasses parchment paper in cheapness and cotton paper in durability, already existed.

This same Augsburg, which rejoices in the oldest linen paper, rejoices also in the oldest cannon, *i. e.,* machines from which, *by means of gunpowder,* balls were fired at the enemy. These were then called "Katzen," or "Büchsen" (generally written "Puchsen," "Buchsen," or "Pugxen"). At first they were of wood with iron hoops, and threw stone balls. Augsburg made use of such machines as early as 1372, in the war against the Bavarian dukes. This is attested by the historian Adelzreiter, and confirmed by the city accounts, in which it may also be seen that the gunpowder manufactured by the city was made from saltpetre. In the city accounts of 1377 "gropze Büchsen," *large* guns, which the city ordered to be *cast,* are already mentioned, hence *metallic* cannon. There also existed a "Büchsen-meister," or a master-gunner, appointed by the city.

I select the following extracts from a valuable little paper on "Augsburg and its Former Industries," long ago out of print and forgotten, written and published, under commission from the city, by the industrious city recorder, Theodore Herberger, in 1852, on the occasion of the exposition of arts and manufactures of the Bavarian district of Suabia and Neuberg, which is based upon an accurate study of the archives there, namely, the city accounts.

In 1371 the city had expenditures "for saltpetre for the guns," for saltpetre for the manufacture of powder for the guns. In the account 20 guns are mentioned as being used in firing; moreover, "Trinkgelder" (pour boire) for the vassals who served these guns. The expenditures for the wooden frames on which the guns were supported are reckoned in the account. A year later, 1372, 400 shot were cast for the guns; lead "for casting" occurs in the account, saltpetre and "wilder schwefel" (wild sulphur) for gunpowder. One year later, 1373, the expenditure for copper, lead, "and other material" is reckoned in the account "for 4 guns. Another year later there occurs in the accounts an item "for a mortar, in which powder for cannon is pulverized." Many such and similar items may be cited to show how far advanced Augsburg was when the art of firing with heavy guns began. Master Walther, the master-gunner, was not only paid, in 1373, the uncommonly large sum of 160fl., but also received a special present in cloth for constructing the guns ordered, and inspecting the preparation of gunpowder in the court of a "canon of St. Moriz." An unusual number of large cannon was manufactured, according to the accounts, in the years 1410 and 1414, and in the year 1416 the master-gunner, Ott, who was also employed to cast bells in foreign cities, cast several large pieces. All this proves the early date of an immense trade in this department. An especially remarkable man appears in Augsburg in the year 1436. Master Heinrich Roggenburger, the master-gunner. His office is more particularly "the casting of guns, large and small," and the firing of them "as dexterously as has ever been seen;" he can also prepare the powder therefor. Besides, he is a man remarkably well versed in the technicalities of his art in other respects also, and in his letter of admission he is recommended for the following qualities: He can "make cast and projectile apparatus, large and small, the like of which was never seen in German lands, for this apparatus stands still after the throw, without moving or altering its position, and not requiring to be bound or held;" these machines throw masses of five or six hundred-weight; besides, he makes lifting-machines, by means of which a hundred hundred-weight may be lifted from or upon a wagon; also shields for guns and war chariots, and bridges which may be carried over land and laid over ditches or running water. Moreover, he understands the building of houses and towers, water-mills, windmills and horse-mills, and can make cast, earthen and wooden water conduits to supply the water of wells to hill and valley. Roggenburger received a yearly salary of 110 fl. In the year 1502 the town had a foundry of its own built, which was called Katzenstadl. Here, according to the account of his contemporary, Clemens Sender, Niklas Oberacker cast one hundred metallic pieces and a mortar; among the larger pieces were several forty feet in length. The most noted of all the gun-founders of Augsburg was Gregor Löffler. He was much occupied, not only in Augsburg, but also in foreign countries. In the year 1529 the Government called him to Innsbruck. In this year and in 1537 he had orders to recast all the old pieces which the Emperor and King Ferdinand had in the Tyrol. Among the newly cast cannon were "Karthaunen," capable of firing a shot of an hundred-weight. This work gained for him such approbation that he was entrusted with casting the statues designed to decorate the tomb of the Emperor Maximilian.

Thus far the recorder Herberger. The statues in Augsburg cast by Gregor Löffel are, nevertheless, not identical with those colossal life-statues which now surround the tomb of the "last of the knights" in the Franciscan church at Innsbruck, the authors of which were the brothers Stephen and Melchior Godl. The statues of Löffler, representing various saints, twenty three in number, are found in the same church, in the so-called "Silver Chapel," on the south wall.

I will not further expand this chapter

on Augsburg gunpowder and Augsburg ordnance; whoever desires to pursue the subject further, him I refer to the Augsburg chronicles of the fourteenth and fifteenth centuries, published by Professor Karl Mayer.

I hasten to conclude. I am aware that this unassuming chat does not solve the problem, but only brings us a trifle nearer the solution. I only desired to instigate doubt and investigation.

THE RATIONAL FORMULA FOR PILLARS APPLIED TO TESTS MADE WITH THE UNITED STATES GOVERNMENT MACHINE AT WATERTOWN, MASS.

By JOHN D. CREHORE.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

In an article entitled "*A new rational formula for pillars*," which appeared in this Magazine for December, 1879, I gave this formula:

$$Q=\frac{P}{S}=\frac{C}{1+\frac{Cl^2}{m\,\pi^2\,Er^2}} \quad . \quad . \quad . \quad (1)$$

where m=1, 4, or 2.28 according as we consider neither, both, or one only of the pillar's ends fixed.

l=length of pillar.

r=least radius of gyration of cross-section, both in inches.

S=area of cross-section in square inches.

E=modulus of elasticity in pounds per square inch.

C=crushing strength of standard specimen in pounds per square inch.

P=breaking weight applied at the end of the pillar and in the line of its axis before deflection, in pounds.

Q=breaking weight in pounds per square inch of cross-section.

Certain conditions upon which the formula depends were pointed out and insisted upon for its proper application to finished pillars. These conditions are:

1st. The pillar must deflect or bend before it breaks.

2d. When m=1, the points of bearing at both ends of the pillar must be in the axis, and the curvature must be single.

3d. When m=4, the line of the axis of the straight, undeflected pillar must be tangent to the curved axis under load, at both ends, and there must be two points of contrary flexure.

4th. For values of m lying between 1 and 4, there will be a blending of parts of the 2d and 3d conditions.

5th. When $m<1$, it is inferred that the line of pressure is not coincident with the axis; and,

6th. When $m>4$, it is to be presumed that the deflection is, for some reason, less than the normal deflection.

It is not the object of the present paper to advocate the adoption of this formula; but, rather, to determine, as far as may be done from the experiments cited, what values are to be assigned to m in a formula based on theoretical considerations, in order that it may apply with reasonable approximation to the compressed members of actual structures, seldom, if ever, fulfilling the conditions imposed upon the formula. Of course the formula, called rational, will become empirical by such determination of the value of m; but this is of no importance

if thereby it shall become also more trustworthy and useful.

Anyone who has ever undertaken the task of adapting a formula to the results of a large number of tests, knows very well how much it is like finding the "laws of disorder." Illustrating this point, Mr. Benjamin Baker has recently said, "taking the mean results of a large number of experiments, the influence of length between the practical limits of 15 to 20 diameters would just be appreciable; but if only a few experiments were compared, the deduction might be drawn that lengthening a column or rounding its ends, increased rather than reduced its strength. Thus, one steel tube 20 diameters in length, tested by the author, bore 22 tons per square inch, whilst a similar tube of half the length bore only 19.2 tons. Again, a round-ended column, 20 diameters in length, bore 19.3 tons per square inch, whilst a flat ended one failed with 18 tons."

Similar anomalies will be found in the tables given below; and no definite and certain explanation of them will be attempted here, for the reason that there is not sufficient knowledge of the material, and the manufacture, and the absolute conditions of the columns, to warrant such an attempt; although, in a few cases, the data at hand seem to throw light on the eccentric behavior reported.

The complete and final determination of the value of m, cannot be made from the few experiments at hand, and with calculated values of C; but, since many such columns are now in existing bridges, and probably many more like them will be placed in future bridges, with no better determination of C, a value for m, though it be only provisional, seems desirable.

The latticed columns of the Detroit Bridge and Iron Company were built of two wrought-iron rolled channels with their flanges outward, and lattice bars riveted to each flange at intervals of 18 inches for the six-inch and eight-inch channels, and at intervals of 22 inches for the ten-inch and twelve-inch channels. These lattice bars had a cross-section ranging from 2 by $\frac{1}{4}$ to 2 by $\frac{3}{8}$ inches, and were placed so as to "break joints" on the opposite sides of the column; thus leaving only 9 or 11 inches of wholly unsupported channel length.

The pin holes were $3\frac{1}{2}$ inches in diameter when the channels were spaced 8 inches apart, and were 3 inches in diameter for channels spaced 6 inches. From Colonel Laidley's Report for 1881, I quote:

"All columns with pin bearings tested with pins in a vertical position.

"Columns over 12 feet in length, supported at the middle by a counter-weight of half the weight of the column.

"Strains gradually applied; the time of the test occupying $1\frac{1}{2}$ hours, unless otherwise stated in the detailed notes of the test.

"Sectional areas of channel bars, computed from the weights of the bars before the rivet holes and pin holes were made; calling the sectional area in square inches one-tenth the weight of the bar in pounds per yard.

"Horizontal and vertical deflections measured at the middle of the columns.

"The compressions measured within the gauged length, a distance laid off along the middle of the upper channel bar, always taking a gauged length less than the distance between the eye plates, to avoid any disturbance of the channel-bar webs the eye plates might occasion.

"Many columns are found to have the pin holes bored out of parallel and not at right angles to the axis of the column. In such cases thin brass packing was placed between the bolsters carrying the pins of the columns and the faces of the compression platforms of the testing machine.

"When such packing was used, the amount in thickness is recorded, showing what was necessary to secure a good bearing for the ends.

"The eye plates, riveted to the webs of the channel bars, in some cases slipped, allowing the pin hole in the web to elongate without disturbing the holes in the riveted plate. The slipping, probably, took place when the friction of the rivet heads was overcome."

From equation (1) we derive

$$C = \frac{Q}{1 - \frac{Ql^2}{m\pi^2 E r^2}}, \quad . \quad . \quad . \quad (2).$$

and $$m=\frac{CQl^2}{(C-Q)\pi^2Er^2} \quad . \quad . \quad . \quad (3).$$

Take $$m_0=\frac{300\,m}{l}=\frac{300\,CQl}{(C-Q)\pi^2Er^2} \quad . \quad . \quad (4).$$

Modulus of elasticity,

$$E=\frac{P_1 l_1}{S\,\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

where P_1 = the pressure which shortens the pillar by the length, λ in pounds.

l_1 = gauged length in inches.

In the accompanying tables all values of l, l_1, P, P_1, Q, λ and S, are taken from the published reports. Q, in Table I, is a mean of two of its values in Table II, for the given length of column.

Table I is given to show the probable effect of thickness of flange where it meets the web, on the strength of the channel used as a strut. It there appears, from the few examples cited, that the strength of a channel strut is roughly proportional to said thickness, allowance being made for length and radius of gyration. Should this law hold generally, it is worthy the attention of manufacturers.

In computing the radius of gyration, r, for columns formed of two latticed channels, the section of each flange has been treated as a trapezoid instead of a rectangle; thus making r, in Table III, a little smaller and nearer correct than by the ordinary method.

In Table II, E is derived from the compression of columns having two channels of the given section, in Table III, and computed by equation (5).

C is obtained from equation (2), assuming $m=4$.

The greatest value of C, in Table II, found for channel bars of each given size, is used in computing m, by formula (3), for Table III.

The deflections in Table III, are those due to the load next before the ultimate.

In Table IV, C = the mean of the two greatest values of Q, for each year 1879 and 1881.

From equations (3) and (4) are found the values of m and m_0.

The factor $\frac{300}{l}$, (l being in inches) is introduced into m, to show that the resulting m_0 is more uniform than m, and is more nearly within the normal range of m from 1 to 4, in case of these examples of Phoenix and Kellogg columns.

By comparing the cases giving the anomalous values of m, that is, values greater than 4 and less than 1, with normal cases of the same ratio ($l \div r$), we see that the values of Q do not keep pace with those of $l \div r$. For instance, in Table III, observe Nos. 1229 and 1230; Nos. 117, 118, 119, 120; Nos. 24 and 25; Nos. 4, 5, 6, 26; Nos. 1109, 17, 15, 16; and Nos. 468, 469, 470. In Table IV, Nos. 12 to 18; and in Table V, Nos. 3 to 7.

Now, since the ends of a pillar cannot be more than fully "fixed," and fixed ends require $m=4$, it seems to follow that the greater values of m, belong to the cases where the pillar is so well conditioned that its deflection is small up to the breaking point. And, where $m<1$, there is manifestly some irregularity in the construction of the pillar; as, for instance, No. 469 of Table III, of which test the record says: "About .035 inch packing at top of south bolster required to make good bearing. Column not straight; lower channel concave .26 inch; upper channel convex .22 inch; measuring at middle and from outside; straight in other direction." And No. 326 of Table IV, where m is small for a flat-end column, has this record: "There was a space of .22 inch between the upper side of one end and the compression platform, and the same space at the under side of the other end, while the column was under a strain of 10,000 pounds. It required about 80 per cent. of the ultimate crippling load to bring the ends to bearing all around the web and flanges."

Another source of irregularity of m for pin-end columns, is the fact that the deflection is seldom wholly normal to the plane of the pins; and hence the least radius of gyration is not the one to be used in these cases which have an inherent bias to deflect out of the plane of the least circle of gyration. This bias may be partly due to a difference in the size of the two channels composing the column. To illustrate this, we observe that of the 45 cases in Table III, where the two channels were of unequal sections, 27 deflected in the direction of the larger channel, and 18 in that of the smaller; showing that in $\frac{3}{5}$ of the cases the smaller channel yielded first to the pressure.

It might appear that no part of a channel *in a column*, sustains so great a load as the same unsupported length sustains when not built in. This is merely apparent; for, when in the column, the mean strength of the two channels is observed, while in fact one of them is generally strained far beyond this mean.

Evidently more tests are needed, using many columns apparently identical, in order that the most probable values of the constants may be found.

TABLE I.

Channel Bars.

Nominal size, depth, and length. Inches.	Actual depth. Inches.	Width of flange. Inches.	Thickness of flange. At web. Inches. *t*.	Thickness of flange. At edge. Inches.	Nominal thickness of web. Inches.	Nominal area. Square inches.	Ratio of length to radius of gyration. $l \div r$.	*tr* — .	Mean value of Q, lbs.
6	6.07	1.71	.43	.24	.22	2.33	12.504	.0358	42,493
8	8.08	2.02	.62	.25	.29	3.85	15.028	.0413	43,295
10	10.00	2.48	.39	.30	.33	4.78	15.251	.0256	35,080
12	12.06	3.03	.40	.38	.32	5.97	13.944	0287	37,240

TABLE II.

Compression Tests of Channel Bars. Flat Ends.

No. of test.	Nominal size of bar. Inches.	Length *l*. Inches.	Sectional area S. Square inches.	Ultimate strength, Q lbs. per sq. inch.	Manner of failure. Buckled.	Manner of failure. Deflected.	$l \div r$.	$\frac{E}{1000}$	$m = 4$. C. lbs. per sq. inch.
1049	6	6	2.33	42,290	1		12.504	28,053	42,548
1050	"	6	2.33	42,695	1		12.504	28,053	42,958
1071	"	17.58	2.37	36,670		1	36.872	28,288	38,383
1072	"	17.70	2.23	37,000		1	36.298	31,762	38,763
1069	"	23.83	2.23	35,160		1	48.869	31,762	38,148
1070	"	23.90	2.37	32,660		1	50.128	26,144	35,481
1064	"	48	2.38	28,140		1	100.835	28,288	37,574
1051	8	8	3.85	42,780	1		15.028	26,801	43,146
1052	"	8	3.85	43,810	1		15.028	26,801	44,194
1068	"	17.90	3.73	35,280	1		33.199	25,501	36,821
1065	"	23.85	3.73	36,540		1	44.234	25,501	39,201
1066	"	23.85	3.73	35,410		1	44.234	26,680	37,903
1067	"	29.90	3.73	33,400		1	55.455	26,680	37,000
1063	"	48	3.73	30,620		1	89.025	26,680	39,787
1053	10	10	4.78	34,810	1		15.251	28,126	35,066
1054	"	10	4.78	35,350	1		15 251	28,126	35,614
1074	"	17.85	4.76	33,820	1		27.173	28,126	34,598
1075	"	23.90	5.04	35,080	1		37.326	25,286	36,888
1076	"	23.87	5.04	33,630	1		37.279	25,286	35,282
1073	"	29.90	4.76	34,050		1	45.517	28,126	36,360
1062	"	48	4.76	34,080		1	73.070	28,126	40,759
1055	12	12	5.97	37,240	1		13.944	27,872	37,487
1056	"	12	5.97	37,240	1		13.944	27 872	37,487
1079	"	17.84	5.95	36,590	1		20.691	27,872	37,119
1077	"	23.92	6.02	36 350	1		27 891	29,630	37,250
1078	"	23.87	6.02	37,040	1		27.897	29,630	37,971
1080	"	29.90	5.96	35,150	1		34.711	27,872	36,557
1061	"	48	6.19	36,040	1		56.630	27,176	40,219

TABLE III.

Latticed Columns built by the Detroit Bridge and Iron Company. Nos. 1059, 1060 have flat ends; Nos. 1095, 1096 have each one pin end; all others have two pin ends, tested with pins vertical.

No. of test.	Size of Channels. Inches.	Length of Post. l inches.	Cross section. S sq. inches.	Gauged length. l_1 inches.	P_1 = load giving λ. $\frac{P_1}{1000}$ lbs.	Compression due P_1. λ inches.	Penultimate deflection. Hor. D ins.	Penultimate deflection. Ver. D ins.	$\frac{E}{1000}$ lbs.	$l \div r$.	Q lbs.	C lbs.	m.
					Channels 8 Inches Apart.								
1059	6	120	4.76	80	135	.0786	.02	.10	28,867	52.623	36,720	42,958	2.457
1060	"	"	4.67	"	120	.0745	.07	.05	27,593	52.292	35,330	"	1.998
1095	"	"	4.75	"	130	.0774	0.	.02	28,288	52.555	33,680	"	1.543
1096	"	"	4.58	"	"	.0890	−.03	−.04	25,514	51.903	33,800	"	1.696
1107	"	144	4.60	100	"	.1100	−.01	0.	25,692	62.478	34,740	"	2.796
1108	"	"	4.57	"	"	.1123	.10	.06	25,331	62.386	34.160	"	2.597
1	"	150	4.56	120	120	.1163	−.07	.08	27,153	64.938	35.880	"	3.427
2	"	"	4.74	"	"	.1162	.20	0.	26,144	65.611	32,380	"	2.194
1231	"	180	4 48	150	"	.1265	−.25	.10	31,762	77.553	33,820	"	3.122
1232	"	"	4.56	"	"	.1396	−.08	−.02	28,276	77.925	34,540	"	3.835
1229	"	210	4.66	180	125	.1712	.05	.06	28,203	91.455	32 750	"	4.141
1230	"	"	4.74	"	"	.1637	−.21	.02	28,997	91.855	31,120	"	3.337
1117	"	240	4.66	200	120	.1955	−.41	−.02	26,344	104.520	29,180	"	3.822
1118	"	"	4.63	"	"	.1850	.18	−.12	28,019	104.340	30,990	"	4.379
1119	"	270	4.57	230	"	.2155	.24	.10	28,025	116.975	30,590	"	5.256
1120	"	"	4.66	"	"	.2206	−.06	−.01	26,848	117.585	31,050	"	5.845
1121	"	300	4.71	260	110	.2090	−.37	0.	29,054	131 022	23.350	"	3.063
1122	"	"	4.63	"	"	.2250	.32	−.09	27.454	130.420	25,360	"	3 886
20	"	330	4.69	280	80	.1571	.28	.04	30,401	143.970	21,850	"	3.070
21	"	"	4.67	"	"	.1616	−.40	0.	29,681	143.700	20,810	"	2.849
18	"	360	4.70	320	65	.1386	.50	.06	31,930	157.510	14,740	"	1.758
19	"	"	4.73	"	"	.1370	−.57	.10	32,098	157.090	15,900	"	1.966
1111	8	160	7.52	120	220	.1320	−.02	.04	26,596	53.229	34,810	44,194	1.769
1112	"	"	7.50	"	"	.1265	0.	.06	27 826	53 192	35,240	"	1.792
1113	"	200	7.48	150	"	.1730	.10	.04	25,502	66.443	33,970	"	2.724
1114	"	"	7.48	"	"	.1830	−.18	−.05	24,108	66.443	33,610	"	2.461
1115	"	240	7.55	200	"	.2461	−.14	−.23	23,681	79.995	32,610	"	3 406
1116	"	"	7.51	"	"	.2196	.25	.33	26,680	79.816	32,140	"	2.851
1123	"	280	7.99	240	"	.2320	.05	.10	28,484	94.604	32,230	"	3.790
1124	"	"	7.67	"	200	.2335	.10	.07	26,801	93.623	31,370	"	3.582
24	"	320	7.78	280	150	.1800	.10	.22	29,991	107.280	31,350	"	4.202
25	"	"	7.75	"	"	.1978	.04	−.07	28,857	107 290	27,850	"	3.043
22	"	360	7.81	320	100	.1376	.60	−.03	29,777	120.930	24,850	"	2.825
23	"	"	7.80	"	"	.1415	.03	−.02	28,993	120.900	26,920	"	3.518
13	10	150	9 68	100	250	.0971	0.	−.05	26,598	41.748	35 550	40,759	1.847
14	"	"	9.59	"	"	.0984	.03	.05	26 493	41.758	35,350	"	1.768
11	"	200	9.55	150	"	.1340	.20	.12	28,126	55.009	33,840	"	2.212
12	"	"	9.61	"	"	.1419	−.10	−.10	27,499	55.012	34,000	"	2.432
3	"	250	9.74	200	"	.1879	.02	−.20	27,320	69.675	33,880	"	3.614
4	"	"	9.81	"	"	.1752	.10	.10	29,036	69.778	33,660	"	3.283
5	"	300	10.04	250	"	.2175	−.17	−.30	28 621	84.163	34,130	"	5.262
6	"	"	10.00	"	"	.2466	.16	−.35	25,286	84 109	31,930	"	4.177
26	"	350	9.30	300	150	.1615	.22	0.	30,659	96.748	32,180	"	4.708
27	"	"	9.57	"	"	.1574	.57	−.19	29.875	97.166	29,380	"	3.370
1109	12	120	12.15	70	370	.0833	0.	.06	25,590	27.024	33,420	40,219	0.572
17	"	"	12.06	"	300	.0605	0.	0.	28,126	26.978	35,070	"	0.718
15	"	180	12.12	130	"	.1086	0.	.03	29,630	40.513	33,830	"	1.195
16	"	"	12.47	"	"	.1110	0.	.05	28,176	40.780	35,490	"	1.805
9	"	240	12.98	200	290	.1566	−.01	.04	30,916	53.872	34,360	"	2.272
10	"	"	12.34	"	320	.1951	0.	−.20	25,583	54.242	33,610	"	2.294
7	"	300	12.14	250	290	.2168	.19	−.05	27,537	67 343	32,940	"	3.037
8	"	"	11.91	"	"	.2184	.05	−.05	27,872	67.250	34,240	"	3.786
28	"	360	12.18	300	300	.2220	−.10	−.45	28,846	81.120	31,610	"	3.413
29	"	"	12.54	"	"	.2060	.30	.10	30,195	81.662	31,340	"	3.177

TABLE III.—(*Continued.*)

No. of test.	Size of Channels. Inches.	Length of Post. l inches.	Cross section. S sq. inches.	Gauged length. l_1 inches.	P_1 = load giving λ. $\frac{P_1}{1000}$ lbs.	Compression due P_1, λ inches.	Penultimate deflection. Hor. D ins.	Penultimate deflection. Ver. D ins.	$\frac{E}{1000}$ lbs.	$l \div r$.	Q lbs.	C lbs.	m.
						CHANNELS 6 INCHES APART.							
463	6	240	4.68	190.50	22	.149	.07	0.	28,128	102.240	25,000	42,958	2.252
464	"	300	4.68	239.50	12	.098	.03	.03	30,568	127.800	15,260	"	1.282
465	8	240	7.75	190.50	24	.173	.09	−.06	27,673	80.000	27,750	44,194	1.748
466	"	300	7.75	242.00	24	.189	.07	.12	30,732	100.000	26,000	"	2 082
467	10	240	9.19	182.00	26	.150	−.03	.22	31,547	65.785	29,980	40,759	1.576
468	"	300	9.19	235.00	26	.200	.12	.02	30,550	81.570	32,000	"	3.271
469	12	240	12.95	181.50	28	.162	0.	.46	31,370	54.114	28,970	40,219	.980
470	"	300	12.95	234.75	24	.203	−.23	−.05	27,754	66.944	30,000	"	1.901

Mean value of m for flat ends, Table III 2.227
" " " one pin end, Table III.. 1.620
" " " two pin ends, " 2.885

TABLE IV.

PHOENIX COLUMNS. FOUR SEGMENTS CIRCULAR. FLAT ENDS.

No. of test.	Length of column l. inches.	Cross section S. sq. ins.	Gauged length l_1 inches.	P_1=the load giving λ. $\frac{P_1}{1000}$ lbs.	Compression due P_1 λ. inches.	$\frac{E}{1000}$ lbs.	$l \div r$.	Q. lbs.	C. lbs.	m.	$m_0 = \frac{300m}{l}$
1879.			$l=l_1$								
1	336	12.062		360	.368	27,250	115.860	35,150	57,215	4.57	4.08
2	"	12.180		402	.424	26,155	"	34,150	"	4.43	3.95
3	300	12.233		396	.356	27,280	103.690	35,270	"	3.67	3.67
4	"	12.100		396	.377	26,043	"	35,040	"	3.78	3.78
5	264	12.371		300	.240	26,455	91.243	35,570	"	3 00	3.41
6	"	12.311		300	.236	27,260	"	34,360	"	2.66	3.02
7	228	12.023		300	.198	28,732	78.801	35,365	"	2.03	2.67
8	"	12.087		300	.213	26,568	"	36,900	"	2.46	3.24
9	192	12.000		200	.120	27,842	66.317	36,580	"	1.62	2.54
10	"	12.000		200	.116	27,569	"	36,580	"	1 64	2 56
11	156	12.185		300	.142	27,048	53.916	36,857	"	1.13	2.17
12	"	12.069		200	.091	28,474	"	37,200	"	1.13	2.16
13	120	12.248		327	.120	26,660	41.474	36.480	"	.66	1.65
14	"	12.339		340	.126	26,303	"	36,396	"	.66	1.66
15	84	12.265		200	.054	25,365	29.032	38,157	"	.39	1.38
16	"	11 962		240	.067	25,155	"	43,300	"	.60	2 16
17	48	12.081		320	.051	24,930	16,590	49,500	"	.41	2.57
18	"	12.119		320	.046	27,553	"	51,240	"	.50	3.10
19	8	11.903		400	.020		2.765	57,130	"		
20	"	11.903		400	.016		"	57,300	"		
Mean	.	..	..	..	..	..	..	..	..	1.96	2.82
1881.											
325	30.000	11.610					10.368	56,070	54,435		
326	142.625	12.181					49 294	38,256	"	1.03	2.17
327	29.940	11.902	24.016	350	.0230	30,706	10.348	52,800	"		
31	372.000	11.430	300	250	.2146	30,576	128.570	31,150	"	4.00	3.21
32	372.000	11.310	300	250	.2196	30,197	128.570	32,700	"	4.54	3.66
33	378.000	11.660	300	250	.2175	29,573	130.643	31,180	"	4.26	3 38
34	378.000	11.580	300	250	.2170	29,846	130.643	32,220	"	4.57	3.63
Mean	..	..	..	..	..	..	..	..	..	3.68	3.21

Mean of all, in Table IV.. 2.33 2.90

TABLE V.

KELLOGG COLUMNS.—Two 10-inch channels, latticed with flanges turned inward. Webs about 11 inches apart, except No. 8, which has flanges turned outward, and webs 4.5 inches apart. Pin hole of No. 9 was 3.915 inches in diameter; other pin holes 3 inches in diameter. No. 8 reached the limit of the machine, viz., 800,000 lbs., or about 40,000 lbs. per square inch of section. C is here put equal to 40,000.

No. of test.	Condition of ends.	Length of column l. inches.	Cross-section S. sq. ins.	P_1=the load giving λ. $\frac{P_1}{1000}$ lbs.	Compression due P_1 λ. inches.	$\frac{E}{1000}$	$l \div r$.	Q.	m.	$m_0 = \frac{300\,m}{l}$
1879.										
1	1 Pin....	285.3	19.244	450	.218	30,603	78.916	25,980	1.53	1.61
2	Flat.	333	19.447	400	.233	29,396	92.110	28,280	2.82	2.54
3	"	237	19.486	450	.165	33,171	65.554	30,740	1.74	2.21
4	1 Pin....	189	19.301	200	.057	34,359	52.267	25,130	.55	.86
5	"	141	19.561	200	.041	35,161	39.002	25,460	.31	.65
6	"	96	19.103	200	.043	23,375	26.554	30,080	.37	1.16
7	Flat.... .	48	19.362	200	.020	24,791	13.277	37,180	.38	2.37
8	"	10	20.322				2.766			
9	1 Pin....	264.5	19.244	200	.078	35,342	73.162	27,900	1.42	1.61
Mean.	..	..	..	..			..	..	1.14	1.63
1881.										
492	1 Pin....	260	17.650	459	.195	29,866	71.918	33,920	3.26	3.77

Mean of all, in Table V	1.37	1.86
" for flat ends, "	1.65	2.37
" " 1 pin end, "	1.24	1.61

RESISTANCE OF RAILWAY CURVES.

From "The Engineer."

IN following exactly the dictates of experience one treads a tolerably safe path; but it is hardly from engineers that we should expect so servile a course, though the expression "experience is a better teacher than theory" finds much favor with some engineers who ought to be able to lay claim to a more independent and progressive policy. It was used on Tuesday evening at the Institution of Civil Engineers as a sort of Promethean warning that one inclined to make a theoretical investigation should beware of any conclusions which would not be in perfect harmony with the received views of the practical man. Engineers as much as those of any profession, have had to acknowledge their ignorance on subjects which their practical experience had led them to prophecy upon against the projects of some little-known man of advanced ideas or theoretical knowledge. They should learn now that even many years of experience will not always guide them in pronouncing a hasty opinion on carefully thought out investigations by younger men of more advanced times. They should remember that it is not those who have steadily observed the beaten track that have commanded things to progess, though they have themselves been on the whole more substantially successful than the innovator. Even railway men of long experience cannot always safely predict that departure from the dictates of that experience must be disadvantageous.

A paper was read last week before the Institution on "Resistance on Railway Curves as an Element of Danger," and was discussed last Tuesday evening. The subject is an old one, but the author had

something new to say upon it; he approached it from a new point of view and in a most temperate manner. Usually it has been considered that the intensity of the pressure brought to bear against the flanges of railway wheels, and especially of leading wheels, in passing round curves, has depended, as one chief element, on the speed at which the curve is traversed; and a maximum velocity has been assumed in calculating the greatest derailing force to be counteracted by flanges. In considering the best form of flanges the same idea has been followed as in the article by Herr Wöhler, referred to in our impression for the 2d March, 1883; but the author of the paper above referred to, Mr. J. Mackenzie, showed that derailment is as likely at slow speeds as at higher velocities, and that the danger of derailment does not necessarily bear any relation to the curve radius. He has thus to appeal to some other cause than centrifugal tendency to account for the pressure against the flanges of the wheels on the outer part of the curve, and for the reason of the subsequent derailment. The active cause he finds in the adhesion of the wheels on the rails, which has to be overcome by the flanges, in correcting the tendency of pairs of wheels of the same diameter rigidly fixed to the same axle, to pursue a path tangential to the curve instead of following it. He says, "In the case of the wheel most likely to mount the rail, namely the outer leading wheel, this side pressure, at slow speeds, is principally caused by the resistance which the treads of the wheels oppose to the sliding motion which takes place in running round a curve." Neglecting the effect of the conical form given to the treads of wheels, it will be seen, as is well known, that of a pair of wheels running round a curve, either the inner one must slip backwards or the outer one forwards, and this slipping is done against the adhesive resistance of the wheels on the rails. Mr. Mackenzie puts it thus: "In order to cause these sliding motions, the outer leading wheel flange exerts against the rail a pressure sufficient to overcome the adhesion or friction of the treads of the wheels; this pressure being exerted directly on the leading wheels, and transmitted to the other wheels through the medium of the engine framing acting as a lever." This, it will be seen, is independent of the speed at which the engine is traversing the curve, except that the friction may be less at high speeds; and from it it follows that when the adhesion between the flange and the rail is greater than the weight upon the wheel the flange will rise and mount the rail. This would take place if the surfaces of contact were vertical, and probably takes place much more easily with the inclined flanges which are always used. Generally, it may be said that a wheel, with flanges approximately vertical, would be caused to mount the rail by a side pressure bearing the same proportion to the load on the wheel which the load bears to the adhesion, while the pressure required diminishes rapidly with the increase of the angle of the flange inclination. Any one who has traveled on a footplate, or watched an engine coming round a curve, will have observed the effect of this side pressure on the outer leading wheel flange, and the periodic forced slipping of the wheels resulting in the engine passing round the curve by a series of jerks transverse to the rail, indicating that the pressure against the flange of the outer leading wheel periodically increases until it is enough to cause that wheel to push the whole engine transversely. As pointed out by the author, "the point of contact between the flange and the rail being in advance of the center of the axle, the motion of the flange at that point is downwards, imparting a downward pressure to the rail, and an upward pressure to the wheel, so that when the flange adheres to the rail the wheel rises. Thus the pressure which would cause the flange to mount the rail is not that which, with the wheel at rest, would force it over the rail in opposition to friction as well as to gravitation, but the very much smaller pressure which, when the wheel is at rest and the tread raised slightly above the rail, would cause friction sufficient to prevent its falling into its place again." From what has been said it will be seen that the action described takes place on a curve of large radius just as it does on a small curve; but with equal slackness of gauge, a greater distance will be traversed in the large curve before an equal pressure is brought to bear on the outer leading wheel flange.

On the whole, it seems difficult to up-

set the author's argument, though in applying his reasoning in a calculation relating to any given six-wheeled engine on a curve of given radius, and taking a normal coefficient of adhesion, it would be found that engines ought more frequently to leave the rails; but in applying the theory the whole of the modifying conditions must be taken into account, including the tractive force exerted, superelevation of outer rail, centrifugal tendency, effect of coning, weight on each wheel, wheel base, and gauge of rails, whether tight or slack: and it is noticeable that as applied to a case recently reported upon by the officers of the Board of Trade, the theory seems to apply very satisfactorily. It is useless to say that derailment is very uncommon, for in the annual Board of Trade report just issued one line alone records fourteen cases of derailment of passenger engines or vehicles, and another ten, both being lines on which flanges with considerable inclination are used. Altogether, a large number of cases of engines and vehicles leaving the rails is reported, and the comparative frequency with which engines do get off the road inevitably suggests that narrow escapes from derailment with some kinds of engines must be uncomfortably numerous. Experience leads us to feel that fixed wheels on parallel axles answer, on the whole, very well, but it must be admitted that were it not for the effect of custom, and were the subject approached anew with unfettered ideas, any engineer would be inclined to say that either loose wheels must be employed as they are on traction engines, or radiating axles must be used. The latter affords the best solution of the difficulty, for there is no doubt great advantage in fixing wheels to the axles. The paper referred to shows the value of this system, and points to the great advantage derivable from the system of lubricating the flanges of leading wheels, as now largely done on the Continent.

THE GEOMETRY OF SPACE.

By RD. RANDOLPH, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE word geometry is here used, notwithstanding its derivation which seems to limit the science to the earth, because there is no other word whose employment has made it the exponent of the same idea. If Euclid had anticipated the discussions that have lately arisen among mathematicians concerning his work, he would probably have called his science *pandiametry*, and have maintained that it was equally true if space alone existed. He would have rejected the appellation *planimetry*, lately given to it, as false, and insisted that the plane was one of his figures in space, and that when a discussion was based upon the assumption that lines and points were confined to the same plane, it was only for the convenience of description; their coincidence with the plane being only the expression of one of their conditions in space. It will be the endeavor of this article to do what Euclid himself would have done if he could have apprehended the consequences of certain defects and incompleteness in some of the foundation stones of his noble edifice which has nevertheless stood unimpaired for more than two thousand years, but which has of late years given birth among eminent mathematicians to a set of ideas concerning space which strike the ordinary mind as monstrous absurdities.

In VAN NOSTRAND'S MAGAZINE for December, 1876, is a discussion of this subject by Professor Helmholtz, and in the *Popular Science Monthly* for August, 1880, is an article by George B. Halstead, A.M., Ph. D., giving an account of the origin, development, and literature of this new departure in geometry. These seem to be the only sources of information of a popular character that have appeared in this country by which we can learn what the new ideas are. Taking these articles as fair exponents, it seems

that the impossibility of demonstrating some of the propositions upon which geometry is based, has necessitated the existence of modes or species of space, where the truth of the propositions may become apparent. And as the straight line, the square and the tube of geometry correspond to the 1st, 2d and 3d power of arithmetic, which series of powers can be continued without limit, while the series of geometrical magnitudes terminate at the cube, then such abrubt termination must be caused by reaching the limit of the space to which they belong, and that the process may be continued by passing over that boundary line and entering the domain of another space where a new geometrical figure will be formed, corresponding to the 4th power of arithmetic.

The geometrical magnitudes are conceived to be generated in two ways—by multiplication and by motion. But it is not possible to multiply a line by a line, any more than it is to multiply a volume by a volume, a surface by a surface, a pound by a pound, or an hour by an hour. A square yard is not obtained by multiplying a lineal yard by a lineal yard, but by multiplying a square foot by the square of 3. Nor is a cubic yard the 3d power of a lineal yard, but it is the product of a cubic foot by the 3d power of 3. So a lineal yard is the product of a lineal foot by 3. There is nothing in the nature of common space to prevent these multiplications being carried on without limit; the cubic foot or any other element by volume can be multiplied by any power of any number.

It is customary to say that a line is described by the motion of a point, a plane by the motion of a line, and a volume by the motion of a plane. These descriptions would be practically illustrated by saying that a line is the aggregate of the particles of chalk left in the track of a moving piece over a board; a plane is the aggregate of such particles left in the track of a number of contiguous pieces moving together over adjoining paths, and that the volume would be the cloud of particles formed in the air if the board were reversed and they fell to the ground. Now if these particles were only mental concepts of positions divested of all idea of extensions but unlimited in number, we would have the mathematical idea of geometrical magnitudes. But that there is no geometrical term to denote the figure described by the movement of a volume is according to Helmholtz, because there can be no mathematical idea of such a figure distinct from that of a volume, unless we appeal to another mode or species of space where the new figure will become apparent. Taking both the practical and the mathematical view of these magnitudes, it is evident that they are all nothing more than aggregates of points or positions arranged according to certain conditions. A line is an aggregate of points, a plane is an aggregate of lines, and a volume is an aggregate of planes and consequently of points, while all the points are positions in space. None of them are spaces nor parts of space. Therefore, none of the geometrical magnitudes are spaces, parts of space, or modes of space.

But the objection that the movement of a volume results only in describing a repetition of itself is not confined to the volume, for if the straight line be moved in the direction of its length, or if a circular curve be moved about an axis at the center of its circle, a line and nothing but a line will be described. So if a plane be moved in the direction of any of its lines at any point in the movement, a plane and nothing but a plane will be described. If these facts do not occasion the want of a new space, why should the fact that a volume moved in the direction of any of its lines will describe only another volume, require the existence of another mode of space?

It is quite as admissible to conceive a plane as being described by the movement of a point, as by the movement of a line, an idea that may be represented by the movement of the shuttle in disposing of the woof of a fabric. And suppose the line represented by the single thread of the woof, disposed in the form of a plane, to move, then there would be a volume described by a line. And even the point may describe the volume by moving over all the lines of all the planes.

But the principal ground of justification for the new ideas seems to be, that it has been found impossible to demonstrate the truth of certain propositions which lie at the base of Euclidian geometry, and that the difficulties can only be overcome by substituting the method of

Euclid by that of modern algebraical analysis, an instrument which gives access to the new variety of space transcending that contemplated by Euclid. As an illustration of the possibility of a transcendental intellect comprehending a distinct science of geometry belonging to such a space, attention is called to the fact that our intellect *does* comprehend a geometry which belongs to a species of space transcending that of a hypothetical being whose existence is limited to what we know as a mathematical surface. Here it is evident that the three geometrical magnitudes, in which ordinary minds can perceive nothing but variously disposed aggregates of points in space, are assumed to be three varieties of space. And all the properties of lines and angles drawn upon a variety of surfaces as properties peculiar to such distinct kinds of space, whereas the ordinary intellect of Euclid would have considered them as ordinary geometrical figures and magnitudes determined in accordance with prescribed conditions, such conditions being secured by confining them to surfaces of a certain character, themselves being figures in ordinary space.

The writers on this subject find themselves between the horns of a dilemma in being forced to choose between two words, these are the shortest in the English language, but have the most important signification. Some of these writers, Helmholtz for one, place the hypothetical beings *on* the surface, while others place them *in* the surface. If they are on the surface, their existence must be in that space outside of which they are supposed to know nothing. If they are in the surface, then it must have thickness enough to admit them and the *volume* of their persons. The supposition of reasoning beings existing within a geometrical surface, is both logically and mathematically impossible. It would, however, have been perfectly logical to suppose Professor Hilgard and his assistants of the Coast Survey having become monomaniacs owing to their long practice of measuring shortest lines between two points on the great circle of the earth, would utterly deny the proposition that there could not be two shortest lines between two points. And that they could no more have an idea of a shorter line, dipping into the bowels of the earth, between the same points, "than men born blind could have of colors," any such propositions from outsiders meeting with the same contempt as that with which the suggestion of *Alice in the Wonderland*, or *Alice through the Looking-glass*, were received by the inhabitants of those spaces, the dimensions of which the author of those books does not inform us.

But it cannot be denied that from Euclid to Legendre, the Alpha and Omega of the science of geometry, some of the main propositions have not been demonstrated, and some of the definitions have been incomplete and insufficient, while others have assumed as truth that which needed demonstration.

The object of this article is to amend these defects.

The first nine axioms of Euclid (Todhunter's edition) and of Legendre, are the only ones admissible as such. A better name, however, for these statements would have been *tautologisms*, as they are nothing more than repetitions of the same idea in different words. For instance, a whole is equal to all its parts. The meaning of whole is, all the parts, and the expression is equivalent to saying that the whole is equal to the whole. The whole is greater than any of the parts. The meaning of the word part is something less than the whole, so the expression is equivalent to saying that the greater is greater than the less. Things that are equal to the same things are equal to each other; which is, if A has the quantity C, and B has the quantity C, then both A and B have the quantity C, the same quantity. If equals be subtracted from equals the remainders are equal; which is, if A and B are equal to A and B then A is equal to A without B, etc., etc.,

The 11th axiom of Euclid, being the 10th of Legendre, viz., all right angles are equal, should have been expressed in the definition of right angles, viz. Equal angles, each one half of all the angles in a plane formed between a straight line and its extension beyond the vertex of the angles.

The 10th of Euclid, equivalent to the 11th of Legendre, viz., only one straight line can be drawn between two points, is a proposition to be proved; for the definition of a straight line, viz., a

line that does not change its direction at any point, does not justify the assumption, as there is no definition of a direction. A ship which sails constantly in the same direction sails in a circle: and it would not do to say that a direction was a straight line, for that would be reasoning in a circle.

The 12th of Legendre, Euclid having no corresponding one, viz.: the shortest distance between two points is measured on the straight line which joins them, depends upon the truth of the proposition that there can be but one straight line between two points which is not demonstrated.

The 13th of Legendre, Euclid having none to correspond, viz.: through the same point only one line can be drawn parallel to a given line, is another proposition requiring proof, for there is nothing in the definition of a straight line or of parallels by which the points of the lines can be located.

The 12th of Euclid is classed as an axiom by his translator, but by Euclid himself it offered as a *request* that its truth be granted. But in Legendre it is the 21st proposition of the 1st book, where it is pretended to be demonstrated; viz.: If the two included angles formed by two straight lines with a third line are together less than two right angles, the two lines will meet if sufficiently extended, it being understood that all the lines are in the same plane. Starting from the improved proposition that there can be but one straight line between two points, Legendre proves that whenever two straight lines form, with a third line included, angles whose sum is two right angles, they never meet; but does not pretend to prove the converse, that all lines which do not meet form included angles equal to two right angles. For all that is proved to the contrary, they may also not meet with angles less than two right angles. But by applying the term parallel to all lines that do not meet this omission is lost sight of in the conclusion. The demonstration is briefly thus: All lines that do not meet are parallel. Two lines which form included angles equal to two right angles cannot meet. (Supposed to be proved.) Two lines which form included angles less than two right angles must meet, because if they did not they would be parallel. But as the word parallel is only a substitute for the sentence, which do not meet, the conclusion is nothing more than this. They must meet, because if they do not meet they will be lines which do not meet.

As for the postulates, they may all be dispensed with by the proper definition of a point, viz., a position anywhere in space, devoid of extension, but unlimited in number within any limits. A straight line may extend between any two points, means that space will afford all the points of a straight line or any other line, surface or volume between any two points or any number of points. Space will also afford a central point of a line of the arc of an angle, bisecting the line or angle, or another point trisecting them, etc. And as there can be a point on the line representing every possible length less than the whole, there will be one coinciding with any lesser line in space, etc., etc.

Among the definitions there are only two that it is necessary to notice, that of the plane and of the straight line. These definitions are the same with both Euclid and Legendre. That of a plane, viz.: a surface such that if any two of its points are joined by a straight line, every point in the line will be in the plane, is a proposition without proof. Suppose a certain surface should be defined as one, such that if any two points were joined by an ellipse, every point of the ellipse would be in the surface. Such a surface could not exist in space. The definition of a plane cannot be pronounced impossible, but the possibility of it may legitimately be doubted. If such definitions were admissible, the gordion knot of the proposition of the parallels could have been cut long ago by defining parallel lines to be such, that a third line could intersect them at right angles to both at any point of either. But the possibility of this would have properly been doubted and the proof required.

The definition of the straight line is the fundamental defect in our system of geometery, and has caused most of the difficulties connected with it. This definition is the same with both Euclid and Legendre, viz.: a line uniform in its direction at every point. But how can the positions of the points of the line be determined by this definition. Suppose a

circle has been defined as a line having a uniform curvature at every point without defining curvature. But the definition of a circle is perfect when it is upon the condition of all the points being in the same plane. The straight line requires a definition by which all its points may be located quite as imperatively as that of a circle or a sphere: and as there is such a definition as simple and as undeniable as that of a circle, it should be used in any geometry.

The following definitions, propositions, and demonstrations, embracing all the objections to the system of Euclid, are submitted as amendments. It will be seen that all the axioms except the tautologisms are discarded, and all the propositions demonstrated. The discussion has reference to space, pure and simple, and the plane is only used as a convenient mode of expressing conditions in space.

Definitions.

1. Space is universal distance, co-extensive with that which is and that which is not. It is absolutely devoid of conditions, modes or form, but is itself the condition precedent of all existence or action either physical or mental.

2. A point is a position anywhere in space. It is devoid of extension, but unlimited in number within any limits.

3. A distance is the spatial relation of two points. It is a quantity independent of all other points.

4. A line is a succession of points, the length of which is the sum of the distances between its points.

5. A spherical surface is the aggregate of all the points that are equally distant from the same point, called the center of the sphere, the sphere being the aggregate of all such surfaces, having the same center within any given one.

6. A circle is a line, all of whose points are equally distant from the same two points in space.

7. A plane is the aggregate of all the circles which respectively have all their points equally distant from the same two points.

8. A straight line is a line of which all the points are respectively equally distant from the same three points in space.

9. Equal straight lines are those which have the same distance between their terminal points.

10. A straight line is continued when all the points of the continuation, as well as of the line are respectively equally distant from the same three points.

11. A radius is a straight line extending from any point of the circle to any point in space that is equally distant from all the points in the circle. When two such points are at the same distance from the circle each one is the vertex of a cone, and the radius is the cone radius of the circle (called slant height of the cone). The straight line whose points are respectively equally distant from the points of the circle, is the axis of the circular plane and of all the cones which have that circle for base, the cone being the aggregate of all its radii. When the point on the axis is equally distant between the vertices of two cones on the same circle and of equal radii, it is the center of the circle, and the radius to this center is the plane radius of the circle. Two plane radii, one of which is the continuation of the other, form the diameter of the circle.

12. An angle is the extent of the divergence from a common point of two straight lines. When these lines form radii to the same circle the angles are in proportion to the length of that portion of the circle included between them.

13. A triangle is the figure formed in space by the lines including an angle being connected by another straight line between any point on either line.

14. Equal angles are those which have the sides of the triangles to which they belong respectively, equal to each other. (Evident by superposition.)

15. Right angles are equal angles, each being equal to one-half of all the angles formed in a plane between a straight line and its continuation beyond the vertex of the angles. When one line forms right angles with another, it is said to be perpendicular to it.

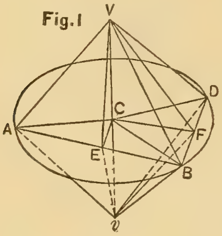
16. Parallel lines are lines in a plane having every point of one at the same distance from the nearest point of the other.

Propositions.

1. Any three points determine the center of the circle to which they belong and all the points of its axis.

Let A, B and D be any three points in space, and let them be connected by straight lines. From the centers, E and F, of these lines, let perpendiculars to them, EV and FV, be extended so as to intersect each other at any point V. Then bring the terminal points of perpendiculars of the same length, Ev and Fv, to a common point v. Also let two other perpendiculars intersect at a point C equidistant between V and v.

Fig. I



As the angles AEV and BEV are equal, and the sides AE and EB also equal, with the side E V in common, the sides AV and BV are equal (Def. 14) For the same reasons the sides BV and DV are equal. Therefore the three cone radii AV, BV, and DV are equal, which are equal to their counterparts Av, Bv, and Dv by construction. In the same way the radii AC, BC and DC are proved to be equal. And as C is equidistant between V and v, they are radii of the circle ABD (Def. 11). These three points VC and v being respectively equidistant from the same three points A, B and D are points of the same straight line (Def. 8). As V and v are any points in space an unlimited number of such points may be determined by an unlimited number of intersections of the perpendiculars to the lines AB and BD. This straight line is the axis of the circle ABD, and of all the cones having it for base (Def. 11).

Corollary.—All the plane radii of the circle are at right angles with its axis. For the sides VB and VC are respectively equal to the sides vB and vC, and the side CB is in common. Therefore, the angle VCB and vCB are equal (Def. 14), and are right angles (Def. 15). In the same way the axis can be proved to be at right angles with any radius to the circle.

2. All of the following propositions are demonstrated by a reference to the definition of equal angles (Def. 14).

A triangle with three equal sides has its three angles equal.

A triangle with two equal sides has the angles opposite the equal sides equal.

A triangle with two equal angles has the sides opposite to the equal angles equal.

If in two triangles the sides are respectively equal, the angles opposite to the equal sides will be equal.

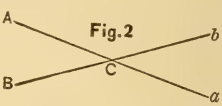
If in two triangles two sides are respectively equal, and the angle included between them equal, the triangles are equal in all their parts.

If in two triangles two of the sides are respectively equal, and an angle in each opposite to one of the sides equal, the triangles are equal in all their parts.

If in two triangles two of the angles are respectively equal, and the side included between them equal, the triangles are equal in all their parts.

If in two triangles two of the angles are respectively equal, and a side in each opposite to one of the angles equal, the triangle will be equal in all its parts.

3. The angle formed by two straight lines is equal to that formed by the extension of those lines beyond the common point. The angles ACb and AcB



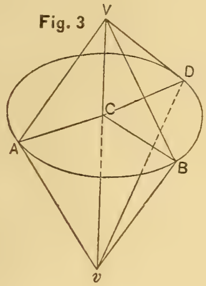
are equal to the angles Acb and aCb, as both sums are equal to two right angles (Def. 15), therefore AcB is equal to aCb (axiom).

4. There can be but one straight line between the same two points.

Let V and v be the two points, and let them be the vertices of two cones based on the circle ABD, whose plane is intersected by the two straight lines between V and v at any point where they are supposed to deviate from each other. As each point in the same straight line is equidistant from the same three points (Def. 8.), and two of these points, V and v, are each equidistant from the points A, B and D, by construction all the

points in either straight line between V and v must each be equidistant from these points. Therefore, neither can intersect the plane of the circle at any

Fig. 3

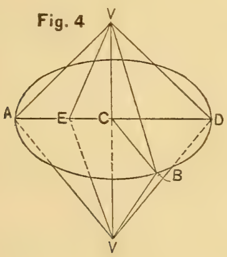


other point than at c, the center of the circle. Therefore, the supposed deviation is impossible; and, as this reasoning applies to any point of supposed deviation, the two lines cannot deviate at any point, and are, therefore, but one line.

5. There can be but one perpendicular to the same straight line extended from the same point.

Let V be the point from which the perpendiculars are to be extended to the straight line ACD, which is the diameter of the circle ABD. Let c, the center of the circle, be the point where one of the perpendiculars intersects the diameter, and let AV, BV and DV be equal. The

Fig. 4



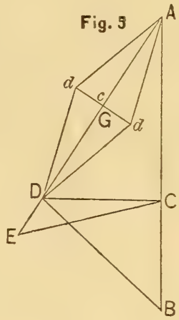
line VC is perpendicular to the diameter, because AV and Ac are equal to DV and Dc, with CV in common (Prop. 2). If there is another perpendicular to the same line it will intersect it on either side of C, at E. But it cannot form equal angles with it, because AE will be less than ED (Prop. 2). Therefore, there can be but one perpendicular from the same point.

Corollary.—There can be but one perpendicular to a plane from the same point; for the same reasoning can be applied to any diameter of the circle ABD.

6. The length of a straight line is less than that of any other line between the same points.

Let ACB be the straight line between the points A and B, and let D be a point in the line not straight which extends between the same points, and let DC be perpendicular to ACB. Then the straight line AD will be longer than AC, and the straight line DB will be longer than CB. For if AD and AC were equal the angles ADC and ACD would be equal (Prop. 2), and both would be right angles, as the angle ACD is so by construction. But this would be two perpendiculars

Fig. 5

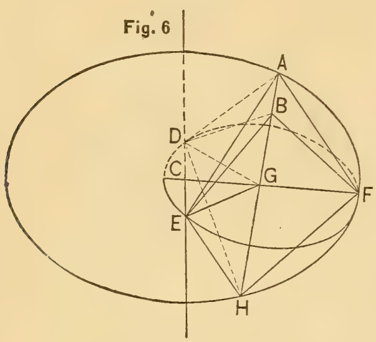


from the same point, which is impossible (Prop. 5). Therefore the lines are not equal. If AD is shorter than AC, let it be extended to E, where it will be equal to AC and connect E and C with a straight line. Then the angles AEC and ACE will be equal, as they are opposite equal sides (Prop. 2). But the angle ACE exceeds the right angle ACD by the angle DCE; therefore, both angles are greater than a right angle, which is impossible, as the sum of any two angles of a triangle is less than two right angles. (Euclid, Prop. 17, Book I.) Therefore, the line AD is neither equal nor less than the line AC, and must be greater. In the same way DB is proved to be longer than CB.

Again, let d be another point of the line not straight on either side of AD, and let a perpendicular extend from d to c on the line AD. Then by the preceding reasoning the straight lines Ad and

dD are proved to be longer than Ac and cD. Thus every pair of straight lines is proved to be longer than the single straight line joining the same points. In the same way the line not straight may be continually subdivided, and every pair of straight lines proved to be longer than the single one between the same points. Thus a constantly increasing sum of straight lines is obtained until it reaches the last series of straight lines which are the distances between the ultimate points of the line not straight, if it should be a curve. Therefore, it is longer than the first straight line between the two terminal points.

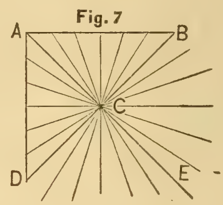
7. If two points of a straight line are in a plane, all the points of that line are in the same plane.



Let A and B be any two points, in the plane of which DCE is the axis and C the center, the points D and E being any points equidistant from C. As a plane is the aggregate of all the circles which respectively have all their points equidistant from the same two points (Def. 7), and as these points are anywhere on the axis at equal distances from the center (Def. 11), the points A and B are respectively equidistant from D and E. Now, let the distance of these points to A and that to B be extended to a common point in the plane, F. Then A and B will be respectively equidistant from the same three points D, E and F. Therefore, all the other points of that straight line are respectively equidistant from D, E and F. (Def. 8.) And as every point in the plane is respectively equidistant from D and E, all the points of the straight line ABGH, whose points are respectively equidistant from D, E and F, are in the same plane.

Carollary.—All the straight lines extending from a common point and intersecting another straight line have a common perpendicular at the common point. For if the intersected line is sufficiently continued, two of the points of intersection are equally distant from the common point. In whatever circle these points may be they will be in the plane to which the circle belongs. And as all its other points are in the same plane the line will be intersected by all the plane radii of the circle, and which has its axis perpendicular to them at the center of the circle.

8. A straight line may be perpendicular to a plane at any point, that is, be at right angles with all the straight lines of the plane which radiate from that point.



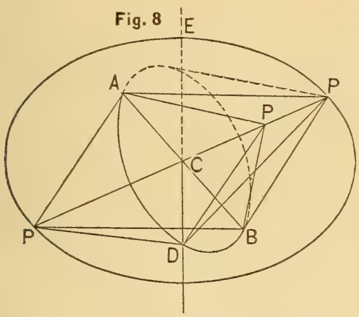
As the axis whose points determine a plane is perpendicular to all the plane radii of its circles (Prop. 1, Corollary); and as a straight line which is intersected by two of these radii has all its other points in the same plane (Prop. 7), it follows that all the straight lines radiating from a common point and which intersect another straight line may have a common perpendicular at that point. If C be any point in a plane and the straight line AB in the same plane, then a straight line can be perpendicular at C to all the straight lines which may radiate from that point and intersect the line AB and consequently to all their extensions between D and E. (Def. 15). So all the lines which radiate from C and intersect the line AD, also in the same planes, and all their extensions between B and E, may have a common perpendicular at C. But as these two sets of lines have AE BDi n common, all the lines which radiate from c in the same plane have a common perpendicular at C.

9. If a circle has three of its points in a plane, then all of its points and its center are in the same plane.

As it requires three points to determine the points of the axis of the circle to which they belong; and as the lines which extend from these points to a common point in the axis and form right angles with it are plane radii of the circle, and the common point is the center of the circle (Prop. 1, Corrolary), it follows that the perpendicular to the plane at a point equidistant from the three points of the circle in the plane is the axis of the circle containing those points, and the equidistant point is the center of the circle. Because all the straight lines of a plane which radiate from any point have a common perpendicular at that point (Prop. 8). And as all the plane radii of the circle are at right angles with the axis, all the points of the circle are in the plane.

10. If the line is in a plane, it is a straight line when all its points are respectively equidistant from the same two points in the plane.

Fig. 8



For if the two points, A and B, be connected with a straight line and at a point on this line, C, that is equidistant from A ane B, there be a perpendicular to the plane, ECD, and upon this perpendicular there be a point D, the same distance from C as A and B; then any other points in the plane which are equidistant from A and B will also be equidistant from D on the perpendicular. Let P, P and P be such points, respectively equidistant from A and B. Then if AP and BP are equal and AC and BC are equal, a line from C to P must be at right angles to AB (Def. 14), and the angle DCP is a right angle because the perpendicular DC is at right angles with every line in the plane radiating from C (Prop. 8). The triangles ACP, BCP and DCP having two sides of one respectively equal to two sides of the other, and the included angles equal, the other sides, AP, BP and DP are equal (Prop. 2). Each point being the same distance from the two points in the plane must be the same distance also from the point on the perpendicular. Therefore the points in the line of the plane which are respectively equally distant from the same two points in the plane are respectively equidistant from the same three points in space; which is the definition of a straight line.

11. If the line is in a plane, it is a circle when all its points are equally distant from the same point in the plane.

A circle being a line whose points are all equally distant from the same two points in space, if these two points are connected with a straight line and from a point on this line, equally distant from the other two, straight lines be extended to every point in the circle, they will be at right angles with the first line. (Def. 14). As all the lines in the plane which radiate from any point have a common perpendicular at that point, if the points in the line of the plane are equidistant from this point they are equally distant from the same two points in space, and which are on the perpendicular.

12. If, in a plane, a connected series of equal straight lines from equal angles with each other, those less than two right angles being on the same side of the lines; and these angles are bisected by straight lines, then points on these bisecting lines which are equally distant from the vertex of the angles are the connecting points of another series of equal straight lines forming equal angles with each other, those less than two right angles being on the same side of the lines as of the first series.

Let the first series be composed of the equal lines AB, BC, CD and DE, and connect each alternate point with the lines AC, BD and CE which will be equal (Prop. 2). Then the equal lines A*a*, B*b*, C*c*, D*d* and E*e* which bisect the equal angles ABC, BCD and CDE will bisect the lines AC, BC and CE (Prop. 2), and are at right angles with them (Def. 15). Also the angles AB*a*, BA*b*, BC*b*, CB*c*, CD*c*, DC*d*, DE*d* and ED*e* are equal (Prop. 2).

These being taken from the equal angles of the hypothesis there remain the equal angles *b*B*a*, *b*B*c*, *c*C*b*, *c*C*d*, *d*D*c* and *d*D*e*. These angles being included by sides which are respectively equal in their triangles, the remaining sides *ab*, *bc*, *cd* and *de* are equal. Also the angles opposite to the equal sides of these triangles, A*ba*, C*bc*, B*cb*, D*cd*, C*dc* and E*de* are equal. These being subtracted from the right angles A*bx*, B*ax*, B*cx*, C*bx*, C*dx*, D*cx*, D*ex*, and E*dx*, there remain the equal angles formed on each side of the bisecting lines with the second series of equal same line (Prop. 5): and the lesser angle must be inside the triangle as the sum of any two angles of a triangle is less than two right angles (Euclid, Prop. 17, Book 1): which shows the lesser angle is on the same side as of the other series. Therefore the proposition is true whatever may be the distance from the vertex of the first series less than the distance to the common point of the bisecting lines if they do meet at such point.

If the lines of the second series are on the side of the greater angles, let the first be represented by the lines *ab*, *bc*, *cd* and

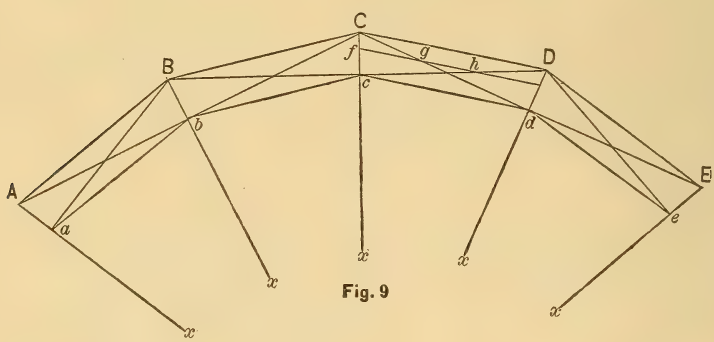


Fig. 9

lines *ab*, *bc*, *cd* and *de*; the angles being less than a right angle, and the lesser angle being the same side as of the first series. In the same way a third series of equal lines with equal angles less than two right angles will connect points on the bisecting lines equally distant from the vertex of these angles; and the process repeated as long as there is any distance between the bisecting lines.

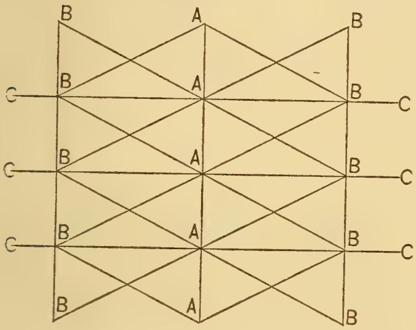
If the points on the bisecting lines do not coincide with any of the lines connecting alternate points of the series they will occur between them and equidistant from the vertex of the angles. Let *f* and *i* be such points and be connected by the line *fghi*, to be repeated between all the bisecting lines. These lines and the angles which they form with the bisecting lines can be proved to be equal to each other in the same way the second series were, by diagonal lines. At *g* and *h* they cross the lines C*d* and D*c* which were proved to be at right angles with the bisecting lines; and therefore form angles with them less than right angles, as there cannot be two perpendiculars from the same point to the *dc*. Then let the lines be drawn at right angles with the bisecting lines from the vertex of the angles *a*, *b*, *c* and *d*. These lines will intersect at A, B, C and D. These points are proved to be equidistant from *a*, *b*, *c* and *d* from the triangles B*ba* and B*bc*, &c., &c., having two angles respectively equal and the included sides *ab*,and *bc*, &c.,&c. equal. The lines AB,BC &c., are proved to be equal to each other by the respectively equal sides of triangles which include the right angle B*c*C, D*c*C, &c., which proves the angles BC*c*, DC*c*, &c., to be equal and less than a right angle, as there cannot be two perpendiculars from the same point to the line C*c*. This process can be repeated continually until reaching the given points on the bisecting lines: and if such points should not coincide with the intersections A, B, C, D and E, they will occur between two of them, where the truth of the proposition can be proved as before by the line *fghi*.

13. If, in the same plane, several straight lines intersect another straight line at right angles, points on these perpendiculars that are equally distant from

the points of intersection are the same distance apart as the points of intersection.

Let AC be the several straight lines intersecting the straight line AA at the equidistant points A, and let the distances AB be equal. These being at right angles with the line AA, the diagonals AB must be equal (Prop. 2) which would make the triangles AAB equal in all their parts. The equal acute angles AAB being taken

Fig. 10



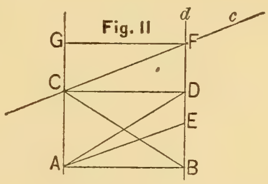
from the right angles AAB leave the angles BAB equal; and the sides including them being respectively equal in the triangles BAB, these triangles are equal in all their parts (Prop. 2). Therefore the lines BB and the angles BBA are equal. If the greater angles BBA are less than right angles, the angles BBC are greater than right angles. If they are greater than right angles the angles BBC are less. As there is a set of these equal lines and angles on each side of the line AA with corresponding angles on the side towards the straight line AA, it follows that if these angles are not right angles the straight line AA is a curved line deflecting in opposite directions at the same points. For, according to the preceeding proposition, if the connected series of straight lines BB form equal angles with each other, which are bisected by the straight lines AB, the points A on these lines which are equidistant from B, the vertex of the angles are the connecting points of another series of straight lines, AA, which also form equal angles with each other less than two right angles, the lesser angles being on the same side of the line AA as of the line BB. Therefore the supposition that the angles BBA and BBC are not right angles is absurd. Therefore they are right angles and the line BB is a straight line. As the greater angles BBA are proved to be right angles and equal to the angles AAB, and as the sides of their triangles, the vertical AB and the diagonal AB, are respectively equal, the triangles are equal in all their parts (Prop 2). Therefore the lines AA are equal to the lines BB, which was the proposition to be proved.

Corollary.—If two straight lines are at right angles to a third straight line, another straight line may be at right angles to both at any point of either.

13. If, in a plane, two straight lines are at right angles to a third straight line, they are parallel; and when they are intersected by a third straight line, the angles formed on the same side of the third line, and, respectively, on the same side of the parallel lines, are equal; and the angles included by the parallels and on opposite sides of the third line are equal.

Let AC and BF be the two straight lines at right angles to the line AB. At any point, C, in the line AC let the line CD be at right angles with AC. Then by the last proposition CD is equal to AB

Fig. 11



and BD is equal to AC; therefore the angle CDB is also a right angle. In the demonstration of proposition 5, it was proved that the shortest line from a point to any point in a straight line was the straight line which was perpendicular to it. Therefore the point A is nearer to the point B than any other point in the line BF, and the point C is nearer to the point D than any other point in the line BF. Likewise the point B is nearer to the point A than any other point in the line AC, and the point D is nearer than any other point in the line AC. Therefore the lines AC and BD are parallel. (Def.16.) Also

let CG be equal to DF. Then, according to the last proposition, GF is equal to CD and the angles G and F are right angles. Therefore the triangles CFG and CFD are equal in all their parts, for they have two sides respectively equal, one side in common and the included angles G and D right angles. Therefore the angles CFD and that on the other side of the parallel line, *cFd*, are each equal to FCG.

The triangles ADB and CBD being equal in all their parts, the angle ADB is equal to CBD. And the triangles BAD and ABC being equal in all their parts, the angle BAD is equal to ABC. Therefore ABC and CBD are equal to BAD and ADB are equal. Therefore the right angle of the triangle ABD is equal to the other two angles, which proves that the sum of the three angles of a right angled triangle is equal to two right angles. And since every triangle can be divided into right angled triangles by a perpendicular to the longest side from the vertex of the angle opposite; and the sum of the lesser angles of each, being each equal to a right angle, it follows that the sum of the three angles of any triangle is equal to two right angles.

Thus are all of the fundamental propositions of geometry rigorously demonstrated without the aid of doubtful definitions or axioms, and without appealing to a space of x dimensions.

APPROXIMATIVE PHOTOMETRIC MEASUREMENTS OF SUN, MOON, CLOUDY SKY, AND ELECTRIC AND OTHER ARTIFICIAL LIGHTS.*

From "Nature."

Sir William Thomson pointed out that the light and heat perceived in the radiations from hot bodies were but the different modes in which the energy of vibration induced by the heat was conveyed to our consciousness. A hot kettle, red hot iron, incandescent iron, platinum, or carbon, the incandescence in the electric arc, all radiate energy in the same manner, and according as it is perceived through the sense of sight, by its organ the eye, or by the sense of heat,† we speak of it as light or heat. When the period of vibration is longer than one four-hundred-million-millionth of a second the radiation can only be perceived by the sense of heat; when the period of vibration is shorter than one four-hundred-million-millionth of a second, and longer than one eight-hundred-million millionth of a second, the radiation is perceived as light, by the eye.

Pouillet, from a series of experiments, deduced a value of the energy radiated by the sun, equal in British units to about 86 foot-pounds per second per square foot at the earth's surface, or about 1 horse-power to every $6\frac{1}{2}$ square feet of the earth's surface. We may estimate from this the value of the solar radiation at the surface of the sun. The sun is merely an incandescent molten mass losing heat by radiation, and surrounded by an atmosphere of incandescent vapor, so that the radiant energy really comes out from any square foot or square mile of the sun's surface, as from a pit of luminous fluid which we cannot distinguish as either gaseous or liquid. Take, however, instead of the sun, an ideal radiating surface of a solid globe of 440,000 miles radius. The distance of the earth being taken as 93 million miles, the radius of the sun is equal to, say in round numbers, one two-hundredth of the earth's distance, hence the area at the earth's distance corresponding to one square foot of the sun's surface, is equal to 40,000 square feet. The radiation on this surface is (40,000 $\times$ 86, or) 3,440,000 foot-pounds, which is therefore the amount of radiation from each square foot of the sun's surface. This amounts to about 7,000 horse-power, which, accord-

---

* Abstract of lecture at the Glasgow Philosophical Society, by Sir William Thomson, F.R.S.

† Sometimes wrongly called the sense of touch. The true list of the senses, first given, I believe, by Dr. Thos. Reid, makes two of what used to be called the sense of touch, so that, instead of the still too common wrong-reckoning of five senses, we have six, as follows :—

Sense of Force.	Sense of Light.
" Heat.	" Taste.
" Sound.	" Smell.

ing to our brain-wasting British measure, we must divide by 144, if we wish to know the radiation per square inch of the sun's surface, which we thus find to be 50 horse-power.

The normal current through a Swan lamp giving a 20-candle light is equal to 1.4 amperes with a potential of 40 to 45 volts. Hence the activity of the electric working in the filament is 61.6 ampere-volts or Watts (according to Dr. Siemens' happy designation of the name of Watt, to represent the unit of activity constituted by the ampere-volt). To reduce this to horse power we must divide by 746, and we thus find about 1-12th of a horse-power for the electric activity in a Swan lamp. The filament is $3\frac{1}{2}$ inches long, and .01 of an inch in diameter of circular section; the area of the surface is thus 1-9th of a square inch, and therefore the activity is at the rate of 3 4ths of a horse-power per square inch. Hence the activity of the sun's radiation is about sixty-seven times greater than that of a Swan lamp per equal area, when incandesced to 240 candles per horse-power.

In this country the standard light to which photometric measurements are referred is that obtained from what is known as a standard candle. Latterly, however, objections have been raised against its accuracy. It has been said that differences of as much as 14 per cent. have been found in the intensity of the light given by different standard candles, and that various differences have been observed in the intensity of the light from different parts of the same candle in the course of its burning. The Carcel lamp, the standard in use in France, has been regarded as the only reliable standard. It is, no doubt, very reliable and accurate in its indications, but it should be remembered that its accuracy is greatly owing to the careful method and the laborious precautions taken to secure accuracy. If something akin to the precautions applied to the Carcel lamp by Regnault and Dumas were applied to the production and use of the standard candle, there is little doubt but that sufficient accuracy for most practical purposes could also be obtained with it; probably as good results as are already obtained by the use of the Carcel lamp.

At the Conference on Electrical Units which met in Paris lately, a suggestion was made to use as a standard for photometric measurements the incandescence of melting platinum, and very interesting results and methods in connection with the proposal were presented to the meeting. According to experiments by Mr. Violle, which M. Dumas reported to the Conference, a square centimeter of liquid platinum at the melting temperature gives of yellow light seven, and of violet twelve times the quantities of the same colors given by a Carcel lamp. The apparent area of the Swan filament, being one-ninth of a square inch, is .23 of a square centimeter, and when incandesced to 20 candles must be about as bright as the melted platinum of Mr. Violle's experiment, as the 7 carcels of yellow and 12 of violet must correspond to something like 10 carcels or 85 candles, in the ordinary estimation of illumination by our eyes. The tint of Mr. Violle's glowing platinum cannot be very different from that of the ordinary Swan lamp incandesced to its "20 candles." Thus both, as to tint and brightness it appears that melted platinum at its freezing temperature is nearly the same as a carbon filament in vacuum incandesced to 240 candles per horse-power.

For approximative photometric measurements the most convenient method is certainly that of Rumford, by a comparison of the shadows cast by the sources of light on a white surface. The apparatus necessary are only a piece of white paper, a small cylindrical body such as a pencil, and a means of measuring distances. Ordinary healthy eyes are usually quite consistent in estimating the strength of shadows, even when the shadows examined are of different colors, and with a reasonable amount of care photometric measurements by this method may be obtained within 2 or 3 per cent. of accuracy. The difference in the colors of the shadows is of course due to each shadow being illuminated by the other light.

Arago has compared the luminous intensity of the sun with that of a candle, and estimates it as equal to about 15,000 times that of a candle flame.

Seidel, as Sir W. Thomson had been informed by Helmholtz, estimated the luminous intensity of the moon as about equal to that of grayish basalt or sand-

stone. An experiment on sunlight made in Glasgow on the 8th of this month (since this paper was read), compared with an observation on moonlight, which he made at York during the meeting of the British Association there in 1881, had led him to conclude that the surface of the moon radiates something not enormously different from one-quarter of the light incident upon it. It would be exactly this if the transparency of the Glasgow noon atmosphere of December 8, 1882, had been exactly equal to that of the York midnight atmosphere of September, 1881, referred to below, for the respective altitudes of the sun and moon on the two occasions. The observation on moonlight referred to above showed the moonlight at the time and place of the observation (at York early in September, 1881, about midnight, near the time of full moon) to be equal to that of a candle at a distance of 230 centimeters. The moon's distance (3.8×10^{10} cm.) is 1.65×10^{8} times the distance of the candle. Hence, ignoring for a moment the loss of moonlight in transmission through the earth's atmosphere, we find $(1.65 \times 10^{8})^2$, or 27 thousand million million as the number of candles that must be spread over the moon's earthward hemisphere painted black, to send us as much light as we receive from her. Probably about one and a half times as many candles, or say forty thousand million million would be required, because the absorption by the earth's atmosphere may have stopped about one-third of the light from reaching the place where the observation was made. The moon's diameter is 3.5×10^{8} centimeters, and therefore half the area of her surface is 19×10^{16} square centimeters, which is nearly five times forty thousand million million. Thus it appears that if the hemisphere of the moon facing the earth were painted black and covered with candles standing packed in square order touching one another (being say one candle to every five square centimeters of surface), all burning normally, the light received at the earth would be about the same in quantity as estimated by our eyes, as it really is. It would have very much the same tint and general appearance as an ordinary theatrical moon, except that it would be brightest at the rim and continuously less bright from the rim to the center of the circle where the brightness would be least.

The luminous intensity of a cloudy sky he found about 10 A. M. one day in York during the meeting of the British Association to be such that light from it through an aperture of one square inch area was equal to about one candle. The color of its shadow compared with that from a candle was as deep buff yellow to azure blue, the former shadow being illuminated by the candle alone, the latter by the light coming through the inch hole in the window shutter.

The experiment on sunlight of last Friday (December 8) showed, at 1 o'clock on that day, the sunlight reaching his house in the University to be of such brillancy that the amount of it coming through a pin-hole in a piece of paper of .09 of a centimeter diameter produced an illumination equal to that of 126 candles. Tihs is 6.3 times the 20 candle Swan light, of which the apparent area of incandescent surface is .23 of a square centimeter or 3.8 times the area of the pin-hole. Hence the sun's surface as seen through the atmosphere at the time and place of observation was 24 times as bright as the Swan carbon when incandesced to 240 candles per horse-power. By cutting a piece of paper of such shape and size as just to eclipse the flame of the candle and measuring the area of the piece of paper, he found about 2.7 square centimeters as the corresponding area of the flame. This is 420 times the area of the pin-hole, and therefore the intensity of the light from the sun's disc was equal to (126×420) about 53,000 times that of a candle flame. This is more than three times the value found by Arago for the intensity of the light from the sun's disc as compared with that from a candle-flame; so much for a Glasgow Dec. sun!

The .09 cm. diameter of the pin-hole, of the Glasgow observation, subtends at 230 centimeters distance, an angle of 1/2556 of a radian; which is 23.7 times the sun's diameter (1/108 of a radian). But at 230 cm. distance the sunlight through the pin-hole amounted to 126 times the York moonlight (which was 1 candle at 230 cm. distance). Hence the Glasgow sunlight was [$(23.7)^2 \times 126$ times or] 71,000 times the York moonlight. We cannot therefore be *very far* wrong in estimating the light of full

moon as about one-seventy-thousandth of the sunlight anywhere on the earth. This, however, is a comparison which, because of the probably close agreement of the tints of the two lights, can probably be made with minute accuracy; and we must therefore not be satisfied with so very rough an approximation to the ratio as this 70,000. A lime light, or magnesium light, or electric arc-light, carefully made and remade with very exactly equal brilliance, for each separate observation of sunlight and moonlight, might be used for intermediary.

CURRENT-METER MEASUREMENTS IN THE RHINE, BELOW THE BRIDGE OF CONSTANCE.

By ADAM BAUM.

"Allgemeine Bauzeitung." Abstracts of the Institution of Civil Engineers.

In studies for lowering the high-water level of the Boden See, it became necessary to ascertain the discharge with different levels of the water in the lake. The measurements were made at a cross-section of the Rhine, below the bridge of Constance, near the point of discharge from the Boden See. For ordinary conditions of the water-level the stream has here a conveniently bounded profile. At the highest water level the stream floods the banks, but the quantity flowing beyond the limits of the channel is vanishingly small compared with that flowing in the ordinary bed. The cross-section has a breadth of 136.13 meters (450 feet), and a maximum depth of 11.4 meters ($37\frac{1}{3}$ ft.)

Surface-fall.—To determine this, nine gauges were fixed, six on the left shore and three on the right. The highest on the left, the Rhein-thorthurm gauge, was that to which all measurements were referred. To determine the surface-fall with different levels of the water in the lake, thirty-three sets of readings of the levels on the gauges are available.

The author has plotted these results, which exhibit great differences. A discussion of them leads to the formula—

$$J = 0.000067541 - 0.00000173474u,$$

where J is the relative fall, and u the height of the water-surface on the principal gauge. The zero point on this gauge is at the highest known water-level.

Measuring Apparatus.—The supports for the current-meter were placed on a platform between two coupled boats, each 33 feet long by 6 feet beam. The current-meter was attached to a fixed vertical T-iron (4 inches by $2\frac{3}{8}$ inches). The arrangements for fixing this and for raising and lowering the meter are described. The current-meter was fixed with its axis normal to the cross-section, and was not directed by a rudder or vane. It had a screw of 4.7 inches diameter driving a worm-wheel, making one rotation to one hundred of the screw. The worm-wheel carried a pin making electrical contact once in each revolution, so that a bell sounded at each hundred rotations of the screw.

Determination of the Constants of the Meter.—The observations give merely the time of one hundred revolutions, and the velocity of the water is not directly proportional to the speed of the meter. It is necessary, therefore, to find a relation between the time z per hundred revolutions, or the number of revolutions n per second, and the velocity v of the water. Then $n = \frac{100}{z}$, and $v = f(n)$ is the relation required. To determine this relation a number of trials were made in still water. The boats carrying the meter were guided by a fixed wire rope, and moved by a windlass. One observer noted the time of one hundred revolutions, and another the distance traversed along the guide rope. Applying the method of least squares, the author finds the equations—

(α) In the direction of the wind,
$v = -0.1271 + 0.3706n$.

(β) Against the wind,
$v = 0.12779 + 0.25104n$.

(γ) From both combined,
$v = 0.02514 + 0.259533n$,

v being in meters per second. The discrepancy of these equations, which is hardly explainable as due to the wind, which was always very slight, leads the author to discard them. The negative value of one of the constants is also impossible. The proceeding ultimately adopted was this: The rotations per second and corresponding velocities were plotted in a diagram as abscisses and ordinates. Through the points so found a provisional straight line was drawn. One point in this line near its upper end was assumed as accurately fixed, and from this and the $m-1$ other values of n and v equations were formed to determine the two constants. The arithmetical mean of these two values gave the following equations:

(α) In the direction of the wind,
$$v=0.06037+0.28168n.$$
(β) Against the wind,
$$v=0.08844+0.26892n.$$
(γ) Mean of both,
$$v=0.0744\ \ +0.2753n.$$

The difference in the first constant is narrow, and indicates a small current reverse to the direction of the wind, at the depth at which the meter was placed. The author discusses some results with the meter differently supported, and concludes that formula (γ) may be used in reducing the observations with the meter in all cases.

Surface-velocity Curves.—The author gives the surface-velocity curves for three conditions of the river. The curves are too irregular to be approximated to any geometrical figure.

Vertical-velocity Curves.—These in general are similar to those obtained in other researches. The exceptional form of some of the curves is due (1) to the friction of the contact-maker, which, when the velocity was very small, had a proportionately greater effect in retarding the meter; (2), to peculiarities of the river-bed, the irregularities of which influenced the position of the point of greatest velocity; (3), to the nearness of the point of discharge from the Boden See. The velocity at each point was due partly to the surface fall at the section, partly to the pressure of the Boden See considered as a reservoir. The peculiarly deep position of the point of greatest velocity in some of the curves may be explained as due to these causes.

Variations of the Velocity at one Point.—The meter was fixed in a selected position in the cross-section, and from the bell signal the time of each successive hundred revolutions was taken for a period of two hours. The results plotted show a continual variation of the velocity. Further, a wavy line can be drawn through the observations, taking a mean position between the shorter oscillations, and having a length from crest to crest corresponding to a period of about an hour.

In order, therefore, to ascertain the accurate velocity at any one place in the cross-section, it would be necessary to extend the observations over the whole of such a period. That is, to observe the time of about six thousand revolutions of the meter. But as this is not practicable, the observations taken in short periods must always be affected by considerable irregularities.

The author then considers observations on a series of verticals, to determine the variation of velocity with varying level of the river. By dividing the area of a vertical velocity-curve by the depth of the river, the mean velocity at that vertical in the given condition of the river is obtained. Setting off these mean velocities as abscisses, with the gauge-reading as ordinate, curves are obtained giving the mean velocity at each vertical for every condition of the river. The curves show great irregularities. These are due to various causes. Partly to the variation of the velocity at each point already discussed, partly to the rising or falling of the water during the observations, partly to the boat not being fixed exactly normal to the cross-section, and partly to the whirling motion of the water, which the author discusses at length.

The discussion of the vertical and horizontal velocity-curves cannot be rendered intelligible without the original drawings.

Discharge.—From the curves of the observations the author obtains a table giving the mean velocity on each vertical for each foot fall of the water-surface of the gauge from 2 feet to 12 feet. The discharge for each of these conditions of the river is obtained by multiplying the area of the cross-section between each

pair of verticals by the mean of the mean velocities on those verticals. For a portion of the section towards each shore the velocity for low conditions of the river was zero. The mean radius and mean velocity has also been reckoned for each of the same gauge readings.

Comparison of measured Discharge with Formulas.—The author has calculated the mean velocity of the river for each level from 2 feet to 12 feet on the gauge by seventeen well-known formulas. The calculated values show very great departures from the measured velocities, especially for low conditions of the river. The author then recalculates from the measured velocities the constants of the formulas, choosing the observations with 5 feet gauge-reading for formulas with one constant, and those at 5 feet and 10 feet for formulas with two constants. Recalculating the mean velocity for all levels of the river with these new constants, there are still extremely wide departures from the measured velocities.

The author then describes some observations with surface-floats in those parts of the river where the velocity was too small for the accurate use of the current meter. Also some observations with rods made to check the observations with the current-meter. The rods were of wood, projecting 20 to 24 inches above water, and reaching to within 16 to 24 inches of the bottom. The agreement with the current-meter observations is satisfactory, but the floats give in general a slightly higher value of the mean velocity than the meter. The difference is due partly to the path of the rods not being quite exactly known, part to the rods not extending to the bottom of the river.

CORRECTIVE DIVISION: AN ABRIDGEMENT OF THE ORDINARY PROCESS OF LONG DIVISION.

By J. BRUEN MILLER, M. S.

Contributed to Van Nostrand's Engineering Magazine.

Division, or the process of ascertaining how many times one number is contained in another, is universally performed by one method. For convenience, the terms "short" and "long" division have been adopted, but in reality the two processes are identical. Short division is the term employed to signify the division of one number by another, when the divisor is a single digit, or when it is one of the numbers whose multiples from 1 to 12 inclusive are obtained from the multiplication table. In short division, the progressive steps of multiplication, subtraction and addition, which are essential to division, are performed *mentally*, familiarity with the multiplication table enabling the student to reach the desired ends without the necessity of ciphering the intermediate results. In long division, the divisor is so large a number that the progressive steps cannot be performed mentally, and the student is compelled to have recourse to ciphering. In method and analysis the processes are identical. Short division is *mental* long division, and long division is *written* short division.

It is true that examples which should properly be solved by long division are often solved by short division, owing either to the readiness of reckoning possessed by the student, or to the peculiar nature of the divisor, as for instance, if the latter be a multiple of 10, or a factor of 100, etc. Instances of this sort are numerous, but no matter how great the skill and reckoning possessed by the student or operator, they are the exceptions, not the rule; and as the divisor increases in denomination, there is a decrease in these instances, and long division offers the only method of solution. The use of logarithms cannot properly be called a method of division, since it requires a series of previously computed tables, and is at best an approximation.

In the new process of "corrective division," which is herein treated, the necessity of performing at length the various progressive steps essential to

division, and of checking and proving the work by writing each successive result is obviated in performing the task by short division, with *mental corrections* at each and every step. For this reason the process is called *corrective* division.

Its discovery was due to an investigation of the properties of numbers with a view to abridging the labors of multiplication and division, and while simple in detail, it was not reached until weeks of labor had been expended in other directions.

The process of "corrective division" is a complete analysis, purely arithmetical, requiring no algebraic formulæ to demonstrate its unvarying accuracy, while to comprehend the method, and to analyze its various steps, no greater intelligence is needed than to acquire a thorough understanding of short and long division.

The new and the old methods are contrasted in the following examples:

Let it be required to divide any number as 847125 by any other number as 67. Without entering into an analysis of the well-known process of long division, it is sufficient to perform the task which appears with its progressive steps as follows:

```
67)847125(12643.656+
   67
   ---
   177
   134
   ---
    431
    402
    ---
     292
     268
     ---
      245
      201
      ---
       440
       402
       ---
        380
        335
        ---
         450
         402
```

In the above example all the indicated steps must be performed in full, and to obtain the quotient as far as reached, sixty figures are required, with eight trial divisions, eight multiplications, and eight subtractions and subsequent additions. It is true that adepts and accountants may multiply and subtract at the same time, thus saving the labor of writing every subtrahend, but the economy of time is inconsiderable. The above process is almost universally employed in its entirety.

Now let it be required to solve the same problem by corrective division. When completed it will appear as follows:

```
 7 
6.7)84712.5
 .3 12643.656+
```

Analysis: If any number, as 847125 be divided by any other number as 67, the quotient will be equal to the first number divided by 10, and this result again divided by the second number divided by 10 ∴ $847125 \div 67 = 84712.5 \div 6.7$. But dividing 84712.5 by 6.7 is equivalent to dividing 84712.5 by 7, *and to each partial remainder adding the product of the last obtained digit of the quotient and the difference between* 7 *and* 6.7 *which is* .3. Proceeding upon this principle, 7 is contained in 8 once, with a remainder of 1. To this remainder add the product of the last digit obtained in the quotient 1, by the difference between 7 and 6.7 or .3, $=1.3$. Multiplying 1.3 by 10, $=13$ and adding the second figure of the dividend 4, $=17$, the process is continued as before.

$17 \div 7 = 2$ with a remainder of 3;
$$[3+(2\times.3)]\times 10+7=43;$$
$43 \div 7 = 6$ with a remainder of 1;
$$[1+(6\times.3)]\times 10+1=29;$$
$29 \div 7 = 4$ with a remainder of 1;
$$[1+(4\times.3)]\times 10+2=24;$$
$24 \div 7 = 3$ with a remainder of 3;
$$[3+(3\times.3)]\times 10+5=44;$$
$44 \div 7 = 6$ with a remainder of 2;
$$[2+(6\times.3)]\times 10+0=38.$$

Here since a cipher was added to the last figure of the dividend, the decimal point must be written were the dividend without decimals, but as the decimal point occurs before the last figure 5, the decimal point must be written in the quotient at a corresponding number of places, ∴ the last obtained figure of the quotient 6, is a decimal. Continuing, $38 \div 7 = 5$ with a remainder of 3; $[3+(5\times.3)]\times 10+0=45$; $45 \div 7 = 6$ with a remainder of 3;

$[3+(6\times .3)]\times 10+0=48$, etc. Thus the process may be continued to an indefinite number of places.

If this analysis is not sufficiently clear it may be amplified as follows:

If 7 is contained in 8 once with a remainder of 1, then will 6.7 be contained therein once with a remainder of 1 plus once the difference of 7 and 6.7 or $.3=1.3$, or in the next lower denomination, 13, whence adding it to the units in that lower denomination, 17 is obtained as the next partial dividend, etc.

In using the corrective method of division it is simpler to prefix each prime remainder to the next figure of the dividend and add the product of the last obtained quotient digit and the differential number as before, omitting the decimal. Thus: $8\div 7=1$ with a remainder of 1; $14+3=17$; $17\div 7=2$ with a remainder of 3; $37+6=43$; $43\div 7=6$ with a remainder of 1; $11+18=29$, etc. It is not essential, though advisable, to write either the 7 or the 3, as they may be carried mentally, nor is it necessary to employ decimals except in the dividend and quotient, as in ordinary division.

Thus the labor of division is abridged in the corrective method, by employing but one fourth the number of figures used in long division, and by rapid mental calculation in place of the full written results. Having observed the corrective process as applied when the divisor contains but two digits, the following rules may be deduced:

I. *Above the divisor write the digit next higher than the first digit of the divisor, and point off the right-hand figure of the dividend.*

II. *Below the divisor write the difference between the second digit of the divisor and* 10.

III. *Proceed as in short division, adding to each partial dividend the product of the last obtained digit of the quotient and the digit below the divisor.*

A General Method of Corrective Division.

While the foregoing rules are applicable to any problem where the divisor is below 100, it is necessary to obtain general rules applicable where the divisor contains any number of digits. Before obtaining these rules, a system of terminology will be adopted to simplify the analysis. Thus if any number, as 8432, be a divisor, it is subtracted from the next highest integral multiple of the same denomination, which is 9000, leaving a remainder of 568. For convenience, 9 is called the *base*, since it is the number on which an hypothetical division is based, the number 568, the *whole difference*, and the numbers 5, 6 and 8 the *first*, *second* and *third difference* respectively. The same terms are employed in every case, the foregoing being taken as an example, and the differences are enumerated from left to right.

Let it now be required to divide any number, as 9876465309, by any other number over 100, as 88789:

$$\begin{array}{r|l} 90000 & \\ 88789 & 987646.5309 \\ 1211 & \overline{111235.23532+} \end{array}$$

In this example 9 is the *base*, 1211 the *whole difference*, 1 the *first difference*, 2 the *second difference*, 1 the *third difference* and 1 the *fourth difference.*

Proceeding as in the previous case, four places are pointed off equaling in number the four ciphers following the base.

Analysis: If 9 is contained in 9 once, with a remainder of 0, 8.8789 will be contained therein once, with a remainder of 0 + once the whole difference taken as a decimal, *but it is only the product of* 1 *and the first difference which will affect the next partial dividend*, hence, as in the first problem, $08+(1\times 1)=9$ which is the second partial dividend; $9\div 9=1$ with a remainder of 0. To obtain the third partial dividend, it is necessary not only to add the product of the last obtained quotient digit and the first difference, *but also the product of the previous or last but one quotient digit obtained and the second difference*, since in the multiplication of the whole difference by the first digit of the quotient, the product of this digit and the second difference would have affected this denomination, and consequently the third partial dividend; hence to obtain this partial dividend, 7 is added to $(1\times 1)+(1\times 2)$, $=10$; $10\div 9=1$ with a remainder of 1. To obtain the fourth partial dividend it is necessary not only to add the products of the last obtained quotient digit and the first difference, and the last but one quotient digit and the second difference, *but also the product of the first quotient*

digit or the last but two obtained, and the third difference, since *two* multiplications of the whole difference have previously taken place which will affect the next partial dividend, it being multiplied successively by the first two digits of the quotient, and in thus multiplying, the product of the first quotient digit, or the last but two obtained, by the third difference will affect the fourth digit of the whole dividend, as will also the product of the last but one and the second difference and the product of the last and the third difference; hence the entire sum to be added, or the *correction* as it will be termed, is $(1\times1)+(1\times2)+(1\times1)=4$. Adding this to the remainder 1 prefixed to the fourth figure of the dividend, or 16, which will be called the prime partial dividend, 20 is obtained as the fourth complete partial dividend, $20\div9=2$ with a remainder of 2. To obtain the fifth partial dividend, the prime partial dividend, 24, is added to the products of the *last obtained quotient digit and the first difference, the last but one obtained, and the second difference, the last but two obtained* and the *third difference, and also of the last but three obtained, or the first quotient digit and the fourth or last difference,* since there have been three previous multiplications affecting the fifth digit of the whole dividend and therefore the fifth partial dividend. Hence the fifth partial dividend $=24+(2\times1)+(1\times2)+(1\times1)+(1\times1)=30$; $30\div9=3$ with a remainder of 3. To obtain the sixth partial dividend the sixth prime partial dividend 36 is added to the products of *the last obtained quotient digit and the first difference, the last but one obtained and the second difference, the last but two obtained and the third difference, and the last but three obtained and the fourth difference. The last but four obtained, or the first quotient digit* no longer affects the partial dividends, since its multiplication by the whole difference only affected the *second, third, fourth and fifth digits of the whole dividend, and hence only the second, third, fourth and fifth partial dividends.* Therefore the sixth partial dividend will be $36+(3\times1)+(2\times2)+(1\times1)+(1\times1)=45$; $45\div9=5$, with a remainder of 0; here is inserted the decimal point, and the division may be carried out into decimals in the same manner, remembering that it is only the last four quotient digits obtained that affect the partial remainders. Thus $05+(5\times1)+(3\times2)+(2\times1)+(1\times1)=19=$ seventh partial dividend. $19\div9=2$ with a remainder of 1; continuing and writing the products without the parentheses,

$13+2+10+3+2=30=$ eighth par. div.
$30\div9=3$ with a remainder of 3;
$30+3+4+5+3=45=$ ninth " "
$45\div9=5$ with a remainder of 0;
$09+5+6+2+5=27=$ tenth " "
$27\div9=3$ with a remainder of 0;
$00+3+10+3+2=18=$ eleventh " "
$18\div9=2$ with a remainder of 0;
$00+2+6+5+3=16=$ twelfth " "
etc.

Hence the process may be carried to an indefinite number of decimal places, and the following rules may be obtained for corrective divsion as applied with any divisor:

I. *Write the divisor and dividend as in short division, and point off from the right as many figures minus one, in the dividend, as there are figures in the divisor.*

II. *Above the divisor place the next highest multiple of the same denomination, and subtract the divisor from it. The factor of the multiple will be the* BASE, *and the remainder the* WHOLE DIFFERENCE.

III. *Divide as in short division, by the base, and to each prime partial dividend add the products of the differences with the last obtained digits of the quotient in inverse order.*

It is unnecessary to state that the ordinary student will find no difficulty in performing the various computations *mentally*, and that no figures need be employed save those of the divisor, dividend, whole difference and the quotient, and the base.

Secondary Corrections.

The above general rules are applicable to any problem in division, but occasions occur when the numbers obtained in the quotient would by this process be in excess of 9, or consist of two digits, which would add to the difficulty of computation. For this reason a system of secondary correction may be employed which will obviate this objection to the method of corrective division. The fol-

lowing example will illustrate this system:

Let it be required to divide any number, as 43571206 by any other number, as 7648:

$$\begin{array}{r|l} 8000 & \\ 7648 & 43571.206 \\ \hline 352 & 56\cancel{8}\cancel{6}.\cancel{9}\cancel{6}1\cancel{8}\cancel{0}3+ \\ & 97.07\ 91 \end{array}$$

Following rules I and II as deduced from the preceding problem, the base is found to be 8, and the whole difference 352. Proceeding by rule III, $43 \div 8 = 5$, with a remainder of 3; $35 + 15 = 50$; $50 \div 8 = 6$ with a remainder of 2; $27 + 18 + 25 = 70$; $70 \div 8 = 8$ with a remainder of 6; $61 + 24 + 30 + 10 = 125$. Now were 125 divided by 8, the quotient would be 15, with a remainder of 5, and it would be necessary to consider 15 as a single quotient number in the computation. To obviate the difficulty which the introduction of a number of two digits would involve, it is *presumed* that the previous quotient digit had been computed as *one in excess of what it was written*, or in this case, 9 instead of 8, and the corrected number is written below. Now if 9 had been originally written as the previous quotient digit, or *if the previous quotient digit had been increased by* 1, it is evident that the partial dividend ensuing would have been *decreased by* 80, while to it would have been *added once the first difference;* hence, the *corrected partial dividend* would be the *uncorrected partial dividend increased by the first difference and diminished by ten times the base* or $125 + 3 - 80 = 48$. Presuming that this had been done the work is continued as before; $48 \div 8 = 6$, with a remainder of 0; $02 + 18 + 45 + 12 = 77$; $77 \div 8 = 9$, with a remainder of 5; $50 + 27 + 30 + 18 = 125$. Here it is again seen that the partial dividend contains 8, more than 9 times, and that again would a quotient of more than one digit result, and proceeding as before, 1 is added to the previous quotient digit. But the previous digit was 9, hence it would become 10 by this addition, resulting again in a factor of two digits. To obviate this second difficulty it is presumed that the *next but one quotient digit had been computed as one in excess of what it was written*, or 7 instead of 6. Had this been done originally, there would have been a *decrease of ten times the base* in the ensuing partial dividend, and an *increase of once the first difference* or a *total decrease of* 77, whence the next ensuing partial dividend would have been 0; and the quotient digit 0, whence the second ensuing partial dividend would have been $00 + 0 + 35 + 18 = 53$. But this same corrected partial dividend may be reached by direct means. Thus, as before, the previous quotient figure, 9 is increased by one, which will render it 0, and that next previous, 7. The uncorrected partial dividend, 125, is decreased as before by ten times the base or 80, leaving a remainder of 45, and increased by once the first difference or 3, and also once the second difference, since the last but one quotient digit obtained is increased by one, and hence affects the second ensuing partial dividend; hence the corrected partial dividend is $45 + 3 + 5 = 53$. Proceeding as before, after correcting the quotient, $53 \div 8 = 6$ with a remainder of 5; $56 + 18 + 0 + 14 = 88$. Adding one to the previous quotient figure, making it 7, and subtracting from 88, $80 - 3 = 77$, the next corrected partial dividend is found to be 11; $11 \div 8 = 1$, with a remainder of 3; $30 + 3 + 35 + 0 = 68$; $68 \div 8 = 8$, with a remainder of 4; $40 + 24 + 5 + 14 = 83$; making the correction as before stated, $83 + 3 - 80 = 6$, with the previous quotient figure 9; $6 \div 8 = 0$, with a remainder of 6; $60 + 0 + 45 + 2 = 107$. Proceeding as before in correcting the quotient, the last obtained quotient digit becomes 1; taking the uncorrected partial dividend, 107, from it is subtracted $80 - 3 = 77$, $= 30$; $30 \div 8 = 3$, with a remainder of 6, and the process may be continued indefinitely.

From the foregoing may be deduced the following rules:

I. *If a partial dividend in corrective division contains the base more than* 9 *times, add one to the previous quotient digit, and subtract ten times the base minus the first difference from the partial dividend, thus obtaining a corrected partial dividend with which proceed as before.*

II. *If at the same time the last obtained quotient digit be a* 9, *add both the first and second differences to the partial dividend minus ten times the base; if the last two quotient*

figures be both 9's, add the first, second and third differences; if the last three be 9's add the first, second, third and fourth differences, and, in general, as many differences in order as there are 9's plus one in the previous adjacent quotient digits, until the differences are exhausted or until the last difference be added.

CORRECTIONS BY INSPECTION OR ALLOWANCE.

The labor of secondary correction may be greatly abridged, by *allowing for operations to be performed*, after inspecting the factor. Thus in the previous example, the student may proceed as follows:

$$\begin{array}{r|l} 8000 & \\ \underline{7648} & 43571.206 \\ 352 & \overline{\quad 5697.071913+} \end{array}$$

$43 \div 8 = 5$ with rem. of 3;
$35 + 15 = 50 =$ second par. div.
$50 \div 8 = 6$ with rem. of 2;
$27 + 18 + 25 = 70 =$ third " "
$70 \div 8 = 9$ with rem. of -2;
$-19 + 27 + 30 + 10 = 48 =$ fourth " "
or to amplify the process,
$1 + 27 + 30 + 10 - 20 = 48 =$ " " "

In this case *allowance* is made for a large partial dividend ensuing, and from this dividend 20 *is subtracted* to add 2 to the previous partial dividend making the latter 72, a multiple of 8. By this means, secondary correction is almost wholly avoided. In general the operator or student may at a glance determine the sum that may safely be allowed as forming the minimum of the succeeding partial dividend, with a view to increasing the present partial dividend to a multiple of the base, and should this sum be overestimated, the necessary re-correction may be made by the *reverse* of the foregoing rules for secondary correction.

TO OBTAIN AN INTEGRAL REMAINDER.

The method of corrective division is especially adapted to problems where a large number of decimal places are required in the quotient, but there are many occasions where the quotient is desired in the form of an integer with a proper fraction, or the remainder is desired as an integer. This result may be obtained as follows:

Let it be required to divide any number as 843217 by any other number as 8679, and to obtain an integral remainder or to express the quotient as an integer with a with a proper fraction.

$$\begin{array}{r|l} 9000 & \\ \underline{8679} & 843.217 \\ 321 & \overline{\quad 97.\frac{1354}{8679}} \end{array}$$

Proceeding as before, $84 \div 9 = 9$ with a remainder of 3; $33 + 27 = 60 =$ second partial dividend; $60 \div 9 = 7$ with remainder of -3; $2 + 21 + 18 - 30 = 11$; should the process be continued as before, the quotient would contain decimals which it is desired to avoid, hence recourse is had to the following method:

$11 \div 9 = 0$, with a remainder of 11;
$111 + 0 + 14 + 9 = 134$; $134 \div 9 = 0$, with a remainder of 134;
$1347 + 0 + 0 + 7 = 1354$; $1354 \div 9 = 0$, with a remainder of 1354;
$13540 + 0 + 0 + 0 = 13540$; $13540 \div 9 = 0$, with a remainder of 13540.

This last step was unnecessary and was inserted merely to amplify the analysis. It will be seen that with the previous step the differences disappear as factors, hence that 1354 will be the integral remainder. This remainder may be obtained directly as follows:

RULE.—*After obtaining the integral quotient affix to the prime remainder, with its proper sign, as many ciphers as there are decimal places in the whole dividend; and add thereto, the decimal of the dividend, the product of the last obtained quotient digit and the first difference prefixed to the same number of ciphers less one, the product of the last but one quotient digit and the second difference with the same number of ciphers less one, etc., the product of the last obtained quotient digit and the second difference, with two less ciphers affixed, the product of the last but one quotient digit and the third difference with two less ciphers affixed, etc., until the differences disappear as factors, when the total sum is the integral remainder.* In the above example the following are the additions:

$-3000=$ prime remainder with three ciphers affixed.

$+\ 217=$ decimal of the dividend.

$+2100=$ product of last quotient digit and first difference, with two ciphers affixed.

$+1800=$ product of last but one quotient digit and second difference, with two ciphers affixed.

$+\ 140=$ product of last quotient digit and second difference, with one cipher affixed.

$+\ 90=$ product of last but one quotient figure and third difference, with one cipher affixed.

$+\ 7=$ product of last quotient figure, with third difference.

$1354=$ integral remainder.

It may be observed in this connection that the process of corrective division applied when the dividend is a multiple of the divisor, or when the divisor is contained in the dividend an exact number of times, will result in one of two quotients. If an integral remainder be sought, the quotient will be the correct quotient less one, with an integral remainder equal to the divisor, when the quotient is corrected by adding to the units and the remainder becomes 0. If the quotient be sought in decimals, there will result a repitend of. 99999 +, which is equivalent to 1, and which may, as in the preceding case, be added to the units, or the correction may be made at any point according to previously obtained rules.

Suggestions.

Though the foregoing rules will enable the student to solve any problem in division by the corrective method, instances occur where the labor of computation may be abridged. Thus when the first figure of the divisor is 1, 2, 3, or, in general, less than 5. the base will be less than 2, 3, 4, or in general, less than 6, and there is some difficulty experienced in employing the method of correcting by inspection or allowance, as previously explained. For this reason it is often convenient to employ the following rule, bearing in mind that it is not essential to the accuracy of the result.

If the divisor be such a number that it may be multiplied by 1, 2, 3, 4, 5, 6, 7, 8 *or* 9, *without causing its first figure to exceed* 9, *so multiply it, and proceeding with this product as a divisor obtain a quotient by corrective division, and multiply this quotient by the same number by which the divisor was multiplied, to obtain the correct quotient.*

By this means a higher base is obtained and the work simplified. If the first figure of an original or corrected divisor be 9, the base will be 10 and not 1, as might on first inspection be supposed.

It will also be found convenient to *check* the work by writing each prime remainder at the upper right-hand corner of each figure of the dividend; thus if confusion arises, or an error is made in computation, the work may be taken up at any previous point. This method, or rather *check system*, is not essential to accuracy, but is, like the former, one of convenience. It is illustrated thus:

$$\begin{array}{r|l} 900 & \\ 843 & 4\,6^{1}\,1^{0}\,0^{1}.\,7^{-6}\,3^{-6-43-306} \\ \hline 57 & 5\,4\,6\,.\,9\ 430604+ \end{array}$$

Explanation: $46 \div 9 = 5$ with a remainder of 1, write 1 at the upper right-hand corner of the second figure of the dividend; $11+25=36$; $36 \div 9=4$ with a remainder of 0; write 0 as before above the third figure of the dividend, and proceed giving remainders the minus sign, if they be minus, and thus insure a check upon the work.

Corrective Division by Subtraction.

The same process of corrective division may be employed by correcting the prime partial dividends by *subtractions*, when the foregoing rules may be *inversely applied.* Thus, let it be required to divide any number as 472164 by any other number as 913. Write the divisor and dividend as in short division:

$$913)\underline{4721.64}$$
$$517.156+$$

In this case 9 may by analogy be termed the *base*, and 13 the *whole difference*, and the division may be performed as before, *subtracting* the products of the differences and the quotient digits instead of *adding* them, thus:

$47 \div 9 = 5$ with a rem. of 2; $22 - 5 = 17$;
$17 \div 9 = 1$ " " " 8; $81 - 1 - 15 = 65$;
$65 \div 9 = 7$ " " " 2; $26 - 7 - 3 = 16$;
$16 \div 9 = 1$ " " " 7; $74 - 1 - 21 = 52$;
$52 \div 9 = 5$ " " " 7; $70 - 5 - 3 = 62$;
$62 \div 9 = 6$ " " " 8; $80 - 6 - 15 = 59$,
etc.

It is here seen that the same analysis hitherto employed will demonstrate the correctness of this method, and a casual inspection would lead the observer to conclude that the method by subtraction was by far the most preferable, since the divisor itself contained both the base and the whole difference; but repeated trial and experiment have demonstrated beyond question that for general use, the method by addition is the superior. In isolated cases the method by subtraction may be advantageously employed; but these instances lessen as the divisor increases. In general it may be adopted as a safe rule, that *the method by subtraction should only be employed when the digits following the first digit of the divisor are all below* 4.

The analyses of the two methods are identical, and the general rules similar, transposing the words "add" and "subtract." It would have been simpler to have analyzed the method by subtraction, but as the method by addition is the one for *general use*, the latter was chosen as the typical process of corrective division. An elaborate analysis of the method of subtraction is unnecessary, as its comprehension is assured to those who thoroughly understand the method by addition.

Corrective Multiplication.

As was first stated the method of corrective division was discovered in seeking an abridgement of the ordinary methods of multiplication and division. In reality the abridgement of multiplication was first discovered; but as the writer subsequently ascertained that this process had been previously employed, it is only included herein as an addenda to corrective division. Corrective multiplication is closely analogous, and in fact identical in analysis with corrective division. It may be briefly explained as follows:

Let it be required to multiply any number as 742137261 by any other number as 4325. Writing these numbers as in the ordinary process of multiplication, the product may be directly obtained by the following analysis:

$$\begin{array}{r} 742137261 \\ 4325 \\ \hline 3209743653825 \end{array}$$

5×1, or the last digit of the multiplier $\times$ the last digit of the multiplicand, $=5$ $=$ last digit of the product. $5 \times 6 =$ second digit of the partial product to which must be added 2×1 or the second digit to the left of the multiplier $\times$ by the last digit of the multiplicand $= 30 + 2 = 32$. Carrying 3, it is added to $(5 \times 2) + (2 \times 6) + (3 \times 1), = 28$; $\therefore$ $8 =$ third digit to the left, of the product; carrying 2, $2 + (5 \times 7) + (2 \times 2) + (6 \times 3) + (1 \times 4) = 63$; $\therefore$ $3 =$ fourth digit to the left, of the product. Carrying 6, $6 + (5 \times 3) + (2 \times 7) + (3 \times 2) + (4 \times 6) = 65$; $\therefore$ $5 =$ fifth digit to the left, of the product, and the process is continued until the final partial product of 4 and 7, or the product of the first digits of the multiplier and multiplicand, + the number carried from the preceding step will give the first two digits of the entire product.

The analysis employed in corrective division is equally applicable in this method of multiplication. In the above example, 4 may be taken as the base and 325 as the whole difference, or 5 as the base and 432 as the whole difference, bearing in mind their relative connection.

It is also evident that the multiplication may be commenced by multiplying the first digit of the multiplicand by the first digit of the multiplier, but in this event a necessity would arise for constant correction.

The corrective method of multiplication may be profitably employed, though the saving of time and labor is by no means so marked as in the process of corrective division. It is given place herein to show the constant relation of the two general processes of multiplication and division, and to demonstrate how an analysis of one may invariably be applied to analyze the other.

While a still further investigation of the process of corrective division will result in the discovery of new methods for special instances, and the formulating of new rules for the student's guidance, it has been sufficiently analyzed and ex-

plained to enable anyone of ordinary intelligence to employ it in every case where long division has hitherto offered the only means of solution. In the foregoing examples its operation is amply explained and analyzed; and, in conclusion, the writer claims for the process which he has entitled "Corrective Division:"

1st. Economy of labor. The use of figures is greatly abridged, and the computations are of the simplest nature, being readily performed mentally.

2d. Economy of time. After a little practice—no more than is required in long division—any person of ordinary intelligence may accomplish the desired results in from one-fourth to one-sixth the time requisite in long division. Repeated trials by both experts and non-experts have clearly demonstrated that the corrective method is the quickest for both classes, the economy of time being proportionately equal in nearly all cases. By corrective division the result may be often obtained in almost as little time as it would were the problem one classed under short division, and when the quotient is desired to be carried out to a number of decimal places, the economy of time is still more marked.

3d. Originality. To the best of the writer's knowledge and belief the process of corrective division is herein described, analyzed and explained for the first time.

ELECTRICITY AS A MOTIVE POWER.

By PROF. GEORGE FORBES.

From the "Journal of the Society of Arts."

The subject of the transmission of power by electricity has been so often discussed in this room, that I have had some difficulty in selecting the point of view from which I should look at the subject to-night. I presume that most of my audience have seen the action of a current which was generated by one dynamo acting upon another dynamo so as to cause it to rotate. I presume that they have also seen such a motor driving a circular saw, or a fan, or a sewing-machine. They may also have heard of the grand possibilities among which the imagination delights to revel, on the realization of which we shall look back on the old life without electricity as being but one step removed from barbarism. I propose to myself something more humble, but, perhaps for all that, quite as useful. That is, to discuss the results which have been already attained, and to show what steps are immediately possible with the knowledge and experience now at our disposal. I will not weary you with descriptions of machines, nor refer to their relative merits. In quoting the performance of any special machine, it will not be so much on account of its merit, as because it may have come more especially to my notice in connection with the particular subject which I may be talking of.

Of course, from the moment that Oersted discovered the action of a current on a magnet, it was known that energy could be transmitted to great distances by a wire. The energy which is put in at one end of a telegraph cable is partially reproduced thousands of miles away, in the form of mechanical movement. But what we have to consider is, the condition of using a current to supply motive power to drive machinery. In what cases is such an application practicable, in what cases is it economical, in what cases convenient? I wish to give you, this evening, an answer to these questions, and to explain what has been done and is being done in this field; how far we can build on past experience, and how far we are justified in foreseeing steps in the future. When new discoveries are made, we are distrustful of them; but we are too often, also, apt to be over-sanguine; we dive, in our imaginations, into the future, and conceive that that immediately will be accomplished which experience ought to warn us can only be the result of many decades of labor.

Let me give you an example. We are

constantly hearing at present of a certain method by means of which water-power, it is said, will be extensively used. This is to use it in charging accumulators with electricity, and then to carry the accumulators on a tramway or railway, and use them to drive the wagons. At first sight this seems very feasible, but reflection makes us pause. The same water-power has been always available: it might have been used to compress air; compressed air tram-cars have been constructed, and, in my opinion, are a perfect success. This method of application of the water-power is much simpler than by the electrical accumulators. But it has not yet been tried, and hence we cannot hope that this method of using electricity is likely to be very soon introduced.

From the time that it was known that a current of electricity, in going through a wire round a rod of iron, converts the iron temporarily into a magnet, inventors have been at work trying to utilize this knowledge in the construction of motors. The chief difficulty lay in the very short distance through which the attraction of a magnet remains powerful. There was also, in the motors of old date, a difficulty of getting over the dead points. The length of pull of a magnet may be considerably increased by various devices. A somewhat similar action may be effected by utilizing the suction of the current through a coil of wire upon a rod of iron. A coil of wire with a current through it acts as a magnet. One end of it attracts the N end of a magnet. But the coil also magnetizes a rod of soft iron placed in this position, and, having magnetized it, it attracts it.

This principle has been utilized directly to obtain power from electricity. The most interesting application of it which I have seen, was at the Electrical Exhibition at Munich last year. That exhibition was especially of interest owing to the various applications of electricity as a motive power, and I shall have occasion to refer to it more than once. The machine to which I now refer is designed by M. Deprez, and is called an electrical hammer. It is intended to perform the same functions as a steam-hammer. It consists of a vertical tube containing a short rod of iron, and surrounded with a coil of wire. The wire at each turn is led away to a contact piece on a plate with a movable switch. By this means, the current passes only through fifteen turns of the wire. But there are one hundred tons of wire altogether, and by moving the switch you can pass the current through any fifteen you please. You suck the iron into one section, and then move the switch, and so raise the center of attraction. In this way you suck the iron up to the top of the tube, and then you can equally well drive it down, with the force of suction added to the weight of the iron. The weight of the iron was 80 lbs., the height of the tube one meter. I can illustrate the principle with this model. He used a current of about 36 amperes; 31 amperes was found sufficient, when passing fifteen times round, to support the weight of the iron. The length of the iron rod was about six times its diameter. This is equivalent to saying that, in such a coil, a current of one ampere must circulate (15×30^2), or 13,500 times, to support the iron rod in the centre. I tried independently with this small coil, which has three layers of forty-three turns each (129 in all), and I found that five amperes were required to support the iron in the center. From this result one ampere would have to make 16,200 turns in place of 13,500, as Deprez found. I give these data because they help to fix our ideas, and numerical measurements in magnetism are far more scarce than in electricity. Dr. Werner Siemens has used the same principle in the construction of a motor to be driven by electricity, though I need hardly say his invention was not derived from the one just mentioned. It will readily occur to you that the electrical hammer of M. Deprez might, by a simple modification, be converted into a reciprocating motor, like the piston of a steam engine, and so it might be used to drive a fly-wheel and machinery. But in this mode of applying it, the whole of the momentum of the iron mass would be lost at the end of each stroke. The same is true of the piston of a steam engine. But in the rotary engine it is not the case. Now although the rotary engine is not so economical as other steam engines, its principal serves to lead us from the electrical hammer of M. Deprez, to the beautifully ingenious electric motor of Dr. Werner Siemens. If we suppose the tube with the coil of wire round

it to be bent round into a circle, the iron cylinder being also bent into an arc of a circle, then, by changing the points of contact for the current to pass through the coils, we could suck the iron core round and round inside the circular tube. This is the principle employed by Dr. Werner Siemens in the elegant motor which he has lately produced. The inner iron core is made in the form of a complete ring, but only a certain arc is of iron, the rest is of brass. It rests on wheels, spaces being left in winding the coils for these supports to reach the central ring. In the actual machine, the points of contact for the current to enter and leave the coils are changed automatically by the rotating ring. In such a form of motor, the magnetism of the iron is never changed or destroyed, and this diminishes largely the waste of energy, by heating during magnetizing and demagnetizing, which is so serious an objection to most motors—I think I might safely say to all other motors.

If this form of motor were used to drive machinery, it is easy to see that, instead of making contact automatically, it would be quite easy to do so by hand, simply turning a handle round; and by doing so more or less rapidly, the speed of the machinery might be regulated by a boy with most perfect facility. It could be quickened, slowed, stopped, or reversed, by the simple process of turning the handle. I consider that there will be a great demand in the future for such a form of motor. I will give one example. I am sure many of my audience will sympathize with me when I speak of one of the horrors of a sea voyage, I refer to the terrors of the steam-winch. What traveller has not spent sleepless nights under the constant influence of the whirring and jarring of that dreadful engine when stowing or discharging cargo. For many years I have felt convinced that this was a field for the application of electricity as a motive power. But for a long time no scheme suggested itself to me which was perfectly satisfactory. In fact, it was not until I saw Dr. Siemens' motor that I felt satisfied that the thing could be accomplished. I have no hesitation in saying that such a motor, with the contacts made by hand in the manner I have described, will be a very perfect machine, and very much more easily managed than the present steam-winch, with its troublesome combination of valves and brakes of multiplying and reversing gear. When such machines are introduced, life on board ship will be a different thing.

Now, I am not going to spend time to night in describing all the different motors which have been invented. Most motors now in use are simply generators of electricity used in a different way. Many years ago, after a prolonged absence from England in the Pacific, I was writing to the late Professor Clerk Maxwell, and asked him what had been going on in science during my absence. His answer was, "the greatest discovery of late years is that a Gramme machine can be reversed." He saw that this must be a more perfect type of electro-motor than any previous one, and that now there was a real future for employing electricity as a motive power. The Gramme machine was a cheap generator of electricity, and was an efficient motor. The problem was solved, and only required development. Of course, Professor Clerk Maxwell's remarks would have applied to any other of the modern generators. I am not going to trouble you with a description of these machines; their construction is known to most of you. It suffices to say that, when one machine is turned so as to give a current, it can drive another machine at a distance.

Now let me say a few words about the convenience of electrical motors. We are certainly a long way from having electricity laid on to our houses, like water, which we may use for any purpose we please. But when it is laid on, it is certain that many people will use it, because they now use water-engines, although water is very expensive, and because electric motors would be far more convenient in many ways. But in a large factory, the only methods available for distributing power is either by shafting or by steam pipes. Both of these are objectionable in many ways, and the convenience of having only to lead wires to the different places where power is required, will in time become manifest to many of our manufacturers. This remark applies more especially to donkey-engines and steam steering apparatus on board ships; and I confidently look forward to electricity being largely used for

the light work on steamers, and this before any long time. Of course, special attention will be given to see that the compasses are not affected by the powerful magnets and currents used for these purposes.

It is found that to get any good effect an electric motor must run at a high speed. In this there is a great difference between the theory of electro-motors and of steam engines. If a steam locomotive were restrained from running down an incline by the pressure of steam on its piston, the steam would be doing no work, and no power would be used up, except by a slight condensation. But if an electric locomotive were to perform the same office, it would be using up the current all the time, and yet not performing external work. When with the electrical hammer the iron cylinder is supported in the tube by the electric curaent, power is being used up, and yet no external work is done. It is the same as with muscular exertion. This introduces us to a new factor in mechanics, the cost of the statical effort. When I support a weight, I am using up muscular energy, and yet I am doing no external work.

But there is one peculiarity in electric machines of great importance. The motor is also a generator. Thence it follows that, when it is in motion, it creates an electromotive force. This tends to produce a current of electricity in the circuit, going in the opposite direction to the one which is driving the motor. The intensity of this opposing force is directly proportional to the velocity of rotation. If the generator and motor were identical, and the work done by the motor was so far reduced as to allow it to rotate nearly as fast as the generator, then the opposing electromotive force would almost be equal to that of the generator, and there would be hardly any current in the circuit. But if a brake be put upon the motor, more work is done, and the current in the circuit increases. The maximum amount of work is got from a motor in this way when the counter-electromotive force is equal to half that of the generator. The greatest efficiency (or return) is obtained when the counter-electromotive force is nearly equal to that of the generator. Some persons have argued from this that we ought to run motors with very light loads for the sake of economy. This is not the case. If the motors were run so as to do only half of the maximum, we should require twice as many motors, and usually the prime cost of the motors is too great to cause this to be economy. The efficiency of a generator for electric lighting or other purposes is highest when there is a very high resistance in the circuit, but electricians, as a matter of fact, nearly always work their dynamos up to their maximum capacity, although this is by no means the most economical, so far as the efficiency is concerned.

I have said that, to produce a good mechanical effect with a dynamo used as a motor, it is necessary to drive it at a high speed. This speed is generally too great for ordinary mechanical applications. For example, we do not require the wheels of a tram car to turn at anything like a speed of 800 turns a minute. In such applications, then, it becomes necessary to reduce the speed by suitable gearing. Thus, in the electrical tramcars which have been constructed hitherto, belting has been used, or some equivalent. This is an awkward expedient, and one which, in practice, is found to give much trouble. I am inclined to think that, in making an electrical railway, it would be preferable to make the wheels of small diameter and attached directly to the axle of the motor. This seems to be by far the best plan in the present state of affairs. It may be that a motor will be found which will work economically at a lower speed; but with those which are now at our disposal, it would be best to work with very small wheels.

We now come to consider, directly, the transmission of energy to a distance. There are two separate kinds of application. One is to fixed motors, the other to locomotives. Let us begin with the stationary motors. There is no doubt that if electricity be capable of transferring the water-power of rivers to the centers of mechanical industry, if it be capable of introducing economy by burning coal at the pit mouth, or the bottom of the pit-shaft, then we have a vast engineering industry ready to open out.

It is not until we have sat down and worked out from experimental data the cost of installation and maintenance, that

we can pronounce an opinion on the merits of such schemes. When we want to send a large quantity of energy to a distance, through a thin wire, we must use the highest electromotive force which we consider safe, and the most powerful current which the conductor will bear without either fusing or injuring its supports by heating, and then we have no means of increasing the amount of energy which we can send, except by increasing the size of our conductors. Unfortunately, when we double the weight of our conductors, we do not double the current which we can pass through them. Hence there must necessarily be a limit to the quantity of energy which can be sent along a single line, which limit is reached when the cost of the conductor becomes too great. When a limit is thus obtained, the only way of increasing the transmission of energy economically is by increasing the number of systems of conductors, or else by introducing means of cooling the conductors. These are the considerations which have guided M. Deprez in his experiments. But Sir William Thomson has shown that the loss of horse-power in heating the conductor more than compensates for the expense of thicker conductors. For economy, we ought to have the value of horse-power thus wasted equal to the value of interest and depreciation of conductors.

In such an installation we require to consider not only the quantity of power which can be transmitted, but also the efficiency of the arrangement. Now the efficiency varies with the quantity of power which is transmitted. If we transmit very little energy, we get a high efficiency, nearly $= 1$; if we transmit the maximum amount of energy possible, the efficiency is $\frac{1}{2}$. In fact, the work done by the motor is proportional to the efficiency minus the square of the efficiency, where $w=4M\ (e-e^2)$.

$M=$the maximum work possible.

$e=$the efficiency.

$w=$the actual work returned.

and consequently, when the efficiency is nearly equal to unity, the work done is zero. It is found in practice to be preferable to get nearly the maximum work out of a machine, rather than to increase the size and cost of the machines, in order to get a certain amount of work from the apparatus.

Of course, it is only after long experience that we shall know what is the maximum power which we can send along a wire, and what limits must be put to the electromotive force which we use. M. Marcel Deprez has spent much time and labor on making practical tests of this nature. One of these formed a chief attraction at the Munich Exhibition last year, where he used an iron telegraph wire 4 mm. diameter (or a sixth of an inch), and transmitted $\frac{1}{2}$-horse-power to a distance of $35\frac{1}{2}$ miles. He has since been carrying on experiments of the same kind at Paris. The results have lately been published, and I shall return to them.

One of the chief things which I proposed to myself, when I was asked to read a paper here, was to give some account of the experiments which I saw at Munich. I was very much struck indeed with the transport of energy by Mr. Schuckert, of Nürnberg, not that there was any new principle involved, but I had never before seen so much as five or six horse-power conveyed by a thin wire so far as three and a-half miles. The power was derived from a turbine, at Hirschau, beyond the English gardens. These turbines are employed on a locomotive factory, no steam being used in any of the shops. They have thus 260-horse power at their disposal. The machine at Hirschau was of a larger size than the motor in the exhibition. This is a point on which Mr. Schuckert has made careful experiments. This machine which was driven by the turbine, worked a number of agricultural machines during the day, and served to light thirteen Pilsen lamps at night, both being in the exhibition building. The turbine being a very large one, and revolving slowly, transmitted its power to the dynamo by belts. A turbine is a very convenient motor for a dynamo, for it happens that, when we are using a dynamo requiring the whole power of the turbine, the proper speed for each is nearly the same, so that the dynamo may be attached directly to the shaft of the turbine, which is, of course, a very great convenience, and a saving of power. In my experiments, in conjunction with Dr. Young, on the velocity of light, at Wemyss Bay, I made use of such an arrangement for producing the electric light. A small

Siemens machine was attached directly to a Thomson's vortex turbine with horizontal shaft. The water was led from a distance of nearly a mile. The convenience of being able to start the electric light by merely turning a handle was very great indeed, and I can recommend this arrangement to anyone who has water-power at his disposal.

But to return to the Hirschau installation. A pair of No. 7 copper wires conveyed the current, one-eighth of the E.M.F. being wasted in heating the wire. This installation, taken as a whole, was much to be admired. I do not say that there was anything new or much information to be gained from it, but it showed to everyone visiting the exhibition a good working practical installation. Mr. Schuckert has been good enough to give me the results he has obtained from two experiments with different sets of machines. All the four machines with which these experiments were made are constructed for generating a current of eight ampères. In one experiment the generator was constructed for fifty-five incandescent lamps, with a motor constructed for thirty-three lamps (the lamps being in compound parallel). In the other he used as generator a machine for about 150 lamps, with a motor made for eighty-eight lamps. The particulars of the machines are given in the following table, from which we see that in each case 36 per ecnt. of the energy put into the generator was reproduced as work done by the motor:—

	First Experiment.	Second Experiment.
Type of generator.......	TL 3	TL 5
Type of motor..........	TL 2	TL 4
Difference of potential—		
(1) At terminals of generator	300	700 volts.
(2) At terminals of motor	260	600
Resistance of leads.. ...	6 ohms.	12 ohms.
HP. spent on generator..	4.4	12.8
HP. given by motor.....	1.6	4.7
Efficiency (per cent.)....	36	36

I give you these figures, not because they show any wonderful performance, but because it is desirable to have as many facts as possible when any new idea is being developed.

You see that, in these experiments, the aim has not been so much to get a high efficiency as to get a high amount of power developed. This is what ought always to be done, while the machines form the chief item of cost.

Let us now speak of the experiments of M. Marcel Deprez. In 1881, M. Deprez published the theory of transmission of energy to a distance, by means of a thin wire. It is a theoretical fact that, if you get a certain return by using a given generator and a given receiver, and with a certain length of wire between the two, you can double the distance with the same results as to power, using the same thickness of wire if you construct a new generator and a new receiver wound with smaller wire, so as to have double the resistance of wire wound, the other dimensions of the machine being unchanged. This had been known before, and the theory had been fully worked out by Frölich and others. But Deprez had the credit of working out numerical examples, and of afterwards putting the theory to the test of experiment. In his first paper, he started with some experiments made at Chatham, in which two Gramme machines were used, one as generator, the other as receiver, when, through a line-resistance equal to that of either machine, a return of 50 per cent. was obtained. He then showed that theoretically a similar pair of machines could be constructed so as to transmit 50 per cent. of the energy to a distance of 100 kilometers, an ordinary telegraph wire being used, 4 mm. thick, giving a resistance of 9 ohms. to the kilometer. He also stated that 12-horse power being used by the generator, 6-horse power would be delivered by the receiver. I do not consider that the theory in this paper was quite clear, but the main point was proved.

After the publication of this paper, the Munich Exhibition was a thing of the immediate future, and M. Deprez was invited to make his experiment with a distance of sixty kilometers. The resistance of a telegraph wire of this length, using the earth for the return, was 475 ohms. Consequently, two machines were made of the normal type, like an "A" Gramme, each of 453 ohms resistance. These machines developed very high electro-motive forces (about 2,000 volts), and when they were installed, it was naturally discovered to be impossible to use the earth as a return. Consequently, another wire was used, increasing the resistance to 950 ohms

This militated against the complete success of the experiment, and I think it is unfortunate that, in their desire to assert the success of the experiments, the friends of M. Deprez withheld this fact from the public, and so led many people to believe that the theory was inexact. As a matter of fact, owing to the great resistance of the line, 60 per cent. of the total electric effect was used up in heating the circuit, instead of 40 per cent., as had been calculated. In order, then, to get a satisfactory return, it was necessary to allow the receiver to do very little work. Thus, in the trial made by the committee of experiments before the machine broke down, only 0.433 of a horse-power was utilized at Munich, and there was a return of 39 per cent. of the power put into the machine at the distant station of Miesbach.

These experiments were of a very high degree of interest, and M. Deprez deserves the thanks of scientific men for the zeal he showed in carrying them out. Let us see, now, whether an economy would thus be introduced by burning coal at the pit mouth, and transmitting power, as in the Miesbach experiment. Allowing 3 lbs. of coal per horse-power per hour, if we work twenty-four hours a day for 300 days in the year, we have almost exactly 10 tons of coal per horse-power per annum. Suppose the coal costs 5s. a ton at the pit mouth, and 1½d. per ton for transport over the kilometer, it follows that the cost of furnishing 0.433 of a horse-power at Munich by transport of coal would be the cost of 4.33 tons of coal, and their transport over 60 kilometers, amounting to £1 11s. 6d. The cost of coal to work the machine at Miesbach, with a duty or efficiency of 39 per cent., would be £2 19s. Let us assume the machines to cost £100 together, then, neglecting the cost of the telegraph wire, and allowing 15 per cent., for interest and depreciation of the machines, neglecting also the extra cost of the larger steam-engine required at Miesbach, we have an annual expense of £17 19s. for the power delivered electrically at Munich, against £1 11s. 6d. for the same power delivered by the transport of coal. Even if the machines cost nothing there would be a loss; if the power cost nothing there would also be a loss. It is folly to use machines costing so much only to produce ½-horse-power.

I have no doubt that M. Deprez could have got more than a ½-horse-power at Munich, but had he attempted this it would have been at the cost of efficiency. It cannot be too constantly borne in mind that, in electro-mechanics, you can increase your efficiency almost up to unity, but at a sacrifice to the total amount of work transmitted. But it must also be constantly remembered that you increase your total work up to a certain maximum, but at the cost of efficiency. The maximum efficiency is obtained when the load is so great that the motor is only just able to turn round. The efficiency is greatest when the load is so light that the motor turns nearly as quickly as the generator. I repeat that an erroneous idea has been promulgated that we ought always to run motors at a speed approaching to this condition of maximum efficiency. But if this be done, the return of work is insufficient to pay for interest and depreciation on the plant required.

The very important experiment of M. Deprez at Munich, is able to give us much information for future use. Among other facts, we learn that, with machines of the type described by him, 2,000 volts is too high an electromotive force to employ. Tue reason is that it is impossible to prevent the contact of the brushes from being sometimes accidentally broken. In such a case it was found that the electromotive force developed by the extra current is sufficient to ruin the insulation.

Another matter deserving of attention is the relative size of the generator and motor. M. Deprez employed two machines of equal size. This slightly simplifies the theory, but it is certainly not the most advantageous arrangement, and we are much in want of accurate measurements on this head.

From what has been said, it is clear that the Deprez experiment at Munich was, commercially, a failure. You might, at first sight, think that if a thoroughly scientific experiment like this was a commercial failure, there are no cases in which it would be a commercial success. This is by no means the case. Since the time of the Munich Exhibition, M. De-

prez has been occupied with experiments of a far more practical nature, with a line of 160 ohms resistance. In the latest experiments, where the effects of friction were deducted, a return of $47\frac{1}{2}$ per cent. was obtained, and 4.4 horse power of work was actually given off by the motor. Judging from the Hirschau experiment, I should say that if a fall of water be used as the motive power, we can install a turbine and dynamos which shall transmit 6 horse power through a resistance of 12 ohms, at a cost of £200, neglecting the unknown cost of the conductor. If this power were used in a place where coal costs 20s. per ton, the cost of fuel for 6-horse-power would be about £60 per annum. The interest and depreciation on the boiler and steam-engine would be about £30 per annum, making in all £90 per annum, exclusive of wages. Electrically transmitted, the interest on plant, at 15 per cent. would be £30 per annum, exclusive of wages. This difference of £60 per annum, after deducting from it the interest and depreciation of the conductor, is so enormous, that it is easy to see what a large saving would be effected in any installation where there is a large consumption of power. There are many factories where it is essential to use a high priced coal, but if the power could be conveyed electrically from a distance of a few miles, an immense saving would be effected by employing a cheaper kind of coal. When water-power is used, it often happens that we can convey the cables along the bed of the river. This preserves the insulation, and keeps the conductor cool, so preventing the usual increase of resistance by heating.

In some towns, and notably in Sheffield, the whole of the water supplying the town comes from reservoirs at a very great height. The very large quantity of energy of this water is at present absolutely wasted; and although coals are not dear in Sheffield, and manufacturers are permitted to burn the most smoky kinds of coal in the center of the town, yet the employment of this waste water-power would add very considerably to the commercial prosperity of the town, and the water would be injured in quality no more by passing through turbines than by passing through the present pipes. If this power were distributed electrically, the benefit to the community would be great indeed.

At the site of the Severn Tunnel there is a width of river of two and a-half miles, where the average rise of tide is fifty feet. If the average rate of flow across this section were one mile per hour, we could utilize 100,000 horse-power, and the market value of that power is something like £1,000,000 per annum, which is now allowed to be wasted. It is worthy of the most serious consideration whether it would not be worth while to erect the enormous engineering works which would be required to utilize this wasted energy, or rather a portion of it. Assuming the interest and depreciation on the turbines and dynamos to be at the rate of £2 per annum per horse-power (and it would be far less than this for a large installation), it is easy to see that a time will come when this source of wealth will not be allowed to be wasted.

I wish to draw special attention to this point, because I have frequently heard people say, with regard to the utilization of the tides, "Oh! that is out of the question: Sir William Thomson has proved that the area required to produce 100 horse-power is so great that it would be more worth while to reclaim the land and cultivate it." Now Sir William never said such a thing. It is true that at the York meeting of the British Association, he showed what area would require to be enclosed in order to produce 100 horse-power on the supposition that there was a rise of tide of only 6 feet 7 inches (2 meters). And he made use of the following expression:—"Thus we are led up to the interesting economical question, whether is forty acres .... or 100 horse-power more valuable." But in the case I am considering, the height of the tide is six times that contemplated by Sir William, and the question is rather "whether is forty acres or 600 horse-power more valuable." The estimate I made above, as to the value of a horse-power, agrees with Sir William's; and the value of 600 horse-power is £6,000 per annum. I confidently state that the reclaimed land on the Severn would not be worth £150 per acre per annum. I have taken the site of the Severn Tunnel as an example, but there are many other places available,

and I have little doubt that the industrial population at Bristol will soon realize the wealth of power at their doors.

Some people, in this country, are timid about the introduction of large turbines. This, I need hardly say, is a gross prejudice. At Connecticut, Massachusetts, 30,000 horse-power is derived from the river by turbines. At Narva, near St. Petersburg, one firm uses 12,000 horse-power, and the size of the turbines used, which were made at Augsberg, was limited only by the size of the tunnels on the railways by which they had to be conveyed. At Munich I saw 260 horse-power rendered available by means of turbines with a fall of less than two meters. In the remote valleys of Italy there are several installations of incandescent electric lighting by means of turbines driving the machines of Messrs. Crompton. In Scotland, too, we have powerful turbines. Mr. Pirie, the paper-maker, has introduced, near Aberdeen, turbines working up to nearly 1,000 horse-power. At Greenock large over-shot wheels are employed. The Shaws Water Worsted Company are now replacing one of these, of 200 horse-power, by a turbine. In Lancashire water-power is also extensively used. At Valserine, on the Rhone, a canal has been cut through the rock, a third of a mile in length, thus giving a fall of about 12 meters and many hundred horse-power, using Jonval turbines. It is proposed to use the falls at Bellegarde, near Geneva, in the same way. The Manville Cotton Company, at Albion, R. I., use turbines, with a fall of 5.4 meters, which takes the place of a steam-engine of 800 horse-power, and economy is materially gained, although during the five months of the dry season they are obliged to use steam. At the Pittsburg Mill, Minneapolis, there is a Victor turbine, working up to 1,400 horse-power, and another is being added of the same dimensions. These are only a few examples to show how thoroughly practical, economical and convenient the use of turbines has become. We can with them calculate on utilizing 75 per cent. of the theoretical power of the head of water. They can be controlled with the utmost ease. Their cost is very small compared with water-wheels, and they can be geared directly on to the shafts of dynamos. As an example of their adaptability to the purposes of the electrician, I may mention the performance of a Fourneyron turbine established at St. Blaise in the Black Forest, which utilizes a fall of 108 meters, turns at a speed of 2,300 revolutions a minute, and gives off 60 horse-power.

The engineering works which would be required on the Severn would be very simple, though on a large scale, and the action could be made quite continuous, independently of the state of the tide, and without any necessity of using accumulators.

Having now spoken of the applications of electricity to stationary engines, let us see what has been done, and what can be done, with respect to locomotives. There are two ways in which this application has been made. First, by storing the electricity in accumulators, and putting the accumulators in a tram-car, a boat, or a tricycle, and second, by deriving the current from a wire or other conductor placed along the line. The experiments made upon the first plan are very interesting, but their practical value is limited, owing to the weight and expense of the accumulators. I think that it would be far more economical to use compressed air engines. Ever since 1865, when I examined the compressed air tram-car of Scott Moncrieff, which ran so successfully in Glasgow, I have felt sure that this must become eventually the motive power for tram-cars, and I have not yet seen anything to lead me to alter my opinion.

What has been done in the second way of proceeding? We will see that considerable progress has been made. At the Berlin Exhibition in 1879, Messrs. Siemens and Halske drove a train of a few carriages round a circular tramway, the gauge being three-quarters of a meter. In April, 1881, the same firm constructed the Lichterfelde and Berlin electrical railway, which is two miles long. One of the carriages of this railway was shown at the Paris Exhibition of 1881. The current is passed through the rails, which are insulated. It is then taken up by the wheels, and so passed through the motor. The motor turns at a very rapid rate, but it is geared to the wheels of the carriage by belting of wire rope, which reduces the speed of revolution. The carriage has the appearance

of an ordinary tramway-car, and the machinery is invisible. It was expected that snow might prove troublesome in such a system where the current is passed through the rails, but the winter of 1881–82 was so mild as to give no trouble. I have not heard whether any difficulty presented itself during the past winter. At the Paris Electrical Exhibition a somewhat similar car was used, but Messrs. Siemens were not allowed to pass the current through the rails, on account of the danger to horses, &c.; hence they were obliged to carry conductors, consisting of split tubes, on telegraph poles, and a small carriage fitting into the tubes made electrical contact with the motor on the car. Another electric railway has now been completed by the same firm, between Charlottenburg and Spandauer Bock. In this case, also, a contact carriage is used, as at Paris. This line is three and a half kilometers long; two and a half kilometers are level, but this line is specially interesting from the fact that a whole kilometer is on the considerable incline 1 in 28. Messrs. Siemens have also applied the same principle to haulage in mines. A small electrical locomotive is used in a mine near Dresden to drive eight corves loaded with coal. Finally we have the electrical tram-car running on the Portrush line, in the North of Ireland, but this has been so well described in this room, and so recently, that I need only mention it.

It has always seemed to me that the principal advantage which would be gained by using electricity as a motive power for railway trains, lies in the great facility with which motive power could be applied to every wheel of the whole train. Captain Douglas Galton, in the concluding paragraph of his report on experiments conducted by himself and Mr. Westinghouse, on continuous brakes, says:—"The advantage which thus evidently ensues from utilizing the adhesion of every wheel of a train, suggests the further consideration as to whether it would not be a more scientific arrangement, as well as being more economical in regard to the permanent way of railways, to utilize the adhesion of every wheel of a train for causing the train to move forwards, instead of depending for the moving force upon the adhesion of one heavy vehicle alone, viz., the locomotive."

Certainly, there can be no doubt that it would save the permanent way from tear and wear, and the passengers from the shocks which are so unpleasant. But it is only by means of electricity that the proposition is practical. Moreover, the movable parts of a locomotive are not perfectly balanced, and this limits the speed at which it can go. With an electrical motor the balance is perfect. By utilizing a motor on every wheel, you could go safely round curves at far greater speed. I feel sure that in this way it would be safe for trains to go at double the speed they do at present, and I hope to live to travel from London to Edinburgh in three and a half hours, smoothly, and without jolting. Furthermore, a train, instead of taking several minutes to get up full speed, could do so in a few seconds. This follows from the same reasoning as is applied to continuous brakes. Lastly, if there were power enough, a train could go up a steep hill instead of being confined, as practically they are, to an inclination of 1 in 80. In a district with heavy goods traffic, this might be a great advantage.

These considerations appear to me to be so important, that I must pause a while to make them clear.

First.—As to getting up speed quickly. With an ordinary train, weighing ten times as much as the engine, the theoretical minimum of time required to get up a speed of thirty miles an hour is one minute, however powerful the engine may be. The reason is that the wheels would slip if the motion were accelerated more rapidly. But if power were applied to every wheel, it would have a speed of 30 miles an hour in six seconds, and with no more jerk than is now felt when a continuous brake is applied.

Second.—As to the going up hill. The same advantage here is gained as would be gained by continuous brakes. An ordinary train, when the lines are in an average condition, slips down an incline of 1 in 50, when the locomotive alone is braked. You all remember the sad accident the other day in the Rocky Mountains from this cause. But when all the wheels are braked, the train rests on an incline of 1 in 5. So also, if we had the power, we could ascend an incline of 1 in 5 by electrical motors.

Third.—As to advantage to permanent

way and speed. The engine does not give a steady pull at all times to all parts of the train. In fact, in going round a curve, part of the engine's power is used up in trying to pull part of the train off the lines. With power applied to all the wheels, this would not be so. Moreover, the enormous weight of the locomotive, imperfectly balanced, which is the chief source of mischief, would be done away with.

Fourth.—As to cost. The cost of making an ordinary railway line with tunnels, embankments, bridges, viaducts, &c., in England, may be taken at £15,000 a-mile; but if it goes up inclines, instead of having expensive engineering works, the cost will be the same as making it on a level, *i. e.*, £5,000; and hence, on a goods line, the cost of construction might be reduced to one-third. This original outlay is a far more serious matter than the working expenses of a railway (of which the cost of the traffic is 26 per cent.); so that, even if by using electricity you increased your working expenses, you might still be making a great saving on the whole. On fast passenger trains, the power to drive your trains at twice the speed would counterbalance any small loss of economy. Again, there would be a great saving in the cost of maintenance of permanent way.

There are many cases where such an application of motive-power would be peculiarly satisfactory. The most important of these is the Underground Railway in our city. It is now universally admitted that steam locomotives vitiate the air to an intolerable extent. If motive power were used electrically, this evil would be avoided. Moreover, a line like this, which is almost totally underground, and is comparatively unaffected by snow or rain, possesses exceptional advantages for electrical insulation. But the system which I have advocated would introduce special advantages to this line. What are the conditions of working? The distance between stations is so short that a great deal of time is wasted in getting up speed and stopping the trains. The latter evil, it is true, has been reduced to a minimum by the adoption of continuous brakes; but you will understand how much speed is limited by the former consideration, when I tell you that an ordinary train cannot get up a speed of thirty miles an hour until it has passed over a quarter of a-mile, just about the distance between some of the stations. If the system I propose were adopted, of applying motive power to every wheel, the same speed would be attained in forty-four yards.

The Channel Tunnel, when completed, will be specially favorable for the insulation of conductors. But the system which I recommend could not be adopted, as the great advantage of the tunnel is that the same wagons will go right through without any transference of goods. In this case, if electricity be used as the motive power, it would be necessary to use it to drive a heavy locomotive.

I think I have now said enough to prove that a great deal has already been done, and that we may reasonably expect a great deal more to be done in the use of electricity as a motive power. Water-power has been usefully employed in various installations to obtain electricity, and the current has been successfully conveyed several miles, to be reconverted into motive power with great economy, and electric railways have been successfully and economically worked at several places. I have not thought it right in this paper to indulge in Utopian dreams of what may be done in the future, however pleasant, such an exercise of the imagination might have been. I have thought it to be more useful to look at the matter from a practical point of view, and to estimate our present position with respect to this force, and to indicate the steps which our past experience would warrant us in taking in the immediate future. I should like to see a larger number of our wealthy compatriots experimentalizing in this field, so as to keep our country in the front rank of the applications of electricity, as she always has been in those of steam. But we must go on step by step—learning from every fresh experiment, and utilizing experience for further advances. We must not hope to reach perfection at one bound. The development of this most potent of agencies must, in the nature of things, be gradual. We must have patience, but at the same time we must work and labor to our utmost, and

take a step onward wherever that step is sure, and in due course we shall be ourselves astonished at the progress which has been made.

"We have not wings, we cannot soar,
But we have feet to scale and climb,
By slow degrees, by more and more,
The cloudy summits of our time.

The heights by great men reached and kept
Were not attained by sudden flight,
But they, while their companions slept,
Were toiling upward in the night."

REPORTS OF ENGINEERING SOCIETIES.

American Society of Mechanical Engineers.—The annual meeting began in Cleveland, O., June 12, with a large attendance. The members were welcomed by Mayor Farley and the president, Mr. E. D. Leavitt, Jr., made a brief address, when the Society proceeded at once to business.

Mr. J. F. Holloway, of Cleveland, then read an elaborate paper on the Marine Engines of the Lakes, embodying a history of the development of marine engineering as applied to lake steamers, and a description of a device for getting them off the dead centers.

Mr. Howell Green, of Jeanesville, Pa., then read a paper on the development of the Winding and Pumping Machinery of the Anthracite Coal Regions, which was of much interest.

This was followed by a paper on Economy in Lubrication of Machinery, by Mr. George N. Comly, of Wilmington, Del. This paper called out some discussion.

After adjournament the members were entertained by the Cleveland Civil Engineers' Club.

SECOND SESSION.

On the second day reports were presented by the Council, the Treasurer and the Tellers, and a number of new members were elected.

Mr. W. F. Durfie, of Bridgeport, Conn., then read a paper on Balancing Vertical Engines, which was followed by a lively discussion.

Mr. W. E. Ward, of Portchester, N.Y., read a long paper on Beton in Connection with Iron as a Building Material.

A paper by H. R. Towne, of Stamford, Conn., on Cranes, was then read. This was followed by an address on the same subject by Mr. Thomas R. Morgan, of Alliance, O., who promised a future paper.

THIRD SESSION.

On the third day there was a long discussion on Mr. Towne's paper on Cranes.

A paper on the Bower-Barff Process for Protecting Metals was read by George W. Maynard, of New York, and called out a lively discussion, in which many members took part.

Papers were then read by Wm. J. Baldwin, of New York, on Standards in Pipe Fittings, and by Wm. Kent, of Pittsburgh, on Relative Values of Bituminous Coals.

Mr. C. C. Collins, of the Stearns Manufacturing Company, Erie, Pa., followed with a paper on Balanced Valves; and Prof. J. B. Webb, of Cornell University, on Reuleaux's Kinematic Models.

Before adjournament, Mr. J. F. Holloway, announced that he had received a dispatch from Prof. R. H. Thurston, of the Stevens Institute of Technology, which at its commencement this week conferred the honorary degree of Doctor of Engineering on E. D. Leavitt, Jr., President of the American Society of Mechanical Engineers. This announcement was received with much applause. After a brief reply by the doctor, the meeting adjourned.

In the evening, the members attended a reception tendered them by citizens of Cleveland.

American Society of Civil Engineers.—The fifteenth annual convention began at St. Paul, Minn., on Tuesday, June 19, with a large attendance. The convention organized by the choice of George S. Greene as temporary chairman.

Addresses of welcome were made by Governor Hubbard, of Minnesota, and Mayor O'Brien, of St. Paul. These were responded to by Chairman Greene.

A permanent organization was then made, with Mr. D. C. Sheppard, of St. Paul, as Chairman, and the convention proceeded to business.

A paper on Building the Dyke at the Falls of St. Anthony, was read by Col. Farquahar, United States Engineers. This was followed by a paper on the Cost of Steam Power, by Charles E. Emery.

At Wednesday's session the two papers read on the previous day were discussed at considerable length.

Papers by Prof. Egleston, of Columbia College, on Accidents to Steam Pipes from the use of Slag Wool; by John Lawler, on a Pontoon Bridge over the Mississippi; and by G. Lindenthal, on the Rebuilding of the Monongahela Bridge at Pittsburgh, were read and discussed.

On Friday a session was held at Minneapolis.

On Saturday an excursion was made to Stillwater.

The attendance was larger than at any previous convention, 175 members being present, accompanied by 90 ladies.

ENGINEERING NOTES.

An Ancient Bridge.—An interesting relic of antiquity was lately received at Berlin from Mayence, consisting of the remains of piles belonging to the bridge which once led from Castel to Mayence, and which is proved to have been in use fifty-three years before the Christian era. The pieces of wood are trunks of various trees, including oak, elm, and white beech and red beech. They are said to be internally sound, and to have pieces of iron at one end. It is intended to devote part of the wood to the manufacture of a piano-case.

The German papers state that the acquisition of these remains was difficult, as English collectors were offering high prices for such objects. Prince Alexander of Hesse has had

some ornamental furniture made from oak which was discovered at the spot referred to, which he has presented to his son, Prince Alexander of Bulgaria.

The Forth Bridge.—On Juue 7, Thomas Tancred, one of the contractors for the Forth Bridge, laid, with masonic honors, the first granite block of stone in connection with the structure. He was accompanied by Mr. Symons, one of the Government inspectors; Mr. Gray, manager of the works at North Queensferry, and several other gentlemen. The stone of pure Aberdeen granite, weighing about 16 cwt., was placed at the south-west corner of pier No. 12 from the south end of the bridge, which is situated a little to the east of the church at North Queensferry. There are now at the works—north and south—nearly 1000 tons of Aberdeen granite for the outer casing of the piers. Four steam stone crushers, each capable of crushing 80 to 90 tons per day, are at work preparing the whinstone for centering of the piers, and all along the line the greatest activity prevails in getting ready the preparatory operations required for this great structure.

The Inland Sea of Tunis.—At the recent lecture given by M. de Lesseps at the Sorbonne on the project of Captain Condarre for flooding the Shotts and creating an inland sea on the borders of Tunis and Algeria, the lecturer expressed his entire trust in the success of the operations from an engineering and sanitary point of view. The survey recently made by M. de Lesseps has satisfied him on these points. All the projectors require to begin with the work is the concession of useless lands which will form the shores of the lake. The evaporating power of the sun is less there than in the Red Sea, and M. de Lesseps does not anticipate that the waters will dry up. The bed of the cutting will be of sand to a very great depth, one boring of 73 meters at Tozeur still showing this kind of bottom. Politically speaking M. de Lesseps anticipates good results from the execution of the scheme, as the sea would form a frontier for Tunis and Algeria. A report of the recent survey has been communicated by the engineers of M. de Lesseps and M. Roudaire to the French Aaademy of Sciences. The Oued Melah mouth, where the flooding canal will leave the Mediterranean, is so well covered at high tide as to form a natural port, especially when provided with jetties. The canal will be straight and its navigation easy; the anchorage in the sea will be of mud and sand free from rocks. The mean depth will be about 20 meters, or over ten fathoms. The soil of the north bank of the inland sea and along the canal from Galies to Biskra is similar to that of the most fertile parts of Algeria and Tunis, and only requires water to make it highly productive. The amelioration of the climate of the sea and the utilization of subterranean waters and wells which exist must, in the opinion of the engineers, be of great benefit to the surrounding country. The calcareous rocks at the entrance of the canal near Galies are considered an advantage rather than otherwise, as they will furnish building materials for the jetties and other constructions of the port, and sluices for filling the Shotts with water. The new route between the Shotts of Djerid and Rharsa avoids the rocks of Kriz, which are of considerable extent, and the altitude of its watershead is 12 meters less than that at Kriz. The canal proposed is 25 to 30 meters wide, but it is not expected that the current will widen it. The time required for excavating it is estimated at five years, and its cost is reckoned at 150 million francs. The report is signed by seven surveyors, among whom are M. A. Couvreux, contractor for public works, M. Emile Dollot, engineer of arts and manufactures, M. Leon Dru, engineer, and M. G. de Kersabiec, a naval lieutenant.

IRON AND STEEL NOTES.

On the Effect of Sulphur and Copper upon Steel.—Opinion is considerably divided as to the proportion of copper and sulphur that may be present in steel without injuring its working properties. Karsten, reproducing the opinion of the practical ironworkers of his time, states generally that copper makes iron red-short. Eggertz states that wrought iron with 0.5 per cent. of copper shows only traces of red-shortness. Stengel draws the following conclusions from the results of a series of experiments:—

1. Sulphur to the extent of 0.116 per cent., and 0.192 per cent. of silicon, without copper, renders iron and steel red-short and useless.

2. Red-shortness becomes apparent with 0.015 of sulphur, and 0.44 of copper per cent.

3. The deteriorating effect of sulphur is much more energetic than that of copper, 0.1 per cent. of the former being probably more injurious to the strength of iron than 0.75 per cent. of the latter.

According to Eggertz, steel made from an iron containing only 0.5 per cent. of copper is worthless.

In America, greater importance is attached to the absence of sulphur and copper in steel than in Germany, 0.15 to 2 per cent. of copper being considered as too high.

In order to obtain more definite information as to the influence of these elements, experiments were made by the author in 1875 at Bochum, by adding them both separately and together to the metal, in a converter containing 3-ton charges. Copper was added in the metallic form, and sulphur as sulphide of iron. The addition was always made before the charge was introduced, in order that the effect might be uniformly distributed through the mass. The ingots were rolled into rails, receiving the same reheating as those ordinarily made. A complete analysis was made in every case, in order to determine whether the red-shortness might not be due to other substances.

From the experiments the author concludes that the effect of copper in producing red-shortness has been over-estimated, as steel containing 0.862 per cent. was perfectly workable, and even in combination with sulphur it is not so very injurious if the latter is kept down below the limits at which it will produce red-

shortness alone. As an extreme limit to be tolerated, the author considers 0.15 to 0.16 per cent. of sulphur as likely to cause red-shortness, while 0.10 per cent. may be regarded as harmless. It may also be that, with softer and less manganiferous metal than No. 4, the injurious effect may be more marked, and in any case the less snlphur that is admitted the better.—*Abstracts of Inst. of Civil Engineers.*

Basic Bessemer Steel.—We understood that plates made in the Bessemer converter by the Thomas Gilchrist process at Witkowitz, in Moravia, were recently sent to the Austrian Llyod's Registry, at the request of that authority, to be tested as to their suitability or otherwise for boilermaking and shipbuilding purposes: and that when they had been exhaustively tried, they were pronounced to have stood very satisfactorily all the tests required by the Llyod's committee. Such a result should be encouraging to those steel-makers who are preparing to make by this process plates for use in British boiler and ship yards. Authentic returns of the production of basic steel by the seventeen firms who are making it show that the annual out-turn is at the rate of 558,800 tons. In the six months ending with March the precise tonnage was 279,400. It was made to the extent of 57,911 tons by the one firm in England; 5,962 tons by the two firms in France; 12,786 tons by the one firm in Belgium; 152,479 tons by the nine firms in Germany; 37,476 tons by the three firms in Austria; and 12,786 tons by the one firm in Russia. The make by Messrs. Bolckow, Vaughan, and Co.—The one English firm at present working a basic plant—is, it will be seen, at the rate of 9,651 tons per month. This is considerably over three times the average make per month by individual German firms, the foregoing returns show, are not only the largest producers of this class of steel in the aggregate, but also the largest producers, per individual firm, of all the continental firms who have adopted the system.

RAILWAY NOTES.

In the Cottran locomotive the driving axle, and those coupled with it, are provided with two pairs of wheels—one pair of the diameter best suited to the ordinary requirements of the road and the traffic, and the other pair—generally placed outside the larger—of a much smaller diameter, for running on supplementary rails when mounting heavy gradients. Thus, with a uniform number of piston strokes, and therefore revolutions of the wheels, the speed of the engine is reduced, with a corresponding gain of power, when there is harder work to be performed. This is not new as a proposal, and there are practical objections, such as obstacles lodging in the space between the inner and the outer rails, and capable of throwing the train off the track by contact with the clearing irons. Again, as the inner rails are necessarily lower than the outer, there is increased difficulty in forming level crossings; but this objection is of less weight than the former, as level crossings are a fruitful source of accident, and are being abolished by the Board of Trade wherever possible. The supplementary rails require to be laid very accurately, and tapered off at each end, so as to avoid any shock to the train when entering upon and leaving them.

The following is the estimated railway mileage of the world, January 1st, 1883: United States, 113,000 miles; Europe, 109,000; Asia, 8,000; South America, 7,000; Canada, 8,500; Australia, 3,200; Africa, 2,200; Mexico, 2,100. Grand total, 253,000 miles. These figures are not claimed to be exact. It is absolutely impossible to obtain official returns for the same period within a year or two after date, and so it is necessary to use the latest available statement, and add the probable increase since that time.

It is announced that the whole of the Centra Bengal Railway will be open for traffic by the end of the year, and that good progress is being made by the Bengal and Northwestern Railway. The earthwork of the entire system of the latter line will be finished early in June, and the greater portion of the brickwork. The *Times* Calcutta correspondent says it is also probable that 140 miles—from Sonepore to Goruckpore—will be ready to be opened next March, and perhaps even the entire line.

The foreign press has been devoting attention to the scheme of a city railway for Paris, the construction of which has doubtless been suggested by the success which has attended a like project at Berlin. Preliminary steps have been taken towards obtaining the necessary powers from the French Legislature. According to the plans now drawn up, the railway will be in two parts, one extending from the Lyons terminus to the Arc de Triomphe, and the other from Montmartre to Montrouge. It is also proposed to construct ten subsidiary lines, uniting at the new post-office. According to the calculations of the commission which has been examining the project, the two main portions of the line would be completed within three years of the necessary powers being obtained. A low scale of fares is projected, and the scheme includes the utilization of existing omnibus and tramway routes in correspondence with the new railway system.

ORDNANCE AND NAVAL.

Compound Armor Plates at Spezia.—The following conclusions from the results of further experiments at Spezia with pieces of the steel and compound plates fired at last autumn with the 100-ton gun are translated from the *Revista Marittima*, May, 1883:

From the experiments made at Spezia in March last against fragments of Schneider 48 cm. armor plates, it might be concluded that the metal behaves under test in an analogous manner to wrought iron, which at first was generally adopted for armor. The resistance to penetration in these plates is gradual, and in the metal which surrounds the point of impact there almost always appears a swelling with a versed sine in proportion to the quantity of metal of the projectile which has penetrated.

Moreover, the cracks which result from the shot all present a radial aspect due to the wedging action of the projectile, and these cracks, in the case of great penetration, develope also in the direction of the axis of the projectile, and result in a force tending to open the plate in the direction of the point struck The penetrations obtained in the trials are somewhat remarkable, although inferior to those which would have happened to rolled iron plates, for which, according to the Muggiano formula, should have shown the guns of 15 and 25 No. 1, a result between 23 and 24 and 32 and 33 cm. respectively.

It is true, however, when treating of fragments already damaged by previous shots from the 45 cm. gun, and struck at times at points presenting a great deficiency of resistance, either through want of support or from pre-existing cracks, such blows should not be reckoned; but we must consider that other shots have been fired against blocks in a good state of resistance, and of such relative sizes as to retain the proportion of weight between the 45 cm. projectile and the entire plate as tested in November last. In such a case the shot may be said to be sufficiently significant. At any rate, the continual occurrence of such phenomena, and their resemblance to those at Ochta, Gavre, and at Shoeburyness, give much weight to the information now collected. With these premises the experiments indicate that in firing against Schneider plates, projectiles of 15 cm. had about an average penetration of 94 mm., and projectiles of 25 cm., No. 1, a penetration of 163 mm. The behavior of the compound is very different from that of the Schneider plates; the hardness and the special tenacity of the steel-faced stratum tends to produce the breaking up of the most resisting projectiles, so that the resistance to the shot, instead of being gradual, may be constdered almost instantaneous.

The penetration is, therefore, much less than in the Schneider plates, and if we omit the blows upon points already much weakened, and by way of compensation those made with experimental projectiles, which were too weak in proportion to their energy, the average penetration is found to be 58 mm. for the 15a R.C. gun, and 74 mm. for the 25 gun, or 27 and 50 per cent. respectively less than in Schneider's. Nor do these figures fully represent the advantages as to penetration that may be expected from compound plates, since there is still included in the calculation the shot with the 25 gun against the Cammell fragment, in which, besides being excessively weakened, there was at the point struck a diminution of almost 4 cm. in the thickness of steel.

Excluding the result of the 25 cm. No. 1 gun, the average penetration against the composite plates was barely 58 mm., or the same as with the 15 cm. gun. This is remarkable, since it seems to indicate that in the composite plates the penetration remains always almost *nil*, whatever be the caliber of the projectile. The superiority of resistance of the compound plates depends certainly upon the high degree of hardness of their face, and it would appear it is not yet known how this can be given to hammered armor. But this excessive hardness would become a defect if there was not a stratum of rolled iron underneath. In fact with plates of one metal only, whenever a very hard quality is chosen the penetration of the projectile is diminished; but, on the other hand, most serious consequences arise by having too brittle armor. If, on the other hand, the metal is soft, much greater local effects are produced. For the plate of one metal only, the question is therefore reduced to finding a maximum limit of hardness compatible with a structure which is not brittle. The data that we have up to the present time are not sufficient for judging whether this limit was arrived at with the experimental Schneider plates, but we may assume with some foundation that it was not far off, since in all the blows struck it was remarked that the plates continued to crack for some minutes after the shots, an internal crackling being heard, and at intervals those metallic sounds which denote the process of separation of the metal.

The cracks in the case of composite differ considerably from those of the Schneider armor. Besides those in radial directions there were sometimes circular cracks, having their center at the point of impact analogous to those produced in a vitreous mass when struck. The only case in which a compound plate cracked completely was when, being imperfectly supported, it was subjected to a powerful bending force. One last remark seems necessary as to the bruising effects of firing upon armor plates of varying degrees of hardness. The method of fastening the plate contributes in no slight degree to modify the resistance that it presents to the effects of bruising. A plate of hard metal supported on a yielding backing is under very inferior conditions for resisting the force of the projectile to those of a plate of some non-ductile metal fixed in the same way. The former when not fastened in a rigid manner, if struck by a projectile, will be, subject to a bending force tending to its fracture, and proportional to the distance between the point of impact and the edge of the plate. In the case of armor plates of large dimensions not fixed in rigidly, whenever the blow takes place towards the center the force we have mentioned is very considerable. This is why the Experimental Commission has wisely proposed a blow in the center as a test for the reception of the compound plate. With a more malleable metal the reaction of the plate upon a pliable backing would, to an extent, do away with the bruising power of the blow. This difference in the behavior of plates of different hardness depending upon the special manner in which they are fixed, has no importance for plates intended for ships, since in this case the system of support may be considered as perfectly rigid, and therefore absolutely favorable to compound armor.

BOOK NOTICES.

A Visit to Ceylon. By Ernst Haeckel. Translated by Clara Bell, Boston: S. E. Cassino and Co.

Although this is the narrative of an eminent

naturalist it is designed to interest and even charm the non-scientific lover of travel. No traveler is so alive to all external influences as the true naturalist; so the habits of the people, their costumes, the climate, the incidents of every-day life, are as surely noted as the objects that demand scientific classification.

The narrative is, throughout, sprightly, thoroughly descriptive, and altogether agreeable.

Hand-Saws: Their Use, Care, and Abuse. By Fred T. Hodgson, New York: Industrial Publication Co.

Many people use hand-saws. Nearly as many misuse them. This little book is written, not in the interest of saw manufacturers, but of that larger class, the amateur artisans, whose time, patience, and money he thinks should be saved.

Chapter I, on the history of saws, is full of interest, and the subsequent chapters on selection and use of saws are full of valuable information.

Plumbing: A Text-book to the Practice of the Art or Craft of the Plumber, with supplementary chapters upon House Drainage. By William Paton Buchan. Fourth Edition. London: Crosby Lockwood & Co.

The text-book for the practical plumber under notice has grown out of a series of articles in the *Building News*, which were written in 1871. These articles had grown into a book in 1876, a second edition of which appeared in 1879, and now a fourth edition has been issued. The different forms of sanitary appliances are fully illustrated, and special attention has been paid in this edition to the subject of ventilation.

A Rudimentary Treatise on Clocks, Watches, and Bells. By Sir Edmund Beckett, Bart. Seventh Edition. London: Crosby Lockwood & Co.

This is practically a ninth edition of Sir Edmund Beckett's work on clocks and watches, for the articles written by the author as the eighth and ninth editions of the *Encyclopædia Britannica* were abridgements of this book. The portion devoted to clocks is practical, and intended to help those who wish to make, or direct the making of their own clocks of superior character; but as an amateur is not likely to make a watch, the part on watch-making deals more with principles than with working details. A list of the great bells of Europe, with date, diameter, and weight, is given at the end of the book.—*Jour. Society of Arts.*

MISCELLANEOUS.

A firm in Paris has patented an invention for the instantaneous formation of steam, which permits of its use at once in the cylinder of the engine. A pump sends the required quantity of liquid between two plate surfaces, which are heated, and between which there is only a capillary space. The liquid spreading in a thin layer evaporates at once, without going into the so-called spheroidal state, and this steam acts in the cylinder as fresh formed steam. The speed of the pump is regulated by the engine, the pump being connected with the shaft of the engine.

A nice opportunity, which should not be lost, of constructing a subway for gas, water, and electric mains, is offered by the construction of the Inner Circle Completion Railway along Cannon street. Along the whole length of this street a heading is made which is above the tunnel, and which will be filled in with earth unless the excavation is utilized for the construction of a subway. This might be done at a comparatively small expense, and thus save for ever afterwards the constantly recurring expense, and what is worse, street obstruction, by the operation of the gas, water, and electric companies.

The Manufacture of Camphor in Japan.—The camphor tree is very widely distributed in Japan, being equally common on the three islands Niphon, Kinshin, and Sikok; but it thrives best in the southern portion of the kingdom, namely, in the provinces of Tosa and Sikok. The sea coast, with its mild, damp air, agrees with it best, and hence the chief production of camphor is in these provinces.

Dr. A. von Roretz, of Ottanyama, Japan, states that the only tree which yields the commercial camphor of Japan and Formosa is the *laurus camphoratus*, which the natives call *tsunoki*. Camphor is collected the whole year through, but the best results are obtained in winter. When the camphor collectors find a spot with several camphor trees in the vicinity, they migrate thither, build a hut to live in, and construct a furnace for making the crude camphor. When that place is exhausted, the hut is torn down and carried to another place. The method observed in obtaining camphor is very simple. The workmen select a tree, and with a hollow-ground short-handled instrument begin to chop off regular chips. As soon as the huge tree falls, the trunk, large roots, and branches are chopped up in the same way, and the chips carried to the furnace in baskets. The furnaces are mostly built on the side of a hill near a stream of water, and serve for the wet distillation of the chips. The furnace is of very simple construction. A small circular foundation A is built of stone, and upon this is placed a shallow iron pan F 2 ft. in diameter, covered with a perforated cover E luted on with clay. This cover forms the bottom of a cylindrical vessel B 40 in. high, and tapering to 18 in. at the top. Near the bottom of this vessel is a square opening D, which can be tightly closed with a board. The whole vessel is covered with a thick coating of clay C, held in place by strips of bamboo. The cover of this vessel G, which is also luted on with clay, has an opening K closed with a plug. Passing through the side of the vessel near the top is a bamboo tube L leading to the condenser H. This condenser is merely a quadrangular box, open below and divided up by four partitions into five compartments communicating with each other. The open side of this box dips into water and is kept cool by water drizzling over it.

VAN NOSTRAND'S
ENGINEERING MAGAZINE.

NO. CLXXVII.—SEPTEMBER, 1883.—VOL. XXIX.

THE PROGRESS OF TELEGRAPHY.

By WILLIAM HENRY PREECE, F. R. S., M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

It is my misfortune, and not my fault, that I have to lead off this course of lectures on the Practical Applications of Electricity. It is my misfortune because it happens that Telegraphy is the oldest and the first of these practical applications, and though it is the oldest and the first, nevertheless it is very young, for it dates its birth only from the year 1837. The great shining lights of this Institution were present at its birth, and Robert Stephenson, Isambard Kingdom Brunel, Joseph Locke, and George Parker Bidder, were its godfathers. There are many (and doubtless there are some present to-night) living members of this Institution who assisted materially in its delivery. It grew around our railway system, and our railway managers were not slow in detecting the power that telegraphy gave them to marshal their trains, to adjust their traffic, and to protect life. In 1851 this art, if I may so call it, had scarcely commenced to take a commercial existence, but now it is only necessary to refer to the map to see to what an enormous extent telegraphy has grown.

A civil engineer would feel himself disgraced if he knew nothing of the strength of materials, of the pressure of liquids, of stresses and strains; but how many amongst my hearers are there who know much of electromotive force, of resistance, of currents, of volts, of induction, *et hoc genus omne.* An American author has told us that it is very dangerous to prophesy unless you know; but I think I am justified in prophesying this, that it will not be very long before these terms become household words, for they have already been admitted into commercial, legal, and parliamentary lore.

Now, gentlemen, what is electricity? Electricity, as we know it and use it, is a mere form of energy; it is brought into existence when it is wanted, and it disappears when it has done its duty. "Like the snowfall on the river—a moment white, then melts for ever." Like sound and light and heat, electricity is a mere abstract idea; it has neither substance nor shadow. Supposing I take a match-box, and out of that box I take a match, is there any man in this room who would say that in the head of that inert match we have light and heat? And yet when I strike that match we excite both heat and light. Would anybody say that in this bell I have sound? And yet when I strike that bell I produce sound. So, when I take a piece of zinc, will anybody tell me that in that zinc there is electricity? There is something, for the mo-

ment when I put this zinc into that battery, I have started something going, the bell rings violently; and the moment I take that zinc out, that something has ceased, for the bell becomes silent again. This zinc contains what the match contains, what my blow produced—energy; and it has simply been the conversion of this energy, first into one form producing chemical action, then into another form producing electric currents, then into a third form producing magnetic power, which produced that other form—sound, which made that bell audible to you all. Now, much brain-wasting power has been devoted in trying to picture some conception of this thing called electricity, but we cannot conceive the existence of that which does not exist. That which exists is energy, indestructible, convertible, and we in our practical applications merely utilize it in its electrical form. My duty to-night is to show you how we employ this particular form of energy and transmit it to the uttermost parts of the earth, there to do work, to express our wishes and our wants.

Now, this electrical form of energy possesses certain properties. It is found in a *potential* or passive state, and in a *kinetic* or active state. We have *electromotive force*, a term that is simply analogous to "head," when we speak of water, "pressure," when we speak of gases, "temperature," when we speak of heat; electromotive force in fact is simply a term analogous to difference of level, to the difference of potential energy that determines the flow of liquids. When we possess this difference of potential or electromotive force, we can produce a *current*, or the kinetic form of electrical energy, provided we supply a path for this flow of energy. It is impossible, with the materials at our command, to find anything that does not more or less oppose or resist the flow of electricity. Hence, materials possess what is called *resistance*, and electromotive force and resistance are expressed in definite magnitudes; they can be measured with greater exactitude than any other system of magnitude at the command of the engineer. The engineer can measure feet and inches and the thousandth part of an inch, but the electrician can measure the millionth part of an inch; in fact there is no magnitude or force in Nature that can be practically measured with greater exactitude and in more minute dimensions. The unit to which electromotive force is referred is called the *volt*, and that to which resistance is referred the *ohm*, while the unit current is called the *ampere*.

The great progress that has been made in telegraphy, and the great advances that have been made in the science of electricity, are due to the power that this system of exact measurement has given the engineer. Now, having at our command this form of energy, and being able to overcome resistance by its means and transmit it to a distance, the question arises, How is it produced? It is produced in nearly every form of telegraph by the simple combustion of zinc, in either sulphuric acid or some solution in which sulphuric acid bears an intimate part. There are exceptions. We have sodium chloride, and there are other liquids, but practically and generally the form of chemical action which takes place is the conversion of zinc into zinc sulphate. Now, without running you through the elementary details of the voltaic cell—a matter absolutely impossible within the hour and a half devoted to the subject—I may simply briefly point out to you the forms which these different batteries take. We have first Daniell's. In Daniell's zinc is consumed, copper is deposited, and we have a normal electromotive force that is very closely allied to our unit, the volt. But the Daniell battery has one serious defect, and that is, it works out quicker when it is idle than when it is at work—there is a considerable local action that very speedily destroys its efficiency. But Leclanche, in Paris, introduced a battery utilizing zinc acted upon by chloride of ammonium, and peroxide of manganese, in contact with coke, by which he succeeded in stopping, not only the local action, but he gave us an electromotive force 50 per cent. greater, so that ten of these Leclanché cells are equal to fifteen Daniell's. But we have gone a step further than this, by utilizing Poggendorff's discovery of the power of bichromate of potash. The result is we get a battery whose electromotive force is more than 50 per cent. that of Leclanché, twice that of Daniell's, and one that gives us an efficiency that is

simply marvelous for telegraphic work. Now, of these cells in our Post Office Department, we have 87,221 Daniell's, 56,420 Leclanché's, 22,000 bichromates; altogether we have about 165,000 cells; and by a rough calculation of the number of batteries in use throughout Europe, at the present moment, they exceed 1,200,000. There are other forms of apparatus by which we produce these currents. The mere movement of a coil of wire in front of a magnet causes those currents that are so powerful in producing the electric light, and by simply turning a handle in this way I am able, by rotating a coil of wire in front of the magnet, to create currents powerful enough to ring that bell.

Great improvements have been made in the form and efficiency of these batteries, by the application of scientific laws to the investigation of their performances; and, owing to the progress that science has made, it is quite possible to maintain batteries in a state of constant efficiency that ten years ago was absolutely impossible. By the aid of rigid tests, and by the aid of accurate instruments, we are able to maintain them, in their full state of perfection without ever deteriorating more than 25 per cent. below their normal value. The only defect that the bichromate battery has developed is a deteriorating influence on the health of the men attending to them, due to the action of mercury. Very promising experiments are being made with secondary batteries which will probably diminish this evil in all large centers.

The production of these currents of electricity being so simple and easy, the next question that arises is, How are they conveyed from place to place? We find materials divided into two classes—*conductors* and *insulators*. The conductors are materials which are transparent, as it were, to this flow of energy, and insulators are materials which are opaque; conductors offer comparatively small resistance, insulators offer great resistance. Copper is one of the best conductors; glass, porcelain, gutta-percha, are some of the worst, and therefore they are very good insulators. The first telegraphs were laid under ground. Here is a piece of a telegraph of five wires, erected between Euston and Camden, and buried underground—creosote timber, or prepared timber, with copper wire let into grooves and covered with a tongue. We call it the "fossil" telegraph. But it was speedily found that copper buried in that way could not be maintained in a condition of insulation, and therefore wires were put overground, attached to poles and insulators. In England, hitherto, we have invariably used wood creosoted, to check decay. Abroad, and in the colonies, iron is used to a very great extent, and to my left here I have one of the best—perhaps the best—iron pole that has been produced, that of the Messrs. Siemens, fitted up with insulators complete, so that you may see the form that experience has now taught us that iron should take to secure the greatest efficiency. Insulators are of all shapes and forms and sizes. Every man who has had anything to do with the advance of electricity has had a shot at a new insulator, and the result is that they are as numerous as the men who have been in power. But by dint of careful examination of all those in use throughout the world, and with the knowledge that it is essential to produce an insulator that shall be readily cleansed, this that I hold in my hand is now the form that is most generally adopted in England and India, and throughout our colonies. There is a very curious meteorological effect that we have to protect ourselves against in England, and I think England is peculiar in this respect, and for that reason the problem of insulation in England is more difficult than in any other country. The reason is this, that in England the prevailing winds blow from a warm climate to a cold one, while on the coast of America they blow from a cold climate to a warm one. Now, coming from a warm climate to a cold one, the result is that aqueous clouds speedily deposit their moisture upon anything in their progress colder than themselves, and the result is that the insulators become coated with a film of moisture—a film that is so dangerous in its consequences that sometimes the whole telegraph system of England has very nearly broken down.

The conductors are almost invariably iron, and within the last few years very great improvements have been made in the manufacture of iron wire. The im-

provements are so great that in the present day wire is exactly 50 per cent. better than it was seven or eight years ago. The No. 8 wire of the present day is as good as the No. 4 used to be. Again, wire is manufactured in long lengths; there are no welds, no joints—sources of enormous trouble in the early days of telegraphy. It has great ductility, it has considerable durability, but in the neighborhood of smoky towns, such as London, Manchester, and some of the places in the north, the decay of iron wire is very rapid. We have coated it with certain materials to try and check this decay. Those who are in the habit of travelling on the London and South-Western Railway may notice between Waterloo and Nine Elms that the material we use to preserve the iron has itself gone. It hangs in unpleasant festoons on the wire, and it was only a few weeks ago that I was asked what was that fungus that grew on the South-Western wires. The improvement in iron is so great that wire which had a breaking strain of some 25 to 30 tons now has a breaking strain of 40 to 50 tons; and wire is absolutely made for submarine cable purposes, and for long spans, with a breaking strain of 90 tons to the square inch. Copper, which is much used in districts where iron rapidly decays, has also followed in this train of improvement, and it is remarkable what variation has been found in the quality of copper. Five specimens of copper,

Samples of copper.	Conductivity.		
	1st test.	2d test.	3d test.
A	101.4	101.1	101.02
B	44.7	44.87	44.51
C	98.7	99.63	98 64
D	101.3	100.2	101.14
E	18.7	18.63	..

taken at random, were submitted to three individuals to test, and you will see that while the three individuals agreed almost exactly in their measurements, the copper varied, 100 being the standard of purity, from 18 per cent. to 101.4, or more than purity. The specific conductivity of the copper of commerce, unless checked and controlled by electrical tests, is liable to give those extremes of 44 and 101. Fortunately in telegraphy our tests are so simple that we can tell with absolute certainty the quality of the material we have; and now it is excessively rare to obtain copper for telegraphic purposes that gives less than 96 per cent. of pure copper. Compound wire, that is a small steel wire surrounded either mechanically or electrolytically by copper, has been experimentally used, but not with much success. Wire made from phosphor and silicious bronze is coming much into use, and it is a very promising material. The latter has the strength of iron, and the conductivity of copper, and if it only stands the test of time it will serve to supply a very serious want. We find now, owing to the multiplicity of wires, that our poles will not carry any more; and if any material with lightness and strength can be produced which will enable our poles to carry twice as many wires, it will be a valuable adjunct to telegraphy.

Wires are carried underground by means of gutta-percha, and the same improvements exactly that have been made within the past few years in iron have also been made in gutta-percha. The insulating qualities, the inductive capacity, the durability, and all other points, are gradually improving. We find that gutta-percha meets with enemies underground—wretched, horrible little insects that you cannot see with the eye, and can only detect with the microscope, make fine meals out of this coating of our wires, and produce considerable mischief. Vermin, mice, rats, seem to have a *penchant* for gutta-percha, so that the troubles we have are very great; but, nevertheless, gutta-percha as an insulator for telegraph purposes remains the very best material at our command. We have in England no less than 12,000 miles of underground wire, and the cry is very often raised that we ought to put all our wires in England underground. Those who make that cry do not know the difficulties that deter us from carrying that out. In the first place, the cost of putting wires underground is four times the cost of putting them overground. Next, the capacity of wires underground is only one-fourth that of wires overground, in consequence of a curious retarding influence upon the currents that slows the operations of teleg-

raphy. The result is that whenever we take into consideration any long length of underground wire, as, for instance, between London and Leeds, or London and Manchester, underground wires are commercially sixteen times worse than overground wires. You can readily therefore imagine that the authorities of the Post Office are not particularly anxious to put wires undergroud. We shall be very glad to do so if the Legislature will find the capital for the purpose, and the amount required to replace our system underground is only £20,000,000! Now this cry for underground work has arisen from certain snowstorms that have occurred recently. Snowstorms and their effects, like a great many other things, are very much exaggerated by the press. The press naturally makes a fuss at any rupture of communication, for it checks the news transmitted, but we always find that a snowstorm is a very fine thing to improve our traffic, for generally speaking, whenever a snowstorm has taken place, our traffic has increased at least 50 per cent. On the occasion of the great snowstorm of January 19th, 1881, the messages at the central station, which averaged 40,000 a day, sprang up to 60.471.

With regard to submarine telegraphy, I have only to refer to the map for you to form some idea of the enormous network of telegraphs that extend all over the world, and which has brought the uttermost parts of the earth into intimate union with London. We have now no less than nine cables crossing the Atlantic—eight in the North Atlantic, and one in the South Atlantic. We have cables coming around the Peninsula, along the Mediterranean, down the Red Sea, across the Indian Ocean, away through the Archipelago to Australia, and from Australia to New Zealand. From Singapore they go northwards through Hong Kong to Japan, and away through China and Russia back to England. We have wires coming down through the West Indies to the Gulf of Mexico, and connecting the West Coast of America. The result is, that there is scarcely a spot throughout the whole world that is not in intimate connection with England. To carry out this tremendous undertaking £30,000,000 have been expended, and there are no less than 80,000 miles of cable at the bottom of the ocean. I remember twenty-three years ago reading a Paper—my first Paper—before this Institution, and I ventured to promulgate the unheard-of doctrine, that we ought to make ourselves as acquainted with the bottom of the ocean as we were with the surface of the land. The President of that evening—not always distinguished for his courteous manner—gave me a very severe rebuff for daring to promulgate such an outrageous notion before this Institution. But, gentlemen, we have since sent ships to every sea. Her Majesty's ship "Challenger" has spent three years in surveying the depths of the ocean. She has found that there is "a life," and a real life, "in the ocean wave," and "a home in the rolling deep," and she has found that the deep "unfathomed caves of ocean" do bear "gems of purest ray serene," and she has brought back to us a knowledge, not only of the life of the ocean, but of the nature of the bottom, so that we can now say that we know more of the depths of the ocean than we do of the surface of many a continent on this globe. The result is, that cables are now designed to suit every depth and bottom, and the operation of laying a cable has become a simple matter. The Telegraph Construction Company who laid, not the last cable, but the cable, I think, of 1880, across the Atlantic, succeeded in laying it without any hitch, without any stoppage, in the incredibly short space of twelve days. Again, repairs of cables have become equally a simple matter. A fleet of twenty-nine ships is maintained in different parts of the world to keep our cables in order. The cables can be brought to the surface from any depth. The 1869 cable, of which I have a specimen here, was brought to the surface from a depth of 1,940 fathoms and repaired; that cable is now thirteen years old, and is working as well as on the day after it was laid. A cable in the Bay of Biscay has been picked up from a depth of 2,700 fathoms and repaired. I am therefore justified in saying that cables have become a solid property, and that their age, their estimated age, has increased considerably from what we took it some few years ago, namely, ten years, to certainly fifteen or even twenty years, and British capital-

ists are now justified in investing their money in such enterprises as this map displays, which I look upon as one of the greatest glories, if not the very greatest glory, of British enterprise.

We have some remarkable accidents in cables. You would scarcely conceive it possible that a cable could be destroyed by fire; yet we have had an instance where a cable was destroyed by fire. Some idle boys lit a bonfire on the beach immediately over the shore end, and the heat melted the gutta-percha and broke it down. We have had a cable broken by a bull; a mad bull rushed vehemently down the streets of Yarmouth in the Isle of Wight, into the harbor and got entangled amongst the wires there, and broke a submarine cable. In the Indian Ocean a cable was found broken, and when they went to repair it they brought up a whole whale. The whale had got entangled in the wire. The whale was dead, and so was the cable. Again, we find little treacherous animals attacking wires, such as teredos, zylophaga, limnoria, and a few other little creatures of that character, which bore into the cable, reach the copper wire, and break down the cable, and the result is, that strenuous measures have to be taken to protect cables from these villainous opponents. The cables that are now laid in depths liable to the action of these teredos are armor-plated; the gutta-percha is coated with a thin taping of brass, and microscopists and physicists have yet to find a little wretch that will pierce its way through brass. Now, gentlemen, I ought to have shown you certain specimens of cable; but you all know what a submarine cable is, and as there are plenty of specimens before you, for which I am indebted to the Telegraph Construction Company, the Gutta-Percha Company, the Silvertown Company, and others, you will see there, in various forms, the character of the conductor now in use.

I have shown how electric currents are conveyed from place to place. I want now to show how we can utilize these currents at distant places to appeal to the consciousness. An electrical signal can appeal to the consciousness, either through the eye, or through the ear. If the atmosphere of this room will only behave itself, I will show how we utilize one fact of electricity to produce effects; but let me assure you, that the vagaries of that needle before me at the present moment, are not due to electricity; they are due to certain currents of air that are flying about the room. However, perhaps I may be able to eliminate from the motion due to the currents of air, the motions due to electricity. I want to show you that whenever a wire conveying a current of electricity, passes in the neighborhood of the magnet, it causes that magnet to take up a position at right angles to the wire. This is the main and simple fact upon which most of our early telegraphs were based, and also upon which most of our present measuring apparatus are founded. Now this gutta-percha wire passes immediately over that magnet, and we have a battery underneath. When I bring these two wires together, you will observe the effect (illustration). It was not air, it was a current of electricity which produced that effect, and I will make him go back again. You will see that when I bring these two wires into contact, I produce a deflection of that needle. Well, that is the simple fact; but it will show, perhaps, better on these little instruments (single-needle telegraphs) before me. When I move this handle, I send a current of electricity around the magnet inside there, and I produce, as you see, an effect. Now, when I send the current in the other direction, I produce an effect on the other side, and then, by combining these two facts in different orders, we are able to form an alphabet, and convey words by spelling each letter—the letter A is that, B is that, and so on throughout the whole alphabet. The first and earliest instrument was this. It is not a Greek temple; it is one of Cooke and Wheatstone's original double-needle instruments—very pretentious in its appearance. In its day it was most useful. I can remember the time very well when, in order to make these needles work, we had to perch a little messenger boy astride on the top of this Greek temple, with a large magnet in his hand, in order to keep the needles steady. In the progress of scientific thought, the causes of that vibration, that interfered so much with our reading were speedily found out and eliminated, and the result was the double-needle instrument was converted into the

particular form that is known as the single needle which is found now in every railway station. There are in the Post Office no less than 3,791, and in the railway service of this country, 15,702; so that we have 19,000 of these little single-needle instruments in this country, carrying on the traffic of our railways and conveying wishes and thoughts from place to place.

The second effect that is utilized for telegraphic purposes, is the simple fact, that if you take a mass of iron and surround it with wire, sending a current of electricity through the wire, you convert that piece of iron into a magnet. Without electricity, I may move this piece of iron amongst those nails, and the effect is *nil;* but with electricity, the effect is very different, the nails are attracted and cluster about each other in a very striking way, but the moment I take the current away the effect disappears. You see the moment I transmit the current through the coil, we have magnetism strong; the moment I take it away the magnetism ceases.

In order to produce sounds, in order to produce effects, it is only necessary to imitate the motion of the hand. Supposing I want to strike that bell, I merely give a little joint movement, and I strike that bell and produce some sound. Now let me put my magnet there, and in front of that magnet I place a little bar of iron jointed, instead of the rough and uncouth nails, and you will see that whenever I send my current through the coil I produce an attraction. You see how easy it is to put on that iron bar a little hammer, and in front of it something that will emit sound, and there I produce a sound. By the bye, let me show you how to make a bell ring, because there are many of you interested in railways, and you will see how simple it is to produce the sound of a bell. You see, by the effect of currents, I simply reproduce the movement of my wrist. Those two effects that I have shown you are the basis of all telegraphs, or nearly so, that exist at the present day. Either we avail ourselves of the deflection of the needle, or we avail ourselves of the production of magnetism. We have the simplest form of telegraphs known in Wheatstone's A B C, of which there are 4,398 in use in the Post Office. There the simple rotation of a needle propelled by currents, causing it to dwell opposite the letter that you want to indicate, enables you to spell out a message; you simply rotate a handle and work some finger-keys, and the result is you are able to cause a little index to dwell opposite any letter of the alphabet you wish to send, and any child or any old woman can work it; but the effect is rather slow, and this instrument is gradually being replaced now by an instrument that will be brought before you the next time we meet, called the telephone.

Acoustic reading—reading by sound—is the advanced character of the telegraphs of the present day. We commenced with a slow and cumbrous double needle; we passed to the single needle, and about the year 1852 one or two various classes of apparatus were designed and devised by Mr. Henley, by Sir Charles Bright and his brother, and by others. Many companies were formed to carry these out; and when the telegraphs were bought by the State there was the survival of the fittest; the best adapted were selected, and the result is that at the present time we have the A B C for small stations; the single needle for bigger stations, and, where real business involving a skilled staff is required, we come to the acoustic instruments. One of the earliest is the "Bell," of the brothers Bright, and here we read by an alphabet formed of two sounds, differing in tone or pitch. That is one sound; that is another sound; and thus the alphabet is formed; so that you have the whole of the letters of the alphabet formed by a mere succession of sounds, each combination of sounds itself distinctly indicating its character as clearly as we say A, B, or C.

This system of acoustic reading has passed through several stages to the instruments we use at the present day. Morse invented the alphabet that is used. Morse's instrument was first employed in America; it came over to this country and to the Continent, and at the present moment, while there are 1,330 Morse instruments in use in England, there are over 40,000 in use on the Continent. The best form is that produced by the house of Siemens, where the characters of dots and dashes are imprinted in clear

ink upon a paper strip. Here is the paper strip, and the paper passes through this instrument, and the characters are depicted upon it in a way I shall show you presently. But it was very soon found that the dots and dashes that were made by a magnet like that, conveyed to a man's ear by their sound precisely the same idea that they conveyed to his eye by the marks they make; he could detect by the ear precisely what was wanted, and sound-reading came in not by invention, but by accident, or by a combination of the two. The instrument broke down; the little magnet continued its work, and the skilled clerk was able to carry on the work by his ear. The result has been that these instruments are simplified in their character; they are more expeditious in their action; they are more accurate, and the rate at which they work is simply the speed at which a man can write. The sounder is an importation from America, where scarcely any other form of instrument is used. In 1869 there were none in England, now there are 2,000. On the Continent there is scarcely one! Now we have got wires brought into this room; one I hope goes to Birmingham; the other may go—I don't know where, but we shall find out directly.

(Mr. Preece then told his assistant to ask for an alphabet from the Central Station. The alphabet was sent so that every one in the hall could hear it.)

You have seen, from the actual operation of reading by sound, how the alphabet is made up by dots and dashes. Although I said that that instrument is distinguished by its great accuracy, nevertheless I do not pretend for one moment to say that it is not inaccurate. Errors are inherent to telegraphy, and, do what we will, errors cannot be avoided. The personal equation enters very largely into telegraphy. A telegraphist is in the position of not seeing what he writes, and of not hearing what he says, and therefore he is in a very much worse position than we who see what we write, and hear what we say. And yet how many of us are there in this room who can write for a very long time withaut making a mistake, or who write a signature that is legible all over the world? In order to prepare myself for this lecture I communicated with every railway company in England. I was laboring under the delusion that my name was pretty well known amongst the railway world at any rate, but I found I was mistaken when I got a letter back addressd "W. H. Keene." It came back addressed "W. H. Green;" it came back addressed "W. H. Greer;" but the worst blow of all was to receive this letter addressed "W. H. Piller!" Now if an accurate and beautiful writer like myself has his signature so mistaken, you cannot fail to comprehend why telegraphists make mistakes sometimes. The mere loss of a dot may cause a mistake. For instance —you have all heard the story perhaps, but it is so good that I cannot resist telling it again: a party of young ladies from a school went to a certain place, and the schoolmistress was very anxious to know of their arrival, so the message was sent, "Arrived all right;" but the schoolmistress received the message, "Arrived all tight!" Now, gentlemen, when the mere loss of a dot occasioned that, you can readily understand how mistakes arise. There was another one that perhaps many of you here may understand. A message was sent, "Five fathoms of eight feet will do;" it was received, "Five fat sows of eight feet will do." Now, there is scarcely any difference whatever between "fathoms" and "fat sows" except a short pause. I could go on all night telling you these stories, but I have come not to amuse but to instruct you. I want to point out that the progress made in the applications of electricity to commercial purposes has been followed by progress in the character of the apparatus used, in the mode of working, and in the efficiency of the staff employed. Careful specification, exact measurement, sound workmanship, and rigid inspection, have given to telegraphic apparatus a character that I defy any other workmanship in the world to surpass. Cheap and nasty are synonymous terms in telegraphy. There are on this table instruments that, in construction, in design, and in workmanship, will bear comparison with the finest chronometers that were ever produced. The progress of telegraphic apparatus is an admirable example of growth by cultivation, of evolution by scientific selection, and of the survival of the fittest.

We have introduced various modes of working, but of all the machines the one that is used to the greatest extent on the Continent, and which in design and workmanship will equal any of them, is the beautiful type-printing apparatus of Professor Hughes. The instrument is used exclusively by the Submarine Company between England and Europe, and as an international instrument all over Europe. It is worked direct between Paris and Constantinople. There are three or four thousand of these instruments at work. We have one here, and I will ask that the alphabet be sent. Now, in this instrument we have letters and words reproduced in bold Roman type, and doubtless many of you who communicate with the Continent receive messages printed in this bold and legible character. After the lecture is over we will have many slips printed, and you will have the opportunity of seeing it in working order. I should like to have spoken of the instruments used in our exchanges and clubs to record news and the price of stocks, and I should like to have spoken of Sir William Thomson's beautiful siphon-recorder that is used on all long submarine cables; but the clock in front of me tells me that I am like many trains in this country—rather lagging behind by time; so I must resist the tendency I have to dwell on many of these things, and run rapidly through my programme,

In the instruments I have shown you, and in the mode of working you have seen, we have what is called simple telegraphy. I want to show you how we can work duplex, that is, instead of sending one message in one direction we can send two messages at the same time in opposite directions. This is one of the simplest of the phenomena that we use, and one that I have very strong hopes of being able to make you understand. For that purpose I have here two instruments, and they are in connection with each other, so that when I move one needle the other is deflected; when I send a current of electricity from here to there I move both these needles; when Mr. Cooper sends a current of electricity to me he also moves both those needles. Now I want to make use of this simple fact, that when I send a current of electricity to the other station at the same moment that he sends a current of electricity to me, these two currents neutralize each other, and nothing whatever passes upon the wire. Many people imagine that in duplex telegraphy currents pass each other. Nothing of the sort. Nothing whatever passes; we utilize the fact that the wire is neutralized and that nothing passes. When I move my needle I want to so arrange my needle that my own current shall not affect it, and for that purpose the needle is surrounded with two wires, through one of which the current goes in one direction, and through the other the current goes in the reverse direction; so that if I make those two currents exactly equal I shall obtain neutrality for my needle—now you see I have not got neutrality—but by putting resistance in I at last get to a point when my needle is not affected but the other one is. I will ask Mr. Cooper to adjust his in the same way. Those two instruments are now so adjusted that while he is working his, mine only is affected, and while I am working mine his only is affected. Why is this? Because I have two currents going around my needle in opposite directions, and he has the same; but if we check or neutralize one of those currents the result is that the other will act. Every time he moves his handle I may move my handle: we do not affect each other in the least. I keep sending dots, he sends something else, and the result is that he can cause my needle to move as he pleases, and I can cause his needle to move as I please, and it is simply owing to the fact that we utilize the neutralization of currents to produce effects at our own station. We have a wire here that goes to our central station, and I hope to be able to show this system of duplex telegraphy working in actual operation. We have got Birmingham here. You see duplex working between this room and Birmingham. [A message was sent while one was being received.]

When you send messages in opposite directions at the same time you have duplex working; you can, however, send two messages in the same direction, though you cannot send two currents in the same direction at the same time. You can, however, send currents which shall vary in direction, and currents

which shall vary in strength, and you can have one instrument that will record its marks by any change in direction independent of strength as well, or you can change the current in its strength whether it goes in one direction or the other, and you can make an instrument respond to changes of strength independent of direction; so that you have one instrument responding to change of direction, and another instrument responding to changes of strength.

That leads me to duplex working—two messages going in the same direction at the same time. Well, if you can have two messages going in the same direction at the same time, and two messages going in opposite directions at the same time, the result is you have quadruplex working, or the method by which four messages are sent upon one wire at the same time, an importation from America where this mode of working was made practical. [Quadruplex working with Birmingham was now shown.] Of course, in absolute telegraph work we have all the morning before us to adjust, or at any rate before business commences; but for illustration in this hall we could only get the wire after seven o'clock. Now, you will notice how we speak to a station, although it is 115 miles away, as though it were in the next room. Space by telegraphy is absolutely annihilated.

You have before your eyes what is taking place every day in England and in America, on a great many circuits. In England we have thirteen of these circuits, and some of them are worked in a curious way. Here [drawing on the board] is West Hartlepool, here is Middlesbrough, here is Leeds, here is London. We have one wire between Leeds and West Hartlepool, another wire between Middlesbrough and Leeds, another wire between Leeds and London. Now that wire works quadruplex between London and Leeds, duplex between Leeds and Middlesbrough, duplex between Leeds and West Hartlepool. West Hartlepool works duplex to London, Middlesbrough works duplex to London, and both those stations can work duplex to London at the same time and on the same wire.

But beautiful as this mode of working is, and greatly as it has increased the capacity of our system, it does not compare in design, in efficiency, or in adaptation to what I am going to bring before you now, viz., Wheatstone's automatic-working system. On this we utilize Morse's alphabet. Here mechanism supplants manual labor. There (on the quadruplex) you saw the clerks working, and the rate of their working was dependent upon the rate at which they could key, and that varies from 20 to 30 words a minute, in fact we may take the average number of words sent by a good telegraphist per minute at, say, 30; but when we replace manual labor by mechanism, we can increase that limit to almost anything, and I will show how we are receiving work upon our wires at the present moment at the rate of from 200 to 250 words per minute. From 1870 to about 1873 or 1874 the rate of increase of wire and of messages was about the same, but then there was a departure. We commenced to understand automatic working and duplex working, and during the last five or six years we have carried it to such a pitch that while the mileage of wire has increased about 100 per cent. messages have increased over 230 per cent.; so that by the teachings of experience and by the application of scientific thought, we have succeeded in so increasing the capacity of our wires that they have been able to meet the tremendous increase of business. I will show you how this is done. We have a wire here. We prepare our messages by mechanism, and instead of sending them by hand they are punched on paper. Here is an alphabet punched on paper; it is like the preparation of paper for lace in the Jacquard loom, and this perforated paper will be passed through a transmitter. A clerk can only punch at the rate of 30 to 40 words per minute; the instrument can send this punched paper at the rate of 200 to 250 words per minuute, so that the instrument is able to take off the woik prepared by five or six clerks. Now I want to show you how the news of this country is transmitted. We have here London [drawing on the black board], Leeds, Newcastle; we have there Edinburgh; we have there Glasgow; we have there Dundee; we have there Aberdeen, and we have there the Institution of Civil Engineers. Now, a strip is punched by

several punchers; it is put into the transmitter at the central station, as you see it there [pointing]. Now the strip is going through the instrument, and at the same moment a similar strip is sending its messages to Leeds, to Newcastle, to Edinburgh, to Glasgow, to Dundee, and to Aberdeen, and every one of those stations, as well as on other wires, Liverpool, Manchester, Birmingham, Bristol, Newport, Cardiff, Plymouth, Exeter, and in other directions other stations, is at the present moment being filled with exciting news, by means of that apparatus that you see before you, and this is going on at the rate of 200 words a minute; so that we have I cannot tell how many writers, at how many stations being supplied by this process. We have twenty-eight of these news circuits at work. And that the process is appreciated, if anything can be appreciated by our press, is evident by the fact that instead of the 5,000 words sent per day that were supplied to the newspapers when the telegraphs of this country were in the hands of private companies, there are 934,154 sent now, and last year there were no less than 340,000,000 words suplied to the press; and still they are not satisfied. In 1871 the number of words delivered in one week was 3,598,000; in 1882 it became 6,557,000! This work is done at an absolute loss. The Government has to pay a considerable subsidy towards providing the newspapers of this country with their daily pabulum of news.

One of the great steps in advance, one of the means by which this tremendous business is done, is this system of automatic working. Five years ago we were only able to transmit 147 messages per mile of wire, now we transmit 256, the ratio of 147 to 256 indicating the rate of improvement in the capacity of our wires. One great improvement that we have introduced is fast speed repeaters. This is a repeater. It is an exquisite instrument in itself. It is very complicated, but very simple in its working; and the result of the introduction of these instruments is to render the rate of working on long circuits the same as the rate of working on short circuits. We have eighty-one of these repeaters in use. By the insertion of a repeater at Leeds, and another at Edinburgh, we are able to maintain that speed of 200 words a minute to all those stations shown on the board, and the value of this repeater is such that there is no more difficulty in sending at a fast rate (I will not say 200 words a minute, but 100 words a minute), between London and Edinburgh, than there would be in sending between London and Calcutta.

On the Continent efforts have been made by Meyer and Baudot to increase the capacity of wires by the application of another principle, called multiple working. A unit of time is divided into four or more sections, and each section apportioned to a pair of telegraphists at the two ends of a circuit—one to send, one to receive. Each section of time allows one letter to be sent, so that four messages can be in the act of transmission at the same time, though there are no simultaneous signals, as in the quadruplex apparatus. A gain of speed is obtained with type-printing instruments, but the game is not worth the candle, for the additional apparatus needed is complicated and delicate. Its success has not been marked, though an immense amount of talent and ingenuity has been expended on its development. The exquisite mechanism of the Baudot apparatus was one of the features of the Paris Electrical Exhibition of 1881.

With great grief I pass over notes involving some interesting matter that I cannot possibly bring before you, but which you may have an opportunity of reading some day or other. Although statistics are very dry in themselves, statistics are most interesting as showing the progress made in this country and in the world in telegraphy. I have been to America, I have been on the Continent of Europe, and I can say this without hesitation, that all these countries have a lesson to learn from us. People are very fond of bringing before us America as an example. We have taken many lessons from America, but America has been very glad to take us as an example also, and America is now applying on their principal wires in the Western Union system, not only this apparatus, but we have had the pleasure of selecting and sending out experts to show them how to work this system. We use the telegraph more in England than they do in America, and our tariff

is less. In England every hundred persons send 91 messages, while in America they send only 77. We send ten times as much news for the press. The amount of work done in England is indicated by that table, but I can give you a fact interesting in itself. I remember the time when we thought 1,000 messages a day a tremendous business in our central telegraph station. On August 4th, 1882, there were not 1,000 messages sent, but 92,017. Gentlemen, I will not occupy your time by giving you many figures. The traffic of the Eastern Telegraph Company has increased in equal proportion; in 1871 they sent 186,000 messages; in 1881 they sent 720,000; and the same argument is equally applicable to them—that they have improved their rates of working as we ourselves and other organizations have. The cable companies do an enormous business, and by the way in which they do their business they have revolutionized trade; they have completely altered the mode of transacting business throughout the world; the home trade of this country has been extended to all quarters of the world, and the old middleman is gradually disappearing. In foreign countries, too, we have some remarkable instances of progress in telegraphy. Japan, for instance, last year transmitted no less than 2,223,214 mesages, and of these more than 98 per cent. were in their own native tongue; while contiguous China does not possess a single telegraph of its own at the present day, as far as I am aware. There were in 1880, amongst the administrations who had joined the International Bureau of Berne—

Lines of Telegraph..	268,000 miles.
Wire..............	768,600 "
Instruments........	53,144 "

The development of railways in this country has necessitated a corresponding increase in the telegraphs required to ensure the safety of the traveling public, and while 27,000 miles of wire in England, Scotland and Wales, were used for that purpose in 1869, at the end of December, 1882, the total had increased to 69,000 miles, equipped with 15,702 instruments, against 4,423 in 1869.

TRANSMISSION OF POWER BY BELTING.

By EDWARD SAWYER.

From the Proceedings of the Society of Arts.

The transmission of power by belting was intelligently investigated by Gen. Morin, the well-known French physicist, some forty years ago. I have not looked back of that for any publication on the subject. Subsequent writers of the better class seemed to have followed Morin. Other writers have published theories and rules which are clearly erroneous, probably without knowing anything about Morin's investigations. An empirical rule, however erroneous its theory may be, if deduced from a correct practical example, will of course work back correctly to the same case, and approximately to others which are nearly like it; but when the conditions differ widely from that, the result may be grossly erroneous. Writers of another class have published a great number of statements of practical cases, giving the widths and speeds of belts driving certain machines, sometimes accompanied with guesses more or less wild as to the power conveyed, satisfactorily or otherwise.

If I pull or push on a pulley strongly enough to move it, the amount of work done depends on the force and on the distance moved by the point of application in a given time. A belt does its work, of course, by transmitting a pull from one revolving pulley to another. Tnere is no difficulty in estimating its velocity from the diameter and rate of revolution of either of the pulleys, but the amount of its pull is not so easily ascertained.

In the use of pulleys and belts we find it is necessary to have more or less tension in the following or slack side of the belt to prevent slipping. Obviously this tension makes a back pull on the driven

pulley and balances an equal number of pounds of the pull of the leading side, and the difference of the pull of the two sides of the belt is all that is available for driving. And here we strike the principal difficulty of the whole matter. We must ascertain how much the tension or pull of the leading side can exceed that of the following side without causing slipping, with different kinds of surfaces and under different circumstances.

Morin proceeds as follows: He assumes, by implication, that the hold of a belt and pulley is due to ordinary friction produced by pressure, and that it is practically the same whether the belt and pulley are at rest or revolving together. The next step is to ascertain the coefficient of friction for various kinds of pulley and belt surfaces. For this purpose he used plane surfaces as more convenient for experiment than pulley rims, and presumably giving the same results. Neglecting the rigidity of the belt, he demonstrates by the integral calculus what the hold of the pulley will be for different frictions. From his experiments he found a coefficient of friction of 0.28, whence he deduces that 1 pound pull on the slack side will hold 2.41 pounds on the tight side; or, to put it in a way which I think brings out the controlling ideas better, 1,000 pounds on the tight side will be held by 415 pounds on the other side, leaving a force of 585 pounds for moving the pulley.

But while Morin's principles seem to be correct, his statements about the coefficient of friction are too brief and indefinite, and the value which he adopts for the case of leather and cast iron is greatly in excess of what I find. He does not say whether he took the pull required to start the slipping, or only that required to keep up the motion after starting; the latter would seem to be the only safe thing to depend upon. It seems possible that he may have used iron surfaces as they came from the foundry, and perhaps he had a different quality of leather from that now used.

The idea that the hold of the belt is due to friction caused by the pull of the belt seems to me to be a reasonable one, and it agrees with all the facts in the case so far as I know. We see that there must be tension on the slack side of the belt to maintain the hold of the surfaces on each other. For any given conditions and materials, the amount of this back tension must be proportional to the hold required, that is, to the number of pounds which the driving surface has to pull on the other to do the work.

The coefficient of friction is found to be independent of the intensity of the pressure by experiment both on flat surfaces and on pulley runs. Here are two cast-iron pulleys, eight inches and fifteen inches in diameter, both having surfaces finished and polished in the ordinary way, and just alike as far as known. We clamp them on a stand, hang different kinds of straps on them, and find what weight on one end will just suffice to stop the sliding caused by a heavier weight on the other end. Numerous trials show that the average hold of the belts is as good on one pulley as on the other, and that the smaller weight is the same fraction of the larger one on both pulleys, but of course not the same for different kinds of belts.

The notion is very common, and it is held by many intelligent persons, that a belt will hold more on a large pulley than on a small one. So far as can be learned from experiments on pulleys at rest, this is one of the many cases where people know things that are not so. By the unanimous verdict of competent experimenters, this notion is exactly the reverse of the truth, and we must eradicate it altogether from our minds before we can have a correct understanding of the subject. If we increase the size of a pair of pulleys without changing the speeds of the shafts (or the number of revolutions per minute), they will travel faster and do more work in the same ratio, the tension and hold remaining just the same as on the smaller pulleys. It is absolutely essential that we should keep in mind the two factors of power—*speed* and *force*—and not credit one with what is due from the other. But here we have run into the question of whether the hold of the pulley and belt are the same when they are revolving together as when at rest. Doubtless the jerking and jumping due to variations in the belt and irregularities of all kinds favor slipping, but in well-arranged belt transmissions the loss of motion between driver and driven is found to be very slight, which seems to indicate that we

need not make much allowance on this account. Centrifugal force tends to throw the belt off from the pulleys, and hence diminishes the hold. The amount of this will vary greatly with the size and speed of the pulleys, as shown by the following estimates:

A pulley of one foot diameter at 150 revolutions loses .006—ordinary machine pulley.

A pulley of three feet diameter at 300 revolutions loses .047—ordinary counter pulley.

A pulley of eight inches diameter at 1,500 revolutions loses .116—picker beater-pulley.

A pulley of four feet diameter at 400 revolutions loses .148—main pulley.

A pulley of five feet diameter at 400 revolutions loses .186—main pulley.

From which we see that it is unimportant in ordinary cases; but that some allowances should be made for it in the case of quick-running blowers, circular saws, picker beaters, etc., and rather more in case of pulleys four feet in diameter and upwards, making four hundred revolutions per minute, which is near the maximum for main shafts in large modern mills. But, then, quick speeds have come up almost entirely since Morin's time.

If the running belt forms a vacuum between it and the pulley, this tends to offset these losses. I know of nothing else which is material on this question. Hence, I suppose that in ordinary cases we may reckon the hold to be about the same in running as at rest. But one must not overlook the allowance for centrifugal force at very high speeds.

Experiments on the hold of pulleys in motion would be interesting and valuable if carefully and skillfully made. When a belt is moistened with some oily substance we may get some advantage from fluid adhesion; on the other hand, the lubricating effect may offset it. When a belt is coated with grease so that the leather does not touch the iron, we have another kind of surface, which may hold better or worse than leather. Sometimes a belt gets glazed over with oil and grime so that it has a very poor hold on the pulley.

If the belt is overworked, too much of the tension is taken out of the following side and put into the driving side, and, as soon as the disproportion between them becomes too great, slipping results. Of course the belt can be tightened, perhaps enough to enable it to do its work temporarily, but if it is overworked, it will show it by stretching and slipping again. One remedy for this is to put on larger pulleys, and thus run the belt fast enough to do the work with the pull which it will stand satisfactorily. Another remedy is to put on a belt wide enough to stand the requisite pull at the present speed. The superior efficiency of the wider belt is due to the fact that it has more stock and will stand more pull than the narrow one, and not at all to the fact that it covers more surface on the pulleys.

Many people do not understand this, but think a narrow belt has to pull harder than a wide one to do the same work, thus causing more friction and wear on the bearings. Some very skillful manufacturers labor under this misapprehension. The total pull is the same, but the pull per inch of width of course increases as the width of the belt diminishes; hence the narrow one will stretch the faster—if too narrow, it will soon become slack, and slip; if a belt runs a long time without doing this, it proves that it is large enough for its work. We can double the stock in a belt, and so double its efficiency, by doubling the thickness, without any increase of contact surfaces on the pulleys. But usually it is better to increase the width and speed of single belt till the pulley reaches somewhere about 36″×6″ before putting on double belts.

We come now to the question of amount of back pull necessary to prevent slipping. Perhaps the most satisfactory method of proceeding will be to take up the direct experiments first.

On polished cast-iron pulleys, hard, new leather belts require fully 75 pounds to hold 100 pounds; but usually the ratio is between 60 and 70 per 100, corresponding to co-efficient of friction from 0.17 to 0.12. Pieces of old belting, and thoroughly oiled, averaged better; some trials went as low as 56 per 100. Rawhide belting appears to hold very well, giving an average a little over 60 per 100. Rubber belting averaged a little under 60 per 100.

The 12-inch pulley with an old, oily,

leather lagging, just as it came from the mill, required not over 20 pounds per 100 pounds with the old leather belt. A pulley turned, but not polished, required about 65 pounds per 100.

If these results are correct, or nearly so, it is not safe to reckon on transmitting more than one-third or one-fourth of the pull on the tight side of an ordinary leather belt to a smooth iron pulley with 180° of contact. A usual method of reckoning in this country has been that a one-inch single belt running at one thousand feet per minute with 180° of contact will transmit one horse-power without any rapid deterioration. This requires much more than the proper maximum strain as given in standard books. Claudel says that a belt will run a long time without stretching at one-fourth of a kilogramme per square millimeter of cross-section, or about 55 pounds per inch of width for single belting.

The proper width for belting to convey a given amount of power may be calculated by the following expression, in which the constants are such as correspond to the best practice:

For double belts:

Width, in inches = 600 × H. P., divided by speed of belt in feet per minute; or 191 × H. P., divided by number of revolutions per minute × diameter of pulley in feet, H. P. being the horse-power which is to be transmitted.

For single counter-belts: twice as wide as double belts.

For machine belts:

Width=1,500 to 2,000×H. P., divided by speed; or 477 to 636×H. P., divided by number of revolutions×diameter of pulley.

In these cases smooth iron pulleys are supposed to be used.

A belt under tension binds around the pulley, and will press against it and produce friction by an amount which will depend upon the amount which the belt wraps around the pulley. This pressure or hold of the belt may be calculated mathematically. The most satisfactory solution of the problem is by the use of the integral calculus, but an approximate solution is readily made without the calculus, and the results of such a solution were given upon the blackboard. By this it was shown that when a belt having a coefficient of friction of about 0.10 goes half-way round a pulley, a little more than one-quarter of the pull on the tight side is communicated to the pulley, and the remainder must be left in the slack side to prevent slipping. Similarly, when the coefficient rises to 0.44, three-fourths of the pull on the tight side may be communicated to the pulley, leaving only one-fourth on the slack side. If the belt does not wrap the pulley for 180°, the practical rule is that the width of the belt should be increased in nearly the same ratio that the angle of contact is diminished from 180°. The stiffness of the belt may slightly diminish the hold on the pulley, but only in case the pulley is quite small. Variation in area of contact with same total pressure does not vary the hold, but slight changes in the condition of the surfaces may considerably change the co-efficient of friction. Some erroneous formulæ, arising from a disregard of these facts, were then discussed.

Uniformity of thickness of belts is important at high speeds. Double belts should be limited to one thirty-second of an inch variation. Rubber belts possess an advantage in this.

Overstraining of belts from careless management causes unnecessary wear of belts and machinery, and great loss of power by friction. The importance of careful adjustment of band strains has of late years received considerable attention in spinning-frames, and its importance deserves more general recognition.

At a meeting of the Lower Rhenish and Westphalian Engineering Association, held a short time ago, Herr Gleim gave some detailed particulars as to the use of steel in the construction of bridges. He alluded to the fact that American engineers attach more importance to the extensibility of steel than to its possessing a high degree of strength. In the standard bars used for the tests—8 in. in length—the former quality must represent 15 to 10 per cent. before a fracture takes place. The productions of American steel manufacturers have now, he remarked, a strength of 29½ tons to 35½ tons per square inch, while the limit of elasticity lies between 16 tons and 17 tons approximately. He further stated that from the fact of the limit of elasticity of steel being somewhat over the half of its ultimate strength, while in iron it is, he says, much less. American technical authorities claim for steel an advantage over iron in greater proportion than the difference between the relative ascertained ultimate strengths of the two substances.

THE RELATIONS BETWEEN THE SIZE, SPEED, AND POWER OF MARINE ENGINES.

By RICHARD SENNETT, Chief Engineer Royal Navy, Memb. Inst. M. E., &c.

From the "Journal of the Royal United Service Institution."

I NEED scarcely remind the members of this Institution that the relations between the size, speed, and power of marine steam-engines are not absolute and unchangeable, but that they have been continuously varying during the progress of steam navigation. In fact, we may say, that advance in marine engineering has resulted from, and been marked by, the modifications made from time to time in these relations.

The elements on which the power of any steam-engine depends are the dimensions of the cylinder, the speed at which the piston moves, and the pressure of steam employed. In general terms the power may be taken to vary as the *size* $\times$ *speed* $\times$ *pressure*. It will be seen from this, that the relations between the size, speed, and power, are directly affected by the pressure of steam at which the engine is worked.

One of the most interesting and remarkable features in the progress of steam navigation has been the successive increments in the working pressures of steam used. The rate of increase may be roughly sketched as follows:—In the few steamships that existed prior to the year 1840, the usual working pressure was 4 or 5 lbs. per square inch. Between 1840-50 tubular boilers were substituted for the old flue boilers, and the working pressures were from 10 to 15 lbs. per square inch. From 1850 to 1860 the ordinary working pressure was 20 lbs. per square inch. Between 1860-70 surface-condensing engines became general for marine purposes, and were worked, as a rule, with steam of 30 lbs. pressure. The introduction of surface condensation, by which system the boilers are fed with fresh water, enabled high steam pressures to be carried with safety in marine boilers; and since 1870 compound engines have become almost universal for steamships. These were at first worked at 60 lbs. pressure. The pressures have gradually increased from 60 to 80, 90, 100, and in some recent examples to 125 and 150 lbs. per square inch. The ordinary working steam pressure in marine boilers at the present day may be taken at from 90 to 100 lbs. per square inch, and it is probable that still higher pressures may be used before long.

The increase in the working pressure of steam has produced two effects:

1. Reduction of the expenditure of coal; which is perhaps the more important, for this, and this alone, has rendered steam navigation for long voyages possible.

2. Reduction of the weight and space occupied by the machinery; this, from many points of view, especially for war-ships, is scarcely, if at all, less important than the reduction in the coal stowage.

These two divisions are so closely allied that it is somewhat difficult to deal with one without referring to the other. The subject of economy of fuel, however, important as it is, does not exactly come within the scope of the present paper; and I therefore propose to confine attention, as closely as possible, to the consideration of the reductions in size and weight of engines of a given power, that have been made, from time to time, by increasing the working pressures of steam and speeds of piston.

This is one of the principal problems with which marine engineers have to deal, and much progress has been made in its solution during recent years, and in connection wiih the machinery of ships intended for purposes of war. In war-ships, reduction of the space and weight required for the machinery may, in many cases, be of even more importance than reduction of coal expenditure. Although it is desirable that war-ships should be self-supporting for as long periods as possible, they are not often required to steam long distances at high speed, which is the normal condition of service in the Mercantile Marine, in which, therefore,

economy of coal consumption is, of necessity, the first consideration.

In most cases, however, we shall find that the same measures that have produced economy of fuel, have also enabled the space and weight required for the engines to be reduced at the same time, particularly at the beginning of the upward rise in the steam pressures. It is however possible that we may reach a point at which the increase in working pressure, though increasing the economy of fuel, will not permit of any reduction in the space and weight required for the machinery, so that the only gain in this direction would be that due to the reduced quantity of coal required to be carried. It is also conceivable that, by-and-by, a point may be reached at which further increase of pressure would not result practically in any reduction either in space or weight required for the machinery (including bunkers), or in the coal expenditure. I think, however, we are scarcely within measurable distance of that position yet.

The effect of the increase in the working steam pressure is to enable a given power to be obtained with a cylinder of smaller diameter, and thus to reduce the size and weight of the engines. The volumes of the steam-pipes and of the steam-spaces in the boilers may also be reduced, because the *relative volume* of the steam is decreased, that is to say, a given *weight* of steam at the higher pressures occupies less space than at the lower pressures. For example, at a pressure of 5 lbs. above the atmosphere 1 lb. of steam would occupy 19.6 cubic feet, at 20 lbs. pressure 11.6 cubic feet, at 60 lbs. 5.7 cubic feet, and at 100 lbs. pressure only 3.8 cubic feet. The boilers, steam-pipes, &c., can be further reduced, in consequence of the collateral advantage gained by the use of high pressure steam, viz., that *less weight* per I.H.P. is required, in consequence of the more economical working of the engines. All these parts may therefore be reduced not only from the decreased *relative volume* of the steam, but also because the total weight of steam required to be generated is likewise diminished.

So far, we have referred only to the reduction in size and weight due to the increase in the working pressures of steam. This has, however, been accompanied by a considerable increase in the speeds at which the pistons of marine engines are worked, which has tended still further to reduce the weight and dimensions of the machinery. This increase of speed has, of course, to some extent resulted from the use of higher pressures; but I think, it must be largely attributed to improvements in design, and in the details of practical workmanship. In the earlier engines it was not considered safe to work the pistons at a much higher speed than about 200 feet per minute. From an old machinery specification, dated 1845, I extract the following:

The speed of piston for

ft.	in.					
4	0	stroke is	not to	exceed	196	feet per minute.
4	6	"	"	"	204	"
5	0	"	"	"	210	"
5	6	"	"	"	216	"
6	0	"	"	"	222	"
6	6	"	"	"	226	"
7	0	"	"	"	231	"
7	6	"	"	"	236	"
8	0	"	"	"	240	"

In modern marine engines of large power the piston speeds are often as high as from 600 to 700 feet per minute, and every effort is being made, by the use of improved workmanship and appliances, to increase the piston speed, with safety, to as great an extent as possible.

The relations between the size, speed, and power of marine engines may, perhaps, be most clearly illustrated by giving a brief sketch of the changes that have been introduced from time to time. In this comparison I have confined attention to the progress made in ships of the Royal Navy, for which the information is more complete and available than for the Mercantile Marine; and this will probably possess the greater interest for the members of this Institution. I think, too, we may safely state that the machinery for ships of-war has, in all stages of the progress of steam navigation, represented the most perfect and complete type of marine engine of the day. It will, I think, be admitted, that the design of the machinery for ships of the Royal Navy has not only kept abreast of the times, but that, in many instances, it has taken the lead and initiated improvements which have considerably advanced marine engineering.

The information that can now be obtained about the earlier vessels is much less complete and exact than about the later vessels, for it is only within a comparatively recent period that the importance of keeping full and accurate records of all the particulars and performances of steam-vessels has been fully recognized. I have, however, endeavored to make the comparison as full and fair as possible, and although it must be considered to some extent incomplete and approximate, I think a few useful lessons may be learnt from it. The *size* of the engine has been represented by two particulars, viz., the cubic capacity of the cylinders, and the total weight of the machinery; the former giving a measure of the dimensions of the engines themselves, independent of the boilers and appliances.

The propeller used in all the earlier steamships was invariably the paddle-wheel, and the type of engine generally employed was that known as the *side-lever* engine, which may be regarded as the marine counterpart of the beam-engine so universally used at that time for land purposes. This engine possessed the advantages of having the pistons and rods, to a great extent, balanced by the pumps and rods and connecting-rod, so that the piston was nearly in equilibrum in all positions. The great length of connecting-rod was also favorable for the transmission of the power to the crank. The engine was, however, very heavy, and occupied much space for the power developed. The boilers that supplied steam to the earlier engines were those known technically as *flue boilers*, in which the heating surface consisted of the exterior surface of a winding flue that conveyed the products of combustion from the furnaces to the funnel. These boilers were excessively heavy and cumbrous, and suitable only for very low pressures of steam.

As an illustration of this type we will take the "Rhadamanthus," which ship was fitted with side-lever engines and flue boilers, by Messrs. Maudslay, in 1832. The nominal horse-power was 220, but the engines were capable of being worked up to about 400 I.H.P., or 1.8 times the nominal power. The load on the safety valves was 4 lbs. per square inch, and the speed of piston, at full power, 175 feet per minute. The cubical capacity of the cylinders was 168 cubic feet, so that only 2.38 I.H.P. were developed per cubic foot of cylinder. The total weight of the machinery was 275 tons, 1.45 I.H.P. being obtained per ton of weight.

The next step was the introduction of *tubular* boilers, in which a series of small tubes was substituted for the large winding flue; the boilers were thus made lighter and more compact, and the working pressures of steam were increased. Attempts were also made to reduce the space and weight required for the engines by the substitution of direct-acting for side-lever engines. There were many varieties of this type, one of the earliest being the well-known double-cylinder engine, fitted by Messrs. Maudslay, in the "Terrible" and several other vessels. This engine consisted of two cylinders side by side, the piston-rods from the two cylinders being attached to a single cross-head. In order to get sufficient length of connecting-rod, the crosshead was of peculiar form and passed down between the two cylinders, having a journal at its lower end, on which one end of the connecting-rod worked, the other end being attached to the crank-pin. The engines of the "Terrible" were completed in 1845, and were of 800 N.H.P. The I.H.P. was 1,905, or 2.38 times the nominal power. The pressure of steam in the boilers was 9 lbs. per square inch, the speed of piston 240 feet per minute, and the total weight of the machinery 607 tons. In these engines 2.1 I.H.P. were developed per cubic foot of cylinder, and 3.14 I.H.P. per ton of weight.

The simplest and most compact form of engine for paddle-wheels was attained by the introduction of the *oscillating engine*, which was adopted and perfected by the late eminent marine engineer, Mr. John Penn, with whose name this type is generally associated, though it was also used by other makers. In this engine the connecting-rod is dispensed with, and the piston-rod is connected directly to the crank-pin. The cylinders oscillate upon hollow axes or trunnions, through which the steam is admitted to, or exhausted from, the cylinders, so that the piston-rod may accommodate itself to the rotatory motion of the crank. As an example of this type we will take the

Ship.	…bic …pa- …ity.	Pistons. Speed per min.	Pistons. V'lume swept out per min'te.	Weight. Engines	Weight. Propellers and shafting.	Weight. Boilers and water.	Weight. Total.	Weight per I. H. P. Engines	Weight per I. H. P. Propellers and shafting.	Weight per I. H. P. Boilers and water.	Weight per I. H. P. Total.	I. H. P. per Cubic foot of cylinder	I. H. P. per Cubic foot swept out by piston.	I. H. P. per Ton of weight.
	…. ft.	feet.	cub. ft.	tons.	tons.	tons.	tons.	lbs.	lbs.	lbs.	lbs.			
"Rhadamanthus"…	…68	175	5,880	..	..	..	275	..	..	..	1,550	2.38	0.068	1.45
"Retribution"……	…04	218	23,504	..	..	..	700	..	..	..	980	1.77	0.068	2.3
"Gladiator"……	…93	178	12,174	165	48	144	357	361	105	315	781	2.6	0.084	2.87
"Terrible"……	…04	240	27,108	230	117	260	607	272	136	306	714	2.1	0.07	3.14
"Sphinx"……	…40	192	14,083	117	43	135	295	222	82	256	560	2.68	0.08	4.0
"Buzzard"……	…68	246	11,008	67	31	145	243	180	93	386	653	3.104	0.076	3.43
"Magicienne"……	…06	287	16,230	95	41	139	275	164	70	240	474	3.28	0.08	4.72
"Himalaya"……	…30	392	25,766	121	47	257	425	133	50	282	465	8.89	0.79	4.82
"Doris"……	…93	432	31,644	143	60	341	544	106	45	254	405	10.25	0.095	5.52
"Galatea"……	…68	410	30,023	139	65	350	554	102	48	257	407	11.38	0.101	5.51
"Immortalite"……	…20	385	24 200	135	50	244	429	127	47	228	402	10.86	0.098	5.57
"Revenge"……	…93	417	30,571	180	62	300	542	158	54	264	476	8.68	0.083	4.7
"Victoria"……	…69	467	43,120	230	76	416	722	117	39	212	368	11.93	0.102	6.1
"Undaunted"……	…20	406	25,520	119	47	225	391	118	46	223	387	10.27	0.088	5.78
"Valiant"……	…93	475	34,831	191	48	379	618	128	32	253	413	11.41	0.096	5,42
"Black Prince"……	…74	438	51,903	240	97	550	887	96	38	221	355	11.75	0.107	6.28
"Agincourt"……	…45	426	47,394	291	115	564	970	110	43	213	366	11.1	0.121	6.09
"Ocean"……	…02	464	46,646	215	78	447	740	113	41	236	390	10.55	0.09	5.73
"Northumberland"	…14	414	48,954	316	98	629	1,043	108	33	215	356	12.72	0.133	6.27
"Crocodile"……	…82	472	48,039	223	85	303	611	125	47	168	338	10.58	0.084	6.6
"Hercules"……	…84	639	97,128	445	99	547	1,091	117	26	144	287	12.47	0.088	7.8
"Inconstant"……	…74	596	70,594	347	88	550	985	106	26	167	299	15.53	0.104	7.5
"Active"……	…95	518	43,719	173	55	271	499	94	30	147	271	14.0	0.094	8.28
"Audacious" (twin s…	…68	451	58,285	229	61	345	635	106	28	160	294	12.46	0.08	7.6
"Swiftsure"……	…49	547	57,294	266	60	334	660	121	27	152	300	11.7	0 09	7.4
"Volage"……	…93	592	47,834	216	49	293	558	107	24	145	276	15.0	0.095	8.1
"Invincible" (twin s…	…69	490	55,394	258	59	354	671	112	25	153	290	15.26	0.09	7.7
"Vangaurd" "	…39	443	50,128	306	72	336	714	127	30	140	297	15.8	0.107	7.5
"Devastation" "	…55	499	69,801	334	92	456	882	113	31	154	298	14.6	0.09	7.5
"Thunderer" "	…53	542	70,173	370	101	504	975	132	33	180	345	13.9	0.089	6.4
"Raleigh"……	…90	665	72,496	336	88	527	951	122	32	192	346	12.55	0.08	6.5
"Amethyst"……	…13	542	28,184	130	27	162	319	126	26	157	309	16.15	0.082	7.24
"Alexandra"……	…17	528	93,324	497	167	656	1,320	131	44	173	348	12.02	0.09	6.4
"Dreadnought"……	…5	604	106,739	528	128	705	1,361	144	35	192	371	10.32	0.077	6.0
"Bacchante"……	…9	608	56,179	301	60	482	843	125	28	200	353	14.7	0.096	6.4
"Inflexible"……	…7	572	101,100	532	112	752	1,396	140	30	198	368	12.0	0.084	6.07
"Shannon"……	…5	524	41,291	242	68	334	644	153	43	211	407	11.2	0.086	5.48
"Temeraire"……	…7	604	84,577	426	113	664	1,203	124	33	193	350	14.3	0.091	6.4
"Euryalus"……	…0	568	53,960	353	65	483	901	150	28	206	384	13.8	0.097	5.82
"Iris"……	…8	582	71,411	382	65	573	1,020	111	19	166	296	20.96	0.108	7.56
"Nelson"……	…3	5	66,080	293	107	556	956	105	38	200	343	15.12	0.094	6.5
"Northampton"……	…0	546	52,098	404	82	588	1,074	150	31	219	400	19.38	0.115	5.5
"Carysfort"……	…3	588	26,294	127	31	214	372	124	31	208	363	18.7	0.087	6.2
"Cleopatra"……	…1.5	540	24,062	121	35	201	357	104	30	172	306	23.4	0.109	7.3
"Agamemnon"……	…0	559	53,329	433	105	598	1,136	161	40	223	424	19.3	0.112	5.28
"Polyphemus……	…5	780	34,866	183	46	209	438	75	18	85	178	38.0	0.158	12.56
"Satellite"(without …	…2	492	10,244	55	15	110	180	110	30	211	351	21.44	0.108	6.19
"Banterer"……	…9	378	4,788	27	6	41	74	134	30	203	367	23.8	0.094	6.1
First-class torpedo-b…	…2.4	876	2,102	4¼	¾	7¼	12¼	20.7	3.65	35.3	59.65	191.6	0.219	37.66

In preparing this …e machinery fitted in the earlier war-ships by their respective firms and also to Mr. J. Wright, C.B., …ion to make use of certain official records.

Ship.	Date.	Maker.	Description: Engines.	Description: Boilers.	Load per square inch on safety valves.	N. H. P.	I. H. P.	I. H. P. / N. H. P.	Revolutions per minute	Cylinders: No.	Cylinders: Diameter.	Cylinders: Stroke.	Cylinders: Cubic capacity.	Pistons: Speed per min.	Pistons: V'lume swept out per min'te.	Weight: Engines	Weight: Propellers and shafting.	Weight: Boilers and water.	Weight: Total.	Weight per I. H. P.: Engines.	Weight per I. H. P.: Propellers and shafting.	Weight per I. H. P.: Boilers and water.	Weight per I. H. P.: Total.	I. H. P. per Cubic foot of cylinder	I. H. P. per Cubic foot swept out by piston.	I. H. P. per Ton of weight.
					lbs.						ins.	ft. in.	cub. ft.	feet.	cub. ft.	tons.	tons.	tons.	tons.	lbs.	lbs.	lbs.	lbs.			
"Rhadamanthus"	1832	Maudslay	Side-lever	Flue	4	220	400	1.8	17.5	2	55½	5 0	168	175	5,880	..	..	..	275	..	..	..	1,550	2.38	0.068	1.45
"Retribution"	1842	"	4-cylinder, vertical	"	6	800	1,600	2	13	4	72	8 0	904	218	23,504	..	..	..	700	..	..	..	980	1.77	0.068	2.3
"Gladiator"	1845	Ravenhill	Vertical, direct	Tubular	11	430	1,023	2.38	15.5	2	79⅛	5 9	393	178	12,174	165	48	144	357	361	105	315	781	2.6	0.084	2.87
"Terrible"	1845	Maudslay	4-cylinder, vertical	"	9	800	1,905	2.38	15	4	72	8 0	904	240	27,108	230	117	260	607	272	136	306	714	2.1	0.07	3.14
"Sphinx"	1846	Penn	Oscillating	"	7	500	1,180	2.36	16	2	82	6 0	440	192	14,083	117	43	135	295	222	82	256	560	2.68	0.08	4.0
"Buzzard"	1850	Ravenhill	"	"	20	300	834	2.78	20.5	2	64	6 0	268	246	11,008	67	31	145	243	180	93	386	653	3.104	0.076	3.43
"Magicienne"	1850	Penn	"	"	14	400	1,300	3.25	20.5	2	72	7 0	396	287	16,230	95	41	139	275	164	70	240	474	3.28	0.08	4.72
"Himalaya"	1854	Penn	Trunk	Lamb's patent flue	15	700	2,046	2.92	56	2	=77⅝	3 6	230	392	25,766	121	47	257	425	133	50	282	465	8.89	0.79	4.82
"Doris"	1857	"	"	Rectangular, tubular	20	800	3,005	3.75	54	2	=82	4 0	293	432	31,644	143	60	341	544	106	45	254	405	10.25	0.095	5.52
"Galatea"	1859	"	"	" "	22	800	3,052	3.82	56	2	=82	3 8	268	410	30,023	139	65	350	554	102	48	257	407	11.38	0.101	5.51
"Immortalite"	1859	Maudslay	Return connecting-rod	" "	20	600	2,390	3.98	55	2	76	3 6	220	385	24 200	135	50	244	429	127	47	228	402	10.86	0.098	5.57
"Revenge"	1859	"	" "	" "	20	800	2,548	3.18	52	2	82	4 0	293	417	30,571	180	62	300	542	158	54	264	476	8.68	0.083	4.7
"Victoria"	1860	"	" "	" "	22	1,000	4,403	4.4	58.4	2	92	4 0	369	467	43,120	230	76	416	722	117	39	212	368	11.93	0.102	6.1
"Undaunted"	1861	Ravenhill	" "	" "	21.5	600	2,260	3.76	58	2	76	3 6	220	406	25,520	119	47	225	391	118	46	223	387	10.27	0.088	5.78
"Valiant"	1861	Maudslay	Return connecting-rod	" "	25	800	3,348	4.18	59	2	82	4 4	293	475	34,831	191	48	379	618	128	32	253	413	11.41	0.096	5.42
"Black Prince"	1861	Penn	Trunk	" "	24.5	1,250	5,571	4.45	54.75	2	=104¼	4 0	474	438	51,903	240	97	550	887	96	38	221	355	11.75	0.107	6.28
"Agincourt"	1864	Maudslay	Return connecting-rod	" "	25	1,350	5,913	4.38	53	2	101	4 0	445	426	47,394	291	115	564	970	110	43	213	366	11.1	0.121	6.09
"Ocean"	1864	"	" "	" "	22	1,000	4,243	4.24	58	2	96	4 0	402	464	46,646	215	78	447	740	113	41	236	390	10.55	0.09	5.73
"Northumberland"	1866	Penn	Trunk	" "	25	1,350	6,545	4.84	47.6	2	=104¼	9 0	514	414	48,954	316	98	629	1,043	108	33	215	356	12.72	0.133	6.27
"Crocodile"	1867	Humphrys	Horizontal, direct	" "	30	700	4,044	5.77	63	2	96	3 4	382	472	48,039	223	85	303	611	123	47	168	338	10.58	0.084	6.6
"Hercules"	1868	Penn	Trunk	" "	30	1,200	8,529	7.10	71	2	=118	4 6	684	639	97,128	445	99	547	1,091	117	26	144	287	12.47	0.088	7.8
"Inconstant"	1868	"	"	" "	30	..	7,364	..	74.5	2	=104¼	4 0	474	596	70,594	347	88	550	985	106	26	167	299	15.53	0.104	7.5
"Active"	1869	Humphrys	Horizontal, direct	" "	30	600	4,130	6.88	74	2	88	3 6	295	518	43,719	173	55	271	499	94	30	147	271	14.0	0.094	8.28
"Audacious" (twin screw)	1869	Ravenhill	Return connecting-rod	" "	30	800	4,834	6.04	75	4	77	3 0	388	451	58,285	229	61	345	635	106	28	160	294	12.46	0.08	7.6
"Swiftsure"	1869	Maudslay	" "	" "	30	800	4,913	6.14	68	2	98	4 0	439	547	57,294	266	60	334	660	121	27	152	300	11.7	0.09	7.4
"Volage"	1869	Penn	Trunk	" "	30	600	4,530	7.55	79	2	=86⅛	3 9	303	592	47,834	216	49	293	558	107	24	145	276	15.0	0.095	8.1
"Invincible" (twin screw)	1870	Napier	Return connecting-rod	" "	30	800	5,180	6.48	81.6	4	72	3 0	339	490	55,394	258	59	354	671	112	25	153	290	15.26	0.09	7.7
"Vangaurd" "	1870	Laird	" "	" "	30	800	5,384	6.73	73.9	4	72	3 0	339	443	50,128	306	72	336	714	127	30	140	297	15.8	0.107	7.5
"Devastation" "	1871	Penn	Trunk	" "	30	..	6,637	..	76.8	4	=80	3 3	455	499	69,801	334	92	456	882	113	31	154	298	14.6	0.09	7.5
"Thunderer" "	1872	Humphrys	Horizontal, direct	" "	30	..	6,271	..	77.5	4	77	3 6	453	542	70,173	370	101	504	975	132	33	180	345	13.9	0.089	6.4
"Raleigh"	1873	"	Return connecting-rod	" "	30	800	6,157	7.69	74	2	100	4 6	490	665	72,496	336	88	527	951	122	32	192	346	12.55	0.08	6.5
"Amethyst"	1872	Rennie	Horizontal, compound	High, cylindrical	60	..	2,310	..	98.5	2	1 No. 55½; 1 " 97¾	2 9	143	542	28,184	130	27	162	319	126	26	157	309	16.15	0.082	7.24
"Alexandra"	1875	Humphrys	Vertical, compound	" "	60	..	8,498	..	66	6	2 No. 70; 4 " 90	4 0	707	528	93,324	497	167	656	1,320	131	44	173	348	12.02	0.09	6.4
"Dreadnought"	1875	"	" "	" "	60	..	8,206	..	67.13	6	2 No. 66; 4 " 90	4 6	795	604	106,739	528	128	705	1,361	144	35	192	371	10.32	0.077	6.0
"Bacchante"	1876	Rennie	Horizontal, compound	" "	70	..	5,413	..	76	3	1 No. 73; 2 " 92	4 0	369	608	56,179	301	60	482	843	125	28	200	353	14.7	0.096	6.4
"Inflexible"	1876	Elder	Vertical, compound	" "	60	..	8,485	..	71.5	6	2 No. 70; 4 " 90	4 0	707	572	101,100	532	112	752	1,396	140	30	198	368	12.0	0.084	6.07
"Shannon"	1876	Laird	Horizontal, compound	" "	70	..	3,538	..	65.5	4	2 No. 44; 2 " 85	4 0	315	524	41,291	242	68	334	644	153	43	211	407	11.2	0.086	5.48
"Temeraire"	1876	Humphrys	Vertical, compound	" "	60	..	7,696	..	78.75	4	2 No. 70; 2 " 114	3 10	537	604	84,577	426	113	664	1,203	124	33	193	350	14.3	0.091	6.4
"Euryalus"	1877	Ravenhill	Horizontal, compound	" "	70	..	5,250	..	71	3	1 No. 73; 2 " 93½	4 0	380	568	53,960	353	65	483	901	150	28	206	381	13.8	0.097	5.82
"Iris"	1877	Maudslay	" "	" "	65	..	7,714	..	97	8	4 No. 41; 4 " 75	3 0	368	582	71,411	382	65	573	1,020	111	19	166	296	20.96	0.108	7.56
"Nelson"	1877	Elder	Vertical, compound	" "	60	..	6,246	..	86	4	2 No. 60; 2 " 104	3 6	413	5	66,080	293	107	556	956	105	38	200	343	15.12	0.094	6.5
"Northampton"	1877	Penn	Vertical, composite	" "	60	..	6,010	..	84	6	54	3 3	310	546	52,098	404	82	588	1,074	150	31	219	400	19.38	0.115	5.5
"Carysfort"	1878	Elder	Horizontal, compound	Low, cylindrical	60	..	2,300	..	107	3	1 No. 46; 2 " 64	2 9	123	588	26,294	127	31	214	372	124	31	208	363	18.7	0.087	6.2
"Cleopatra"	1878	Humphrys	" "	" "	60	..	2,611	..	107.9	4	2 No. 36; 2 " 64	2 6	111.5	540	24,062	121	35	201	357	104	30	172	306	23.4	0.109	7.3
"Agamemnon"	1880	Penn	Vertical, composite	High, cylindrical	60	..	6,000	..	86	6	54	3 3	310	559	53,329	433	105	598	1,136	161	40	223	424	19.3	0.112	5.28
"Polyphemus"	1882	Humphrys	Horizontal, compound	Locomotive cylindric'l	120	..	5,500	..	120	4	2 No. 38; 2 " 64	3 3	145	780	34,866	183	46	209	438	75	18	85	178	38.0	0.158	12.56
"Satellite" (without blast)	1882	"	" "	Low, cylindrical	90	..	1,115	..	98.5	2	1 No. 36; 1 " 62	2 6	52	492	10,244	55	15	110	180	110	30	211	351	21.44	0.108	6.19
"Banterer"	1880	Barrow Co.	" "	" "	60	..	452	..	126	2	1 No. 28; 1 " 48	1 6	19	378	4,788	27	6	41	74	134	30	203	367	23.8	0.094	6.1
First-class torpedo-boat	1880	Thornycroft	Vertical, compound	Locomotive, cylindric'l	120	..	460	..	438	2	1 No. 12¾; 1 " 20⅞	1 0	2.4	876	2,102	4¼	¾	7¼	12¼	20.7	3.65	35.3	59.65	191.6	0.219	37.66

In preparing this table I have to acknowledge my indebtedness to Messrs. Joshua Field and John Penn, who have kindly supplied me with particulars of the machinery fitted in the earlier war-ships by their respective firms and also to Mr. J. Wright, C.B., Engineer-in-Chief of the Navy, for much information respecting the later vessels and for permission to make use of certain official records.

"Magicienne," which ship was engined by Messrs. Penn in 1850. The pressure of steam in the boilers was 14 lbs. per square inch, piston speed 287 feet per minute, I.H.P. 1,300, and total weight of machinery 275 tons. In this engine 3.28 I.H.P. were developed per cubic foot of cylinder, and 4.72 I.H.P. per ton of weight.

The introduction of the screw-propeller for the propulsion of ships was the most important step in the progress of steam navigation. In order to obtain the same speed of ship, it was necessary to drive the screw-propeller at a much greater number of revolutions than the paddle-wheel. When the screw was first introduced, it was not considered practicable to drive the pistons at a sufficiently high rate of speed to enable the engine-shaft to be connected directly to the propeller shafting, and the earlier engines used for working screw-propellers were *geared*, so that the screw-shaft was caused to revolve more rapidly than the engine-shaft. A large spur-wheel keyed on the end of the crank-shaft of the engine worked into a pinion on the screw-propeller shafting, so that the speed of the engine-shaft might be multiplied on the screw-shaft as many times as might be required. The pressures of steam and speeds of piston employed with these *geared engines* were practically the same as those of the paddle-wheel engines that immediately preceded them, so that while it is interesting to note this step it is not necessary to discuss it further.

Soon after the introduction of the screw-propeller, improvements in workmanship, appliances, and mechanical details so far advanced, that the speeds, both of piston and of revolution, could be sufficiently increased to enable the crank-shaft to be coupled directly to the screw shafting. The boilers for these screw engines were made sufficiently strong to carry higher steam pressures, and the increase both of the pressure of steam used, and of the speed at which the pistons were worked, led to a very considerable increase in the power that could be obtained within a given weight and space, and gave a great impetus to the advance of marine engineering. It is quite certain that the very powerful engines which are now so general would have been altogether impossible, had not the screw-propeller superseded the paddle-wheel. During the period 1850-60, a number of wooden frigates and corvettes, fitted with horizontal screw engines, were added to the Navy. The engines were placed horizontally, in order to keep them below the level of the water-line, so as to be protected from the effect of shot and shell. The pressure of steam used was about 20 lbs. per square inch, and, in the majority of the better examples, the speed of piston was increased to about 400 feet per minnte. In a few cases, the speeds of piston were still higher than this; the "Doris," for example, of 3,000 I.H.P., having a piston speed of 432 feet per minute, and the "Victoria," of 4,400 I.H.P., a piston speed of 467 feet, per minute. The engines were all jet condensing, and very little expansion was carried out in the cylinders, so that the consumption of coal for the power obtained was high: this, however, only indirectly affects the point under consideration at present. As the average results of this type, we may take—

I.H.P. developed per cubic foot of cylinder	=10.0
I.H.P. developed per ton of weight	= 5.5

This will be seen to be a very considerable advance from the best examples of the slower moving paddle-wheel engines.

The engines fitted in the earlier ironclads were very similar in design to those just mentioned; but as they were of larger power, and the beam of the ship permitted a considerable increase in the length of stroke, the speeds of piston were somewhat higher, and the average results were rather better than those quoted above; as may be seen by reference to the table appended to this paper.

With the jet injection condensers fitted in the earlier ships, in which the steam was condensed by actual mixing with sea-water, the feed-water was practically as salt as the sea-water itself, and a pressure of from 20 to 25 lbs. per square inch was considered to be the highest that could be safely carried in marine boilers, in consequence of the danger that would result from scale accumulating on the heating surfaces. The general adoption of surface condensation, however, overcame this difficulty, by enabling the boilers to be fed with fresh water. In these condensers the steam is condensed by being brought in contact

with the cold surfaces of a series of small tubes, through, or around which, cold sea-water is kept circulating, by the agency of a pump. There is, therefore, no admixture with the sea-water, and the fear of overheating from incrustation on the heating surfaces of the boilers is thereby removed. Since the year 1860, this system has become universal for marine purposes, and has rendered high pressures for steam navigation practicable.

When the above system was first introduced, the old flat-sided boilers, made to fit the section of the ship, were still retained. The form of shell in these boilers is obviously unfit for high pressures, but they were strengthened by fitting additional stays, &c., to enable them to carry working steam pressures of 30 to 35 lbs. per square inch, and the great majority of the war-vessels built during the years 1860-70 were fitted with surface-condensing engines, worked with steam of this pressure. The piston speeds were also considerably increased, especially in the larger ships, in which a long stroke could be obtained. In fact, during this decade, and with this type of machinery, the speed of piston of marine engines reached a point which has only been exceeded in a few ships of recent construction. With this type of engine, the piston speeds varied from 500 to as high as 665 feet per minute. To promote economy of fuel, the cylinders were generally made very large to allow for a considerable amount of expansion at full power, and the boilers were fitted with superheaters, so that the reduction of weight was not so great as might have been anticipated from the augmentation of the piston speed. In the majority of these engines, between 13 and 14 I.H.P. were developed per cubic foot of cylinder, and about $7\frac{1}{2}$ I.H.P. per ton of weight. In some engines of this class fitted to several of the twin-screw iron-clads, the speed of piston barely reached 500 feet per minute, but the other results were as given above.

We now come to the ordinary type of compound engine which has been fitted to nearly all war-ships since 1870, and which may be considered as the general type of marine steam engine of the present day. In these engines the steam from the boilers is only admitted direct to a small cylinder, usually known as the high-pressure cylinder, and at the end of the stroke in that cylinder, instead of passing at once to the condenser, the steam enters one or more additional and larger cylinders, in which the expansion is completed; after which the steam passes to the condenser. The boilers are therefore only in direct communication with the high-pressure cylinders, and the condensers with the low-pressure cylinders.

The working steam pressure in the Royal Navy with this type of engine hitherto has been from 60 to 70 lbs. per square inch. The engines now under construction are designed to be worked with steam of 90 lbs. pressure. As pointed out in the earlier part of this paper the principal object aimed at in increasing the pressure of steam has been to increase the economy of working, and the change from the ordinary surface-condensing engine, with 30 lbs. steam pressure, to the compound engine with 60 lbs. pressure, resulted in a reduction of the coal consumption per I.H.P., of between 30 and 40 per cent.

This step, however, has not been accompanied by a corresponding increase in piston speed, and decrease in dimensions and weight required per I.H.P. In fact, it must be admitted that, although the present type of compound engine is lighter than simple expansion engines worked at the same steam pressure, and with an equal amount of expansion, would be, yet the machinery as a whole is generally heavier than that of the surface-condensing engines with flat-sided boilers, worked with steam of 30 lbs. pressure which immediately preceded them, and the piston speeds are certainly no higher.

The only advantage, therefore, in point of reduction of weight and space that has been gained, as yet, by the introduction of compound engines and high-pressure boilers has been the reduction of coal-bunker space required. This is most important, but it scarcely comes within the range of the present paper, though intimately connected with it.

I will endeavor to point out the causes of this apparent check in the reduction of weight, &c., and to indicate what appears to be the most probable direction in which advance in the future is likely to take place. As a matter of course in

such a case it is impossible to speak with any degree of confidence; all that can be done is to discuss the several points that push themselves forward and suggest the most reasonable and probable solution, so far as our present knowledge, experience, and judgment will guide us.

The first point that strikes us is the increased weight of the boilers. For example, compare the boilers of the "Devastation" with those of the "Nelson." The engines of both ships developed rather more than 6,000 I.H.P. The boilers of the "Devastation" are flat-sided, pressed to 30 lbs. per square inch, and, including water, weigh only 456 tons. The boilers of the "Nelson" are cylindrical, pressed to 60 lbs., and weigh 556 tons, including water. In other words, while the "Devastation's" boilers weigh 154 lbs. per I.H.P., the boilers of the "Nelson" weigh 200 lbs. per I.H.P., or 30 per cent. more. Again, compare the "Inflexible" with the "Hercules," both of which ships have engines of about the same power. The boilers of the "Hercules," loaded to 30 lbs. pressure, weigh 547 tons, or 144 lbs. per I.H.P., whilst the cylindrical boilers of the "Inflexible" weigh 752 tons, or 198 lbs. per I.H.P., which is an increase of $37\frac{1}{2}$ per cent.

This increase of weight may be to some extent attributed to the increased strength necessary to carry the higher steam pressure. This, however, is insufficient to account for all the increase, for, in consequence of the more economical working of the engines, less steam is required to be generated, so that the volumes of the boilers and the areas of heating and grate surface may be made less than in the low-pressure boilers.

The principal cause of the increased weight of the boilers appears to be due to the additional thickness of plate allowed to provide against the effects of corrosion. Many of the earlier boilers, fed with water from surface condensers, were so rapidly weakened by corrosion that they had to be renewed after having been at work for one commission only. The expense of opening out the ship to do this was so great that it was considered desirable to increase the thickness of the shell plates, to enable the boilers to be kept in the ship, without the necessity of removal, for at least two commissions.

By the introduction of steel plates and more improved systems of management, it is hoped that this difficulty will be to a great extent removed. Steel plates are more uniform in structure and much stronger than iron, so that the scantlings may be reduced for a given strength; and now that the mystery that appeared for some time to enshroud the subject of the corrosion of marine boilers has been dispelled, and the true causes of the action ascertained, it appears probable that a reduction in the *factor of safety* usually employed may be safely made. The great importance of this would result from the increase in the maximum working pressure of steam that could then be carried with the present type of marine boiler.

To illustrate this point, let us consider the case of a cylindrical boiler 10 feet in diameter, the shell of which is made of steel plates $\frac{3}{4}$ of an inch thick. The tensile strength of these plates is usually specified to be not less than 26, nor more than 30, tons per square inch. Many engineers are desirous of raising the lower limit of strength, and this may be possible before long; but for our present purpose we will take the lower limit of 26 tons, and estimate the difference in the pressures of steam that could be carried by allowing factors of safety of 8 and of 5 respectively. The strength of the joint has been taken as 0.75 of that of the solid plate.

If 8 be taken as the factor of safety, the maximum working stress allowed on the material would be one-eighth of the ultimate stress, or 7,280 lbs. per square inch. This would be produced by a working steam pressure of 68 lbs. per square inch. If, however, a factor of safety of 5 were considered to leave a sufficient margin of strength to provide for all contingencies, the maximum working stress on the material would be increased to 11,648 lbs. per square inch, which would permit a working steam-pressure of 109 lbs. per square inch to be carried. Further, if we suppose that after four or five years' work the plates were uniformly thinned by corrosion to the extent of $\frac{1}{8}$ of an inch, the factor of safety in the second case, if the original working pressure were retained, would still be 4.167, which is by many engineers considered ample; but if it were deemed

desirable to retain the original margin of safety, this could be done by reducing the working pressure to 91 lbs. per square inch.

In estimating the strength of a structure like that of the shell of a boiler there are few disturbing elements, and almost exact calculation can be applied. The strength of the material used may be considered uniform, and with proper supervision during manufacture, inferior workmanship may be prevented. The most uncertain element, hitherto, has been the effect of corrosion and wear and tear, and this has caused a high factor of safety to be generally employed. Now, however, that most of the difficulties attending the boiler-corrosion question have been overcome and the methods of reducing or preventing this action have been satisfactorily ascertained, we may hope that marine boilers may retain their original strength for much longer periods than was formerly the case, and it would therefore appear that the factor of safety used for the shells of the boilers may be reduced with advantage.

The criterion of the relative strengths of the several parts is the strength they respectively possess when the boiler is worn out and unfit for further work. It is the usual practice in the Government Dockyards to burst by water pressure, for the sake of experiment, one boiler out of each set condemned, and a mass of very valuable information as to the ultimate condition of the boilers is thereby obtained. I have had, in the course of my duty, to conduct many of these bursting experiments, and, so far as my experience goes, the weakest part has, in every case, proved to be the furnace or combustion chamber, and I think it is quite safe to say that while the present form and dimensions of furnaces and combustion chambers are retained—and there appears to be no tendency to increase the thickness of the plates in these parts—there is no necessity to use a higher factor of safety for the shells than 5. Even with this factor I believe that, when the boilers come to be worn out, the furnaces and combustion chambers will be found to be the weakest parts, notwithstanding the fact that they apparently had a much greater margin of strength than the shells, when new. It must not be forgotten, that in these parts the material is weakened to some *unknown* extent by the working and flanging at the fires during manufacture, and when the boilers are under steam, unequal and unknown strains are brought on the material by the expansion resulting from the heat of the furnaces. It is also probable that the material deteriorates from the alternate heating and cooling to which it is exposed; and corrosive action, if it occurred at all, would probably produce more effect on the heating surfaces than on the shells, which are kept at a much lower and more uniform temperature. The plates in these parts also are generally thinner than in the shells, so that the percentage of loss of strength for a given amount of corrosion would be the greater.

The present type of marine boiler is also a slow and wasteful generator of steam. Even when the draught is forced by means of the steam blast, not more than about 30 lbs. of coal can be burnt per square foot of fire-grate per hour; and only about one-half of this is utilized in evaporating the water. In many cases more than one-half of the heat that the coal is capable of evolving by complete combustion is wasted in various ways. Cylindrical boilers are even more slow and wasteful generators than the old flat-sided boilers.

In order to reduce the weight of the boiler, rapid combustion is necessary. The greater the quantity of coal that can be efficiently burned per square foot of fire-grate per hour, the smaller may the furnaces be made for a given power. In locomotive practice the rate of combustion of coal in ordinary work often reaches as high as from 80 to 100 lbs. per square foot of grate per hour, and in some cases it even exceeds this. This, combined with the smaller amount of water carried, has caused many marine engineers to look to this type as a means of reduction of weight. Mr. Thornycroft, in his fast torpedo-boats, was, I believe, the pioneer in this direction; and he forced the draught by closing the stokeholds and putting them under air-pressure. The air was blown into the stokeholds by means of a rotatory fan, and a pressure of air, equal to the weight of from 3 to 6 six inches of water, is easily maintained. In some experiments, made at Portsmouth, to ascertain the perform-

ance of the boiler of a first-class torpedo-boat, it was found that with an air pressure equal to 3 inches of water, 62 lbs. of coal could be burnt per square foot of grate per hour; and when the pressure was raised to 6 inches of water, the rate of combustion was increased to 96 lbs. per square foot of grate per hour.

The only ship of large size in which this plan has been adopted is the torpedo ram "Polyphemus." The machinery of this ship has been constructed by Messrs. Humphrys and Tennant, and every effort has been made to secure lightness. The engines are driven at a high speed, both of piston and of revolution. They are expected when working at full power to make about 120 revolutions per minute and to have a piston speed of about 780 feet per minute. This will give about 38 I.H.P. per cubic foot of cylinder, and $12\frac{1}{2}$ I.H.P. per ton of weight; which is a very great advance on anything yet attained with the ordinary marine engine; and the experiment is most interesting and instructive from both a scientific and practical point of view.

I do not, however, think the locomotive type of boile vill prove itself suitable for marine purposes generally; though it may be useful in some special cases. The water spaces are too confined for general work at sea, and it would, I think, be found impossible to keep the flat sides of the fire-boxes from bulging and becoming unsafe. The great diffi culty that has hitherto been experienced with these boilers, even on the trial trips, which have as yet been the only hard work to which they have been subjected in the Service is the leakage of the tubes at ths fire-box ends. This is equally true both of the torpedo-boats and of the "Polyphemus." The cause of this appears to be that the intense heat of the fire being so close to the tube-plate, causes it to expand and compress the ends of the tubes; so that when the fires are checked, the contraction of the tube-plate leaves the tubes slack in their holes. The leakage has shown itself in nearly every case, when the engines were eased after the full-power run, the forced draught being stopped, which reduced the temperature of the fire. The cold air, also, that enters the fire-door when it is opened, impinges directly on the hot tube-plate, without having to pass over such a length of fire as an ordinary marine boiler.

For the present working pressures of steam the ordinary type of marine boiler appears to be the most suitable; and probably little variation in its form need be made, so far as strength is concerned, for pressures up to about 150 lbs. per square inch. I doubt, however, if it would be wise to much exceed that pressure with the existing type of boiler. If pressures beyond this limit be arrived at, it will be, in my opinion, necessary to adopt some form of boiler built entirely of small tubes, to enable the steam to be generated with confidence and safety. I do not profess to indicate what type of *tubulous* boiler will prove most efficient. Those that have been tried hitherto have not given general satisfaction, but it is probable that the failures have been due more to defects in the details of construction or of management, than to causes inherent to, or inseparable from, the type of boiler. The Herreshoff coil-boiler has proved itself economical and efficient for small boats, and is the lightest type yet constructed for a given power. It is absolutely safe, but whether it is adaptable to larger powers has yet to be proved.

With the present type of marine boiler it is necessary that *artificial draught*, of some kind, should be employed for full-power working, in order to keep the dimensions within moderate limits. Until recently the steam blast was the only means used for forcing the fires. This, however, is a very wasteful way of getting steam, especially with surface-condensing engines. Other methods of forcing the draught are—

1. Fitting an exhausting fan in the funnel.
2. Blowing air into closed ashpits.
3. Blowing jets of air into the base of the funnel.
4. Blowing air into closed stokeholds.

The first plan is obviously unsuitable for ships, for the fan would require to be so large to allow all the products of combustion to pass through it, at a sufficiently high velocity, that the apparatus would be too cumbrous and unwieldy.

The blowing of air into closed ashpits is a very efficient plan, but has the objection that the pressure in the furnaces is greater than that in the stokeholds, so

that, unless care be taken when opening the fire doors, accidents are liable to occur. With artificial stoking this plan would probably be found to be both economical and efficient.

The blowing of jets of air into the base of the funnel has been tested by experiment in the French Service, and favorably reported on. It is also on trial in one or two ships belonging to the French Navy; but little is known, as yet, of its practical working and efficiency.

The fourth plan, viz., blowing air into closed stokeholds, which has been adopted from the torpedo-boats, has found most favor in the Royal Navy. The stokeholds of several of the more recent ships are being arranged and fitted, so that, when working at full power, they may be closed in and kept under air pressure by means of fans.

The "Satellite," now completing at Sheerness, is the first ship in which this system has been practically tested. This ship has two independent stokeholds, in each of which there are two boilers. During a three hours' trial made on the 11th instant, with the forward stokehold closed, and kept under an air pressure equal to about one inch of water, the rate of combustion of coal per square foot of grate was raised to 39.4 lbs. per hour, and the I.H.P. developed from the two boilers was 865, or 15.7 I.H.P. per square foot of fire-grate. The average number of revolutions made by the engines per minute was 95.38. On a previous trial made on 3d April, 1882, without artificial draught, the coal burnt per square foot of grate was 18.6 lbs. per hour, and the average I.H.P. developed with four boilers was 1,115. The effect of forcing the draught, in this case, enabled the power obtained from a given set of boilers to be increased from 558 to 865, or about 55 per cent.*

With respect to the engine itself, it is probable that a considerable reduction of weight could be made by the more extended application of steel in construction. Forged steel has for some time been largely used for crank and propeller shafting, piston and connecting rods, &c., and the weights of the shafts have been further reduced by making them hollow. Recently, steel castings have been used, in lieu of cast-iron, for several parts of the machinery. The pistons for the engines of the steel cruisers now under construction at Messrs. R. & J. Napier's, Glasgow, are made of cast steel, and their weight is only about one-half the weight necessary for cast-iron pistons of the same diameter. Mild steel castings of great strength and toughness, and free from blowholes, can now be made, and as the processes of manufacture are more fully developed, I think we may look forward to a considerable extension in the application of this material, which will much facilitate the reduction of weight of marine engines. The general use of *wrought* iron or steel framing for marine engines would be very costly, as it would involve a great expenditure for labor, and this system is only likely to be adopted in special cases. If, however, mild steel castings could be made, at moderate price, to supersede iron castings for the various parts, the extra expense due to increased workmanship would be avoided; and it is most probable that the material would be extensively used.

The engines of the "Nelson," designed by Mr. A. C. Kirk, now the head of the firm of Messrs. R. & J. Napier, of Glasgow, form one of the most complete examples of light wrought iron and steel framing properly and scientifically trussed and secured to the structure of the ship itself that has, as yet, been constructed.* The reduction in weight in this case is considerable, the engines only weighing 105 lbs. per I.H.P., while the average weight for engines of the same class, with ordinary cast-iron framing, is 140 lbs. per I.H.P., or 33 per cent. greater. Engines constructed in this manner are, however, necessarily very expensive in manufacture, as much additional labor is involved, and both the workmanship and material employed must be of the highest quality.

* The steam trials of the "Heroine," sister ship to the "Satellite," took place at Devonport on the 30th and 31st May, 1882. On the 30th May, a six hours' run was made with natural draught only, the average power developed with four boilers being 1,127 I.H.P. On the following day a three hours' trial was made with the two forward boilers, the fires being forced by the steam blast. There were four blast nozzles used in the funnel, each $\frac{7}{16}$ inch diameter, and the average power developed with the two boilers was 695 I.H.P. By the use of the steam jet, therefore, the power of the boilers was increased from 563 to 695, or about 23½ per cent.

* See Journal, vol. xxiii, No. CI, page 614, *et seq.*

It is probable that a saving in weight might be effected if the framing of the engine and of the ship at the section in which the machinery is placed were considered, so far as possible, as one. As a rule the ship is only regarded as a platform to carry the machinery, and the necessary transverse strength is obtained by increasing the weight of the hull, without reference to any strength that might be obtained from the engines. In an able paper by Messrs. Read & Jenkins, of "Llyod's Register," "On the Transverse Strains of Iron Merchant Ships," read at the recent meetings of the Institution of Naval Architects, the necessity of providing additional transverse strengthening in the engine and boiler space in steam-vessels is clearly pointed out. It would, therefore, be an advantage in the design of the engines, particularly when they are required for warships, in which reduction of weight is so important, if the framing could be so arranged and constructed that it would add the necessary additional transverse strength to the section of the ship, instead of being merely a dead weight to be carried by the ship.

This point was emphasized by Mr. F. C. Marshall, of Newcastle, in his paper "On the Marine Engine," read at the meetings of the Institution of Mechanical Engineers, in August, 1881. He says: "Great saving in weight can be effected by careful design, and by judicious selection and adaptation of materials; also by the substitution of trussed framing and a proper mode of securing the engine to the structure of the vessel, in place of the massive cast-iron bedplates and columns of the ordinary engines of commerce." Also: "The hull and engine should be as much as possible one structure; rigidity in one place and elasticity in others is the cause of most of the accidents so costly to the shipowner. Under such conditions mass and solidity cease to be virtues, and the sooner their place is taken by careful design, and the use of the smallest weight of material (of the very best kind for the purpose) consistent with thorough efficiency, the better for all concerned."

The reduction in the weight of the engine that could be effected by improved design and workmanship, and the use of stronger material, although most important, does not, of itself, offer so large a scope for improvement as increase of speed, both of piston and of revolution. These have been highly developed by Mr. Thornycroft in the engines of the fast torpedo-boats, which at full power make about 440 revolutions per minute and have a piston speed of 880 feet per minute. The total weight of the machinery, including boilers and water, is below 60 lbs. per I.H.P. In the "Inflexible" the number of revolutions per minute was 71.5, speed of piston 572 feet per minute, and the total weight of the machinery 358 lbs. per I.H.P. Even in the "Polyphemus," in which the nearest approach to the torpedo type of machinery has been attempted, the estimated number of revolutions per minute is 120, speed of piston 780 feet per minute, and total weight of machinery 178 lbs. per I.H.P.

In pursuing this part of the subject we are met with many difficulties, arising, more especially, from the action of the propeller. The generally accepted theory of propulsion is, that the propeller produces a sternward momentum in the water on which it acts, which momentum measures the thrust exerted on the ship. The larger the quantity of water acted on, therefore, the greater will be the thrust. The usual practice has consequently been to make the diameter of the propeller as large as possible. The surface and edgewise friction of these large propellers is very great, and the large mass of water acted on in each revolution entirely precludes the engines being worked at a high speed of revolution, while they are connected direct to the present type of propeller.

From experiments made by the late Mr. Froude, it would appear that considerably more than one-half of the total energy transmitted to the propeller is wasted from various causes. It is, therefore, probable that the design of improved propelling arrangements affords a large field for invention, from which increased economy of propulsion may be reasonably anticipated in future. A large proportion of the loss of efficiency in existing screws arises from the augmented resistance of the ship from the reduction of the pressure of water under the stern produced by the propeller, so that it is very probable that improved propulsion

may result, as much from some alteration in the form of ship or in the position of propeller, as from modification of the form and arrangement of the propeller and fittings.

The substitution of steel blades in lieu of gun-metal or cast-iron, by offering less resistance, will enable the engines to be driven somewhat faster; but a more radical change than this needed to effect any very substantial reduction in weight and space occupied. It is probable that propellers of smaller diameters, driven at higher speeds, may be used with advantage in many cases. This was clearly shown in the "Iris," in which ship, by reducing the diameter of the screws from 18′ 6½″ to 16′ 3½″, the speed of the ship was increased from 16.577 knots to 18.573 knots; the I.H.P. developed by the engines being practically the same in the two trials. It would appear to be very desirable to make further experiments in this direction, especially with twin-screws, to ascertain how far reduction of diameter and increase of speed of revolution may be efficiently carried.

Mr. Thornycroft, in 1879, patented a new form of propeller, by means of which the propelling effect of a screw of given diameter is much increased. The boss is made small at the forward and large at the after end; and behind the propeller is arranged a body of peculiar form furnished with guide plates or blades, for directing the water projected by the propeller. Around the propeller and body a tube case or hollow guide is fitted, which facilitates the flow of water to the propeller, and regulates its discharge sternwards, after having been operated on by the propeller.

Mr. Thornycroft, in his reply on the discussion which followed his paper on torpedo-boats, read at the Institution of Civil Engineers, in May, 1881, stated that with his propeller, the diameter of the guide tube being 3 feet, 400 I.H.P. was utilized to the best advantage at a speed of 18 knots. Gunboats whose engines develop about 400 I.H.P. have screws generally about 9 feet in diameter. Mr. Thornycroft also calculated that two propellers, on his principle, similar to the model for the "Iris," each 7 feet in diameter, would be sufficient to use 45,000 I.H.P. at 40 knots. Of course, in these cases, the high speed of the ships would allow a full supply of water to the screws, and the statement is not altogether applicable to ships of ordinary form and speed; but it does appear to be probable that by some alteration in the form of ship or propeller, or of both, we may hope to obtain a much greater efficiency of propulsion from propellers of small diameter driven at high speeds. It is in this direction, I think, that a great scope for improvement exists. This, however, cannot be determined theoretically, and little can be done, until by a careful and extended series of experiments more definite knowledge is obtained of the whole of the conditions of the action and efficiency of propellers. It is very probable that we all have very much to learn, and possibly to unlearn, on this subject.

For the present we shall have to continue to use, with probably slight modifications in size and form, the existing screw-propeller, driven at a comparatively slow speed. It is therefore, I think, worthy of consideration whether or not it would be wise to proceed in the direction of making any material increase in the speed of engines, to reduce their dimensions and weight, until a form of propeller adapted for high speeds of revolution has been devised. If high speed engines were used for driving the existing propellers, it would be necessary to interpose gearing to reduce the rate of revolution. This is much objected to by many engineers, but it is possible that the advantages that would be gained thereby would outweigh its disadvantages.

It is only the reversal of the process that took place when the screw-propeller was first introduced. At that time there were no engines suitable to drive it direct, and the old type had to be utilized to drive it by means of multiple gearing. This, however, gave the screw the chance of proving its efficiency, and mechanical science soon produced engines capable of driving it direct. Improvements in the screw-propeller at present appear to have almost arrived at a standstill, so far as speed is concerned, but if gearing be admitted, the weight and space required for the engines could be materially decreased. In this case the gearing would be used to reduce the speed, and might be expected to work much more smoothly

than when the reverse operation had to be performed. If the plan proved successful, we should have the satisfaction of knowing that if a high speed propeller were devised, the engines would be prepared and ready to drive it direct.

The engines of a first-class torpedo-boat, which weigh only $4\frac{1}{4}$ tons, develop 460 I.H.P., with a speed of 438 revolutions per minute. This is about the same power that is developed by the engines of the gunboats in Her Majesty's Service. The engines of these boats, however, neglecting the boilers and propellers, weigh 27 tons, or more than six times the weight of the engines of the torpedo-boat which develop the same power. In the torpedo-boats the cylinders are $12\frac{3}{4}$ inches and $20\frac{1}{8}$ inches diameter, with a 12-inch stroke; the corresponding dimensions for the gunboat engines are—cylinders, 28 inches and 48 inches diameter, and stroke 18 inches. The speed of piston in the former case is 876 feet per minute, while in the latter it is only 378 feet. It is therefore clear, that if the gunboat engines could, with safety, be replaced by those of the torpedo-boat type, and the propeller shafting driven by means of gearing, a considerable saving of weight and space might be effected.

How far this system would be generally applicable it is difficult, if not impossible, to predict. One thing is certain, viz., that in order to enable the weights to be much further reduced, the speed must be increased. The only way of obtaining this increase of speed of the engines, with ths existing form and dimensions of propeller, is to drive the propeller shafting by means of gearing, so as to reduce the speed of revolution. Whether, or not, this is desirable, and likely to increase efficiency, can only be determined by experience. The point is, in my opinion, worthy of consideration, so that its relative advantages and disadvantages may be fully discussed and thoroughly threshed out.

In order to drive engines at high speeds of revolution it is desirable that the resultant driving forces on the crank should be made to be as nearly as possible uniform. To effect this, the *weights of the reciprocating parts* of the engines should be carefully adjusted to suit the required maximum speed, so as to cause the resulting tangential pressures on the crank-pin to vary as little as possible. It is very important that this should be carefully attended to in all high speed engines. The ordinates of the indicator diagram only give the pressures on the piston; and to obtain the corresponding pressures on the crank-pin, it is necessary to combine, with the indicator diagram, a diagram showing the work expended on the acceleration and given out during the retardation of the motion of the reciprocating parts of the engines. It is therefore clear that *strength* is not the only point that should be considered in the design of these parts; but that, if possible, their *weight* also should be so arranged that, when the engines are working at full speed, the effect of the inertia of the reciprocating parts may tend to produce uniformity in the tangential forces acting on the crank-pin. I am unable to do more than simply mention this point; which, however, is one that may make all the difference between a smoothly and quietly working engine and one whose motion is irregular.

One other point requires to be mentioned with reference to the probable increase in the working pressures of steam, which, although it primarily affects the economy of steam, is also important as regards the strains on the framing and shafting, &c. The ordinary two-cylinder compound engine, with initial steam pressure of 80 to 90 lbs. per square inch, has now practically arrived at the same position, with respect to range of expansion and temperature in each cylinder, that the simple expansion engine, with steam of 30 to 40 lbs. pressure, was in, when it was superseded by the compound engine. If, therefore, the pressures are increased beyond this limit, it will be necessary to divide the expansion of the steam into three stages in order to increase its practical efficiency, and to reduce the maximum strains on the shafting, framing, &c. This has been carried out in a few ships, one of the most recent being the steamship "Aberdeen," built by Messrs. R. & J. Napier, of Glasgow, and fitted with triple expansion engines, designed to be worked with steam at a pressure of 125 lbs. per square inch.

I cannot hope that I have done more, in this paper, than to have merely given a rapid and imperfect review of the

changes that have taken place during the past 50 years in the relations between the size, speed, and power of marine engines, and to have indicated, so far as our present knowledge and experience serve as guides, what appears to be the most probable direction for future advance.

I think it will be admitted that the progress already made is great. To say nothing of the more special types of marine engines, which may perhaps be considered to some extent as experimental, we may just compare the "Rhadamanthus," mentioned in the earlier part of this paper, the engines of which ship were built in 1832, with the "Cleopatra," built in 1878. The machinery of the "Rhadamanthus" weighed 275 tons and developed 400 I.H.P. The machinery of the "Cleopatra," which weighs 357 tons, developed at full power 2,611 I.H.P. Machinery of the "Cleopatra" type, of the same weight as that of the "Rhadamanthus," would be capable of developing 2,011 I.H.P., or five times the power of the "Rhadamanthus." It would appear from the recent trials of the "Satellite" that, with closed stokeholds under very moderate air pressure, machinery of modern design and construction would develop at least six times as much power as the earlier types of engines of equal weight.

One other feature is the great increase in the total engine power that can now be made available for the propulsion of ships. For example, take the case of the "Terrible," which represented the finest type of steam war-ship of her day. Her maximum I.H.P. was less than 2,000, and her speed about 10 knots. In the despatch vessel "Iris," two sets of engines are fitted, capable of developing 7,700 I.H.P., and of driving the ship at a speed of 18½ knots per hour. In several ships recently built, engines capable of developing 10,000 I.H.P. have been fitted.

So far as we are able to judge, the "Terrible" was as near finality in 1845 as the "Iris" and "Inflexible" are to-day. I think, therefore, we need not despair of the future, but may confidently look forward to still further progress in marine engineering. In what direction advance will be made it is, perhaps, unsafe to predict, but it does appear probable that the fields which afford the most enlarged scope for radical changes are the boilers and the propellers.

THE INSPECTION OF PUBLIC WORKS.

By PROF. GEORGE L. VOSE.

Proceedings of the Society of Arts.

Nearly all the disasters which occur from the breaking down of bridges are caused by defects which would be easily detected by an efficient system of inspection. Not less than forty bridges fall every year in the United States. No system of public inspection or control at present existing has been able to detect, in advance, the defects in these structures, nor to prevent the disasters. After a defective bridge falls, it is in nearly every case easy to see why it did so. It would be just about as easy, in most cases, to tell in advance that such a structure would fall if it happened to be heavily loaded. Hundreds of bridges are to-day standing in this country simply because they never happened to have received the load which is any time liable to come upon them

In a country where government controls all matters on which the public safety depends, and where no bridge over which the public is to pass is allowed to be built except after the plans have been approved by competent authority, where no work can be executed except under the rigid inspection of the best experts, nor opened to the public until it has been officially tested and accepted, it makes little or no difference whether the public is informed or not upon these matters; but in a country like the United States, where any man may at any time open a shop for the manufacture of bridges, whether he knows anything about the matter or not, and is at liberty to use cheap and insufficient materials, and where public officers are always to be found ready to buy such bridges, simply

because the first cost is low, and to place them in the public ways, it makes a good deal of difference.

To see what may be accomplished by an efficient system of public inspection, it is necessary to know something in regard to the structures to be inspected. We have now in common use in this country, both upon our roads and our railroads, bridges made entirely of iron, bridges of wood and iron combined, and occasionally, though not often nowadays, a bridge entirely of wood, and these structures are to be seen of a great variety of patterns, of all sizes, and in every stage of preservation. Of late so great has been the demand for bridge-work that this branch of engineering has become a trade by itself, and we find immense works, fitted up with an endless variety of the most admirably adapted machine tools devoted exclusively to the making of bridges of wood, iron, steel, or all combined. As in all divisions of labor the result of this specialization has been to improve the product, to lessen the cost, and to increase the demand, until many of our large firms reckon the length of bridging they have erected by miles instead of feet. As usual, however, in such cases, unprincipled adventurers are not wanting who, taking advantage of a great demand, do not hesitate to fit up cheap shops, to buy poor materials, and to flood the market with a class of bridges made with a single object in view, viz., to sell, relying for custom upon the ignorance, or something worse, of public officials. Not a year passes in which some of these wretched traps do not tumble down, and cause a greater or less loss of life, and, at the same time, with uninformed people, throw discredit on the whole modern system of bridge-building.

An impression exists in the minds of many persons that it is purely a matter of opinion whether a bridge is safe or not. In very many cases, however, perhaps in most, it is not at all a matter of opinion, but a matter of fact, and of arithmetic. The whole question always comes to this: Is the material in this bridge of good quality, is there enough of it, is it correctly disposed, and properly put together? With given dimensions, and knowing the load to be carried, it is a matter of the very simplest computation to fix the size of each member. We know what one square inch of iron will hold, and we know also the total number of pounds to be sustained, and it is no matter of opinion, but one of simple division, how many times one will go into the other.

In 1875 the American Society of Civil Engineers, in view of the repeated bridge disasters in this country, appointed a committee to report upon "The Means of Averting Bridge Accidents."

The conclusions arrived at by this committee are of great weight, and their tables of the loads which highway and railroad bridges should be capable of carrying [these were here given and explained by the speaker] are the result of valuable experience.

To pass now to railroad bridges, we find here a very heavy load coming upon the structure in a sudden and often very violent manner. Experiment and observation both indicate that a rapidly moving load produces an effect equal to double the same load at rest. This effect is seen much more upon short bridges, where the moving load is large in proportion to the weight of the bridge, than upon long spans, where the weight of the bridge itself is considerable. The acttal load upon a short bridge is also much more per foot than upon a long one, because the locomotive, which is much heavier than an equal length of cars, may cover the whole of a short span, but only a part of a longer one. [Tables of the load per running foot for railroad bridges were here given and discussed.]

The load which any bridge will be required to carry being determined, and the general plan and dimensions fixed, the several strains on the different members follow by a simple process of arithmetic, leaving to be determined the actual dimensions of the various parts.

It will, of course, be understood when it is said that bridge-building may be called a science that it can only be so when in the hands of an engineer whose judgment has been matured by wide experience, and who understands that no mechanical philosophy can be applied to practice which is not subject to the contingencies of workmanship. There are many bridges which will stand the test of figures very well, which are, nevertheless, very poor structures. The general

plan of a bridge may be good, the computations all right, and yet it may break down under the first train that passes over it. There are many practical considerations that cannot, at any rate have not yet, been reduced to figures. It is not enough that the strain upon each member of a bridge should be correctly estimated, and fall within the safe limits; the different members of the bridge must be so connected, and the mechanical details such as to insure, under all conditions, the assumed action of the several parts. In fine, while we can say that a bridge that does not stand the test of arithmetic is a bad bridge, we cannot always say that a structure which does stand such a test is a good one.

We often hear it argued that a bridge must be safe since it has been submitted to a heavy load and did not break down. Such a test means absolutely nothing. It does not even show that the bridge will bear the same load again, much less does it show that it has the proper margin for safety. It simply shows that it did not break down at that time. Every rotten, worn-out and defective bridge that ever fell has been submitted to exactly that test. More than this, it has repeatedly happened that a heavy train has passed over a bridge in apparent safety, while a much lighter one, passing directly afterwards has gone through. In almost all such cases the structure has been weak and defective, and finally some heavy load passes over and cripples the bridge, so that the next load produces a disaster.

In view of the preceding, what shall we say of a bridge company that deliberately builds a bridge in the middle of a large town, where it will be subjected to heavy teaming, and, owing to its peculiar location, to many crowds, and warrants to the town that it shall safely hold a ton per running foot, when the very simplest computation shows beyond chance of dispute that such a load will strain the iron to 40,000 pounds per square inch. We are to say either that such a company is so ignorant that it does not know the difference between a good bridge and a bad one, or else so wicked as to knowingly subject the public to a wretchedly unsafe bridge. The case referred to is not an imaginary one, but existed recently in the main street of a large New England town. The joints in that bridge which could safely hold but 20,000 pounds were required to hold 60,000 pounds under the load which the builders had warranted the bridge to carry safely. The case was so bad that, after a lengthy controversy, the town officers had a thorough expert examination of the bridge, which promptly condemned it as in imminent danger of falling and as having a factor of safety of only 1.15, which is practically no factor at all. Notwithstanding all this, and in the face of the report, the president of the bridge company came out with a letter in the papers, in which he pronounced the bridge "perfectly safe." Thus we have actually the president of a bridge company in this country stating openly that a factor of safety of 1.15 makes a bridge perfectly safe; or, in other words, that a bridge can safely bear the load that will break it down, for he very wisely made not the slightest attempt to disprove any of the conclusions of the commission. And this company has built hundreds of highway bridges all over the United States, and is building others to-day wherever it can find town or county officers ignorant enough to buy them. It might be supposed that under the above condemnation the authorities controlling the bridge would have taken some steps to prevent the coming disaster. They did, however, nothing of the kind, but allowed the public to travel over it for more than a year, at the most fearful risk, until public indignation became so strong that a special town meeting was called, and a committee appointed to remove the old bridge and build a new one. One of the worst cases of utterly dishonest bridge-building that we have heard, of late years, in Massachusetts, is that of the iron highway bridge across the Merrimac, at Groveland, a few miles below Haverhill, one span of which broke down last January. Can we do anything to prevent towns and counties from being imposed upon by dishonest builders? We certainly can, if those who control these matters care enough about it to do it. By the employment of a competent engineer to make the specifications, of a lawyer to draw up the contracts, and by having all materials and workmanship submitted to the engineer or inspector

before acceptance, an iron or other bridge may be had which will be absolutely safe.

One point always brought forward when an iron bridge breaks down is the supposed deterioration of iron under repeated straining, and we are gravely told that, after a while, all iron loses its fiber and becomes crystalline. This is one of the mysteries which some persons conjure up at tolerably regular intervals to cover their ignorance. It is perfectly well known with engineers, the world over, that with good iron nothing of the kind ever occurs. We have only to allow the proper margin for safety, as our first-class builders all do, and this antiquated objection at once vanishes. The examples of the long duration of iron in large bridges are numerous and conclusive.

The question is frequently asked: Does not extreme cold weaken iron bridges? To this it may be replied that no iron bridge made by a reliable company has ever shown the slightest indications of anything of the kind, though they have been used for many years in Russia, Norway, Sweden, and Canada, and nothing that we know in regard to iron gives us any reason to suppose that anything of the kind will ever happen. But here, again, everything turns upon the quality of the iron. Iron containing phosphorus is "cold short," or brittle when cold, and will break quicker under repeated and sudden shocks in cold weather than when it is warm. An immense number of experiments made upon all sorts of iron shows conclusively that cold has no effect whatever upon the strength of good iron. The securing of such iron is a matter to which the utmost attention is paid by our first-class bridge-building firms.

At least half the most disastrous failures of railroad bridges in the United States have been due to a defective system of flooring. With a very large proportion of our bridges the failure of a rail, the breaking of an axle, or anything which wlll throw the train from the track is almost sure to be followed by the breaking down of the bridge. The cross-ties are in many cases very short, and the floor is proportioned for a train *on* and not *off* the rails. When an engine on such a floor leaves the track it plunges off the ends of the cross-ties into the open space between the stringers and the chords, and generally wrecks the bridge. To prevent this the cross-ties should be long and well supported, and placed so close that a derailed engine cannot cut through them. The track should also be provided with guard timbers, well fastened, and the width between the trusses should be so great that the wheels of a derailed train will be stopped by the guard rail before the side of the widest car can strike the truss.

The importance of a substantial floor system has been very fully recognized by the Railroad Commissioners of Massachusetts, who have recently issued a very suggestive circular, accompanied by numerous examples of track construction for railway bridges. If this circular receives proper attention it is sure to produce good results.

Another point which has often been neglected is making sufficient provision to resist the force of the wind. A tornado, such as is not uncommon in this country, will exert a force of forty pounds per square foot, which, upon the side of a wooden bridge say of 200 feet span and 25 feet high, and boarded up as many bridges are, would amount to a lateral thrust of no less than 100 tons, and this load would be applied in the worst possible manner, viz., in a series of shocks. There have been many cases in this country where bridges have been blown down, and a case recently occurred where an iron railroad bridge of 180 feet span and 30 feet high, and presenting apparently almost no surface to the wind, was blown so much out of line that the track had to be shifted. The recent terrible disaster at the Frith of Tay was no doubt due to this cause.

Having seen something of the structures which require inspecting, let us now see what kind of inspection we have in this country, and the result of it, and let us also see the inspection which we might have, and the results which might be produced.

Looking first at railroad bridges, it might be supposed that no one could be so much interested in keeping such structures in good order as the companies which own those bridges, and which have the bills to pay in case of disaster. This is of course so, but in

spite of the fact the Ashtabula bridge broke down on one of the best managed lines in the country, and cost the company over half a million dollars in damages.

During the past ten years over two hundred railroad brdges in the United States have broken down. These bridges were all kept under such inspection as the railroad companies owning them considered sufficient, or such as they could afford; but either the supervision was defective, or the companies knowingly continued the use of unsafe biidges, and this fault has by no means been confined to the smaller and poorer roads. It would seem, therefore, that inspection by the companies themselves has not been sufficient. It certainly has not been enough to prevent two hundred disasters in ten years. It is the custom in several of the United States to maintain what is termed a Railroad Commission. Except in Massachusetts, where the State has taken care to secure men of ability for Railroad Commissioners, it is very doubtful whether these commissioners have been of any use. In many States it is very certain that, in regard to matters of inspection, the work of these boards has been simply a farce: and it could hardly be otherwise in a State which pays its commissioners only $1,000 salary, or worse yet, as in some cases, only $500. Add to this that in many cases the appointments have been purely political ones, and we can see the absurdity of expecting any results of value. We should hardly suppose that three men, in many cases entirely unaquainted with mechanical matters, could, by riding over a railroad once or twice a year, occasionally getting out to examine the paint on the outside of the boards which conceal a truss from view, judge very correctly of the elastic limit of the iron rods which they have never seen, and of which they do not even know the existence. For ample proof of the utter inefficiency of the present system we have only to compare the reports of the Railroad Commissioners in almost any State with the actual condition of the structures described.

While in a few States the inspection is nat quite so bad as that referred to, as a general thing it is no better, and we have no right to expect anything better under the present system. The State inspection which we have had throughout this country has not prevented the breaking down of one hundred bridges in the past ten years. Twenty States have railroad commissioners, but in nine of them the commission consists of only a single man, who in some cases is paid five hundred dollars a year. A State can pay five hundred dollars a year for having its bridges inspected, and it can get such service as never did and never will prevent a disaster, or it can pay a good price for competent inspection which will be worth ten times the money to the State. The money which the Lake Shore Railroad paid in damages for the Ashtabula disaster alone would have employed permanently six men at five thousand dollars a year each, and a hundred lives would have been saved besides.

With regard to highway bridges we are, if possible, even worse off than in regard to railway bridges, for in the case of such structures neither the owners nor the State make any pretence at inspection. It is impossible to say how many highway bridges have broken down during the past ten years, but it is estimated by bridge-builders that the number cannot be less than two hundred. This is about one a year for every two States, and is no doubt far within the truth. It is quite as important that highway bridges should be built and kept under some kind of control as that railroad bridges should, perhaps even more so, as towns and counties are much more liable to be imposed upon by dishonest bridge-builders than railroad companies are.

Admitting now that structures so important to the public safety as bridges both upon roads and railroads ought to be kept under rigid inspection and control, and that no system at present existing has been able to prevent the most fearful catastrophes, what shall we do?

Directly after the Ashtabula disaster, the Ohio legislative committee appointed to investigate that affair presented to the Legislature a bill "to secure greater safety for public travel over bridges," in which were plainly specified the loads for which all bridges should be proportioned, the maximum strain to which the iron should be subjected, and a method

for inspecting the plans of all bridges before building, and the bridges themselves during and after construction. The Governor with the consent of the senate, was to appoint the inspector for a term of five years, at a salary not exceeding $3,000 a year. Such inspector to pass a satisfactory examination before a committee of the American Society of Civil Engineers, themselves practised experts in bridge construction, and he was also to take a suitable oath for the faithful performance of his duty. This bill never became a law. An appropriation was made for a short time to pay for certain examinations, and there the matter stopped.

The committee of the American Society of Engineers was not agreed upon this matter. Messrs. James B. Eads and Charles Shaler Smith suggested the appointment in each State of an expert to whom all plans should be submitted, and by whom all work should be inspected, such expert to have been examined and approved by the American Society of Civil Engineers. The inspector was also to visit the scene of any accident, so called, and to ascertain, as far as possible, the cause. Messrs. T. C. Clarke and Julius W. Adams believed that in the present state of public opinion the above method would be impracticable, and feared that if inspectors were appointed it would be by political influence, and that the result would be worse than at present, as the inspectors would be inefficient, and yet to a great extent would relieve the owners of bad bridges from legal responsibility. They held that the best that could be done would be to provide means, in case of disaster, to fix plainly the responsibility, and recommended, first, that the standard for strength fixed by the society should be the legal standard, and in case it should be found that any bridge was of less strength than this, it should be taken as *prima facie* evidence of neglect on the part of the owners; second, that no bridge should be opened to the public until a plan, giving all dimensions, strains and loads, sworn to by the designers and makers, and attested by the corporation having control of it, had been deposited with the American Society; and, further, that the principal pieces of iron in the bridge should be stamped with the name of the maker, place of manufacture and date. Messrs. A. P. Bollen and Charles Macdonald looked rather toward effecting the desired result by so directing public sentiment by keeping the correct standard for bridges before it, that it would eventually compel the passage of the necessary laws.

Whether it is possible, in this country, to make an appointment dependent purely upon honesty and capacity, and free from political influence, may well be doubted. The examination before the expert committee of the American Society of Engineers would seem to be an excellent idea, and would be pretty sure to keep the number of applicants down to a pretty low figure. In case such a plan was found feasible let the State appoint a single person as inspector of roads and bridges, or State engineer. Pay him for his whole time, and let him give his whole time to the work, for he will need to do it. Such person should have in his possession a complete set of plans of every bridge of importance in the State, with all the computations of its strength, and as complete a history of each structure from its commencement as can be made up, all this to be supplemented by periodic examinations.

If, from such records, we find that a bridge was made of ordinary green timber twenty-five years ago, and that it has been getting rotten ever since, that it has rods of common merchant iron that was bought by some person not especially acquainted with the business, from an unknown firm, we had better pull it down before it falls

If, from such records, we find an iron bridge built twenty-five years ago, by an unknown company, with iron at best of a doubtful quality, and having a factor of three or four for the rolling stock and speeds of twenty years ago, instead of a factor of six for the rolling stock and speeds of to-day, we had better remove that bridge before it removes itself.

Such a record would be the property of the State, always accessible to anyone, and would be handed down so that the knowledge of one person would not expire with his term of office. No bridge should be erected in any State without first submitting the plans to the inspector, and receiving his approval, and depositing with him a complete set of the

plans and computations for the work. By this approval is not meant that the inspector is merely to give a favorable opinion as to the plan, but that he is to find as a matter of fact whether the proposed dimensions and proportions are such as will make a safe bridge; and just what a safe bridge is can be plainly defined by law as it is in Europe, and as it has been proposed by the American Society of Civil Engineers.

For example, if the law says that an iron railway bridge of one hundred feet span shall be proportioned to carry a load of three thousand pounds per lineal foot besides its own weight, and that with such a load no part shall be strained by more than ten thousand pounds per inch, all the inspector has to do is to go over the figures and see that the dimensions given in the plan are such as will enable the bridge to carry the load without exceeding the specified strains. When the work is erected the inspector must show that the plan has been exactly carried out, that the details are good, and proper evidence of the quality of materials used should also be given. Such inspection as this would at once prevent the erection of bridges like those at Ashtabula and Tariffville, and would save the public from such traps as those that fell at Dixon and at Groveland.

Perhaps the most difficult thing to do willbe to get satisfactory evidence in regard to the bridges that have been for a considerable time in use, and of which we do not know the history. This will be especially true in regard to the wooden bridges, of which there are so many about the country. Not only is it very difficult to be sure of the exact condition of the timber, but it is equally hard to tell anything about the iron. The Tariffville bridge fell on account of defective iron, and the defect was of such a nature as to defy any ordinary inspection.

What do we know to-day of the quality of the iron rods in any wooden bridge in Massachusetts? It is very doubtful if the best inspection we have in the United States at the present time would have found any defect so evident in the Tariffville bridge as to condemn it as unfit for the passage of trains. There are hundreds of exactly such bridges all over New England as far as we can tell by the best inspection we now have, made on the same plan, with no more material, and of which we know just as little of the quality of the iron as we did in the Tariffville bridge.

Of course we cannot expect to get a perfect system all at once. Any plan which would be proposed would no doubt be found more or less defective at first. We can hardly get a system worse than the one we now have, which allows forty bridges to break down every year. We may get a better one. To make the public see the need of such a system is the first step to be taken.

CORROSION OF IRON AND STEEL.

By M. L. GRUNER.

Translated from Annales des Mines for VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

In a previous note by the writer on the use of cast steel for rails, attention was called to the fact that moisture by favoring oxidation would hasten the wear of rails, and relying upon some experiments of M. Adamson, the belief was expressed that if soft steel rails wore less rapidly than hard steel, it must proceed in part from the greater liability to corrosion in steel that is more or less charged with a foreign element.

There remained some doubts as to the soundness of this conclusion, as M. Adamson had stimulated oxidation by acidulated water, instead of leaving it to the action of moist air.

Within the last year the writer has undertaken a series of experiments to determine the relative oxidizing effects of the different influences to which iron and steel are subjected in constructions and in the arts. The results of previous experiments have also been considered, especially those of the English engineers.

The question is not a new one. At

the suggestion of the British Association for the Advancement of Science, an extended series of experiments was performed by M. R. Mallet, of Dublin, to determine the durability of iron and steel structures when exposed to the prolonged action of air and water more or less pure. The results were published in the Association Reports of the years 1840 and 1843.

More recently the subject was reviewed, so far as it applied to the use of wrought iron and steel for hulls of vessels and for steam boilers, by the English Admiralty Board, and by two engineers, Messrs. D. Phillips and W. Parker. These results have been published, the first in the Reports of the Society of Civil Engineers, Vol. LXV., and the second in the Journal of the Iron and Steel Institute for May, 1881.

We shall see that the conclusions are in general accord with those of the writer, although regarded from a slightly different point of view.

The writer's experiments were made upon square plates measuring one decimeter on each side, cleaned and polished with lime and a dry grindstone. The weight varied according to the thickness, from 150 to 350 grammes, but as the surfaces were the same, the results obtained by the successive weighings may be directly compared.

In order to subject the plates to exactly the same conditions, they were fixed to the number of fifteen in a wooden frame, which was worked by a lever.

The frame was made of two strong vertical sides united by four horizontal cross pieces which were notched with a saw so as to hold the plates. The latter were therefore supported by the four corners in a manner which prevented reciprocal galvanic action.

The whole could be immersed simultaneously in a lead-lined trough containing the liquid designed to act upon the plates, or they could be suspended above the trough and exposed only to the moist air. The plates were placed parallel to each other and fifteen millimeters apart, thus forming together a sort of rectangular block with vertical compartments.

The first experiments were made in the winter of 1881–82 during a visit of the writer to the works of Saint Montan, near Beaucaire; some others at Paris, the last in Normandy near the sea, during the summer of 1882; all with the same apparatus.

The plates experimented upon were from different establishments. Eleven were supplied by M. Brustlein, Director of the Holtzer Steel Works at Unieux, near Firminy (Loire). Seven were furnished by M. Alfred Evrard, Director of the old works of Verdie, Firminy, and ten by the son of the writer, who is Director of the Saint Montan Works near Beaucaire.

The Unieux plates were of pure crucible steel, unequally charged with carbon, but all containing small quantities of foreign elements, except two, one of which was a chrome, the other a tungsten steel.

The amount of carbon in the plates according to M. Brustlein was:

Plate No. 1 contained 0.40 per ct. of carbon.
Plate No. 2 contained
0.90 per ct. carbon $\begin{cases} 0.022 \text{ manganese.} \\ 0.006 \text{ phosphorus.} \end{cases}$
Plate No. 2 contained
0.90 per ct. carbon $\begin{cases} 0.03\text{—manganese.} \\ 0.03\text{—phosphorus.} \end{cases}$
Plate No. 3 contained 1.10 per ct. carbon.
Plate No. 4 contained 1.40 per ct. carbon.
Plate No. 5 contained 0.55 per ct. carbon, and 2 per ct. chromium.
Plate No. 6 contained 1.26 per ct. carbon, and 1 per ct. tungsten.

M. Brustlein adds that No. 2 was analyzed with care in the laboratory of M. Boussingault at Paris. Also that plates Nos. 1 to 4 belong to the variety of *tool* steel made at Unieux from Sweedish iron or from that of the Pyrenees. Nos. 1, 3 and 4 differ slightly from No. 2 in regard to containing manganese and phosphorus. The difference in amount of carbon is the chief difference in composition. The silicon is below one thousandth. Nos. 5 and 6 contain besides chromium and tungsten, a little more manganese than the others, and a slightly stronger trace of silicon. All the specimens were practically free from sulphur, and all are steels of the first quality.

Considering furthermore that four of the plates tempered at Unieux were from the same ingots as Nos. 1, 3, 4 and 5, and fearing that the others had also been partially tempered during the rolling

process, it was considered expedient to heat them to a dull red in a closed crucible, before subjecting them to the oxidizing tests.

The samples from Verdie, although made by the Martin-Siemens process were remelted in a crucible to render them more homogeneous. Nos. 1, 4 and 6 were remelted only. Nos. 5 and 7 were treated with wood charcoal to increase the charge of carbon.

The composition of these plates, according to M. Evrard, was for 100 parts of steel as follows:

Specimen No.	Carbon.	Manganese.	Sulphur.	Phosphorus.	Silicon.
1	0.160	0.140	0.048	0.039	0.093
2	0.450	0 160	0.044	0.037	0.139
3	0 600	0.150	0.022	0.034	0.186
4	0.800	0.140	0.027	0.034	0.279
5	0.900	0.110	0.025	0.033	0.186
6	1.400	0.110	0.033	0.030	0.372
7	1 560	0.120	0.020	0.032	0.320

It may be seen therefore, that these steels are as pure as those of Unieux, except that Nos. 6 and 7 contain a little more silicon by reason of remelting in earthen crucibles.

Before subjecting these plates to the oxidizing processes, they were treated to a reheating as in the preceding case, to avoid inequalities of hardness acquired at the time of rolling.

The steels from Chatillon and Commentry, unlike those of Firminy, are of an ordinary kind for rails and plates. Three of them from Saint Montan were obtained from a Bessemer converter. They are hard steel for rails. Two of the specimens from Montluçon were made by the Martin-Siemens process, and were soft steel designed for plates.

The five specimens yielded to analysis the following constituents:

		Carbon.	Manganese	Silicon.
Steel of Montluçon.	No. 1	0.21	0.28	0.09
	No. 2	0.17	0.21	0.13
" " St. Montan	No. 1	0.26	0.36	0.24
	No. 2	0.55	0.72	0.56
Hard steel of St. Montan..		0.59	1.36	0.93

The last specimen was quite impure, and was admitted by accident.

The experiments in oxidation included besides the steels, four specimens of cast iron of very different composition.

1st. A casting direct from the ore at Saint Montan from a charge designed for Bessemer steel rails. It is very graphitic, black, and obtained by working at high temperature, with a strongly calcareous slag. It contains 3 to 4 per cent. of manganese, 1 to 2 per cent. of silicon, and very little sulphur or phosphorus.

2d. A very pure cast iron from an old ship cannon, made in a reverberatory furnace with wood, at Ruelle, in 1845. It is compact, tenacious, and of a gray color. To obtain the specimen, a fragment was remelted in a crucible.

3d. Ordinary gray iron for pots, of slight tenacity, and containing little phosphorus; also remelted.

4th. White specular iron from Saint Montan containing 20 to 25 per cent. of manganese.

It was necessary to cast the plate about a centimeter thick in order to resist the action of the dry grinding. For this reason the weights of the cast plates are in some cases about double those of some of the steel specimens, but the exposed surface are made the same.

1. Action of Moist Air.

1*st. Frequent but brief immersions in water with long exposure to the air.*

Passing now to the experimental results, we consider first those obtained by causing rust by frequent short immersions in water at ordinary temperatures, and alternate free exposure to the atmosphere.

At the time of the first experiments, which were unfortunately too brief, being of ten days' duration only, the plates were wet with fresh water. The results were not striking. The influence due to the variable character of the plates was hardly sensible. This experiment is to be repeated, and will be prolonged through several weeks. In the meanwhile, the results obtained in this first experiment, made in Normandy, between the 5th and 15th of September, 1882, are here given.

It may be added that to facilitate cleansing, the plates were dipped on the last day in water acidulated with $\frac{1}{4}$ per

cent. of sulphuric acid of 66 "Beaume," after which they were brushed, then rapidly dried, and finally weighed.

TABLE A.

Origin of plates.	No.	Original wt.	Final wt.	Loss.	Remarks.
	grs.	grs.	grs.	grs.	
Annealed steel from Holtzer Works.	1	300.5	299.2	1.3	
	2	324.4	323.5	0.9	
	2a	331.9	329.2	2.7	
	3	304.2	303 0	1.2	
	4	320.8	318.2	2.6	High steel.
	5	338.5	336 0	2 5	Chrome steel.
	6	339.0	337.7	1.3	Tungsten steel.
Hard steel from Holtzer Works.	1	313.9	312.5	1.4	The numbers correspond to the annealed plates.
	3	291.1	289 0	2.1	
	4	326.5	324.5	2.0	
	5	333.2	331.5	1.7	
Soft steel from Montluçon	1	149.9	149.0	0.9	
	2	161.4	160.0	1.4	
Hard steel from St. Montan.	1	282.2	280.5	1.7	
	2	357.5	356.2	1.3	

The only conclusion to be drawn from the above results is, that the rusting due to moist air attacks all steels nearly alike whatever their degree of carbonization or their purity, and moreover, that the temper of the steel does not seem to modify in a sensible degree the liability to corrosion.

2d. *Short but frequent immersions in acidulated water; long exposure to the air.*

The same plates were subjected to action of exposure to air after immersion in water containing $\frac{1}{2}$ per cent. of concentrated sulphuric acid. The immersion lasted ten minutes. They were taken out as soon as hydrogen was set free to any sensible degree. The exposure to the air continued from six to ten hours, and then they were immersed anew for some minutes. After five days the acidulated water was renewed, and the previous operations were repeated for eleven days, so that the total duration of the experiment was sixteen days (May 29th to June 13th).

At the end, the plates were submerged for two or three hours in the bath which had been used for the last eleven days.

The following results were obtained:

TABLE B.

Origin of plates.	No.	Original wt.	Final wt.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Holtzer works.	1	311.3	307.0	4.3	
	2	334.9	330.5	4.4	
	2bis	342.3	338.8	3.5	
	3	315.3	311.0	4.3	
	4	331.0	327.5	3.5	
	5	351.2	345.4	5.8	The chrome steel cleanses readily, yielding a gray, greasy deposit.
	6	348.3	345.3	3.0	
Tempered steel from Unieux.	1	326.3	320.2	6.1	
	3	304.1	297.3	6.8	
	4	340.8	334.1	6.7	
	5	344.1	339.1	5.0	
Soft steel from Montluçon	1	157.3	152.5	4.8	
	2	169.7	165.8	3.9	
Hard steel from St. Montan	1	292.6	287.8	4.8	
	2	369.2	364.6	4.6	

As in the preceding experiments, we see that the hardness of the steel has little or no influence on the liability to rust. We can only state that tempered steels, with the exception of chrome steel, are more strongly attacked than the same steels annealed; that among the untempered steels, chrome steel is the most affected, and tungsten steel the least. We shall see that these differences should be attributed to the acid, for in the experiments in which acidulated water alone acts, there being no lengthened exposure to the air, the chrome steel is most acted upon, and the tempered steels suffer more than the untempered ones.

The same method of immersion in acidulated water and subsequent exposure to the air has been applied to the steel plates from Verdié; the steels charged with manganese and silicon from St. Montan, and to the four cast-iron plates above mentioned, one experiment continued five days, the other twenty.

For the first experiment, which lasted from the 15th to the 20th of March, the water contained $\frac{1}{2}$ per cent. of sulphuric acid, which was renewed on the second, and again on the fourth day so as to excite energetic action.

In the second experiment the solution contained at first only $\frac{1}{4}$ per cent. of acid. On the sixth day this was increased to $\frac{1}{2}$ per cent., but the bath was renewed only twice during the last fifteen days, in order that the oxidizing action might be less intense.

The results of the first of these experiments are as follows:

Table C.

Origin of plates.	Nos.	Original weight.	Final weight.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed plates from Verdié	1	245.0	242.3	2.7	
	2	266.8	263.8	3.0	
	3	268.5	265.8	2.7	
	4	283.1	280.6	2.5	
	5	265.0	262.5	2.5	
	6	295.5	294.0	1.5	Strongly carbonized. The coatings more easily remov'd.
	7	273.0	270.7	2.3	
Steel from St. Montan containing silica and manganese.	*	265.6	363 6	2.0	These plates cleaned easily, especially the tempered one, which deposited a soft coati'g
	†	369.5	367.0	2.5	
Black pig iron for Bessemer steel.	1	315.5	311.6	3.9	Promptly attacked by the acid, leaving a graphite dep'os't
Charcoal pig for cannon.	2	337.9	336.0	1.9	
Gray pig for pots.	3	394.0	391 3	2.7	Not so readily attacked as No. 1.
Manganese pig iron fr'm St. Montan.	‡	726.2	723.4	2.8	

* Untempered. † Tempered. ‡ White crystalline.

As in the preceding cases, these steels exhibit but slight differences as regards liability to oxidize.

Apart from the black pig iron, and the manganese iron from Saint Montan, the irons are not more corroded than the steel plates. The charcoal iron from Ruelle is the least oxidized. The specular iron, although containing 20 per cent. of manganese, is relatively but slightly affected, the carbon seeming to protect the metal. The black cast iron is, on the contrary, easily oxidized, partly from its porosity, and partly, without doubt, owing to the contained silicon.

The second experiment, continued through twenty days, gave results as exhibited in the next table.

Table D.

Origin of plates.	Nos.	Original weight.	Final weight.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Verdié forge.	1	242.0	238.0	4.0	The steels act nearly alike; the coating is easily removed. Under the rust is a gray, unctuous coating.
	2	263 7	259.4	4.3	
	3	265.4	260.8	4.6	
	4	280.0	275.2	4.8	
	5	261.8	257 1	4.7	
	6	292.5	288.1	4.4	
	7	269.9	265.3	4.6	
St. Montan steel with silicon and manganese	*	362.0	358.2	3.8	Black coating under the rust.
	†	362.9	358.1	4.8	
Black pig iron from St. Montan.	1	295.7	291.6	4.1	No. 1 was easily cleaned of rust. A black coating of graphite beneath.
Charcoal pig iron from Ruelle.	2	335.2	331.9	3.3	
Gray pot metal.	3	382.4	379.5	2.9	
Manganese iron of St. Montan.	‡	721.9	720.0	1.9	

* Untempered. † Tempered. ‡ White crystalline.

The differences are less pronounced than in the preceding case, by reason of the less amount of acid in the bath. The conditions more nearly resemble those of the earlier experiment in which moist air alone was the oxidizing agent.

It will be seen here that as in the preceding cases, tempering favors oxidation.

The iron plates resist oxidation rather better than the steel, the manganese, *spiegel* iron being less corroded than any of the others.

II.

Action of Fresh Water and Sea Water.

Following the experiments made under the influence of moist air, were conducted some in which oxidation depended upon fresh water and salt water respectively.

It is well known that pure water will attack iron, neither at the ordinary temperatures nor at the heat of boiling water. But oxidation is after a time produced under the combined action of the air and carbonic acid dissolved in the feed water of boilers.

In order to properly measure the effect of fresh water, a prolonged immersion is evidently necessary. Now the only experiment made upon our plates was continued only eleven days. It was manifestly insufficient, but even this short period was enough to show that the iron plates appear to disadvantage when compared with steel in fresh water. The manganese irons particularly yielding with great readiness and exhibiting a covering of black rust. Thus while the steel specimens have lost at the maximum only 0.gr.3 in eleven days, the loss of the black cast iron of Saint Montan was 0.gr.7, and of the spiegel iron 1.gr.5 Then follows the charcoal iron 0.gr.6, and the phosphorus iron (hot metal) with 0.gr.3.

The steel plates furthermore were only slightly reddened on the parts near the surface of the water, while the two manganese plates were entirely covered wit a flocculent dark rust.

But although fresh water acts slowly, sea water acts with great energy. A chloride of iron was observed to form on the first day of exposure, and this was soon followed by flakes of oxide. A li tle hydrogen was seen to escape, and the iron plates were covered with a black rust.

The experiment continued nine days. and the water was renewed three times. The results of the action of sea-water are shown in Table E, preceding column.

It is readily seen by Table E that sea-water, like fresh water, attacks the irons more readily than the steels, and that of the four iron specimens it is the manganese iron which is most corroded. The difference is great and contrasts singularly with the action of humid air, which attacks the manganese iron with less vigor than the three others.

In comparing the two tables D and E, it may be remarked that while sea-water attacks the gray phosphureted iron quite strongly, fresh water and moist air act upon such iron less readily than upon the other varieties.

TABLE E.

Origin of plates.	Nos.	Original weight.	Final weight.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Verdié.	1	234.8	232 7	2.1	
	2	256.8	254.8	2 0	
	3	258.0	256.9	1.1	
	4	272.5	270.3	2.2	
	5	253.9	252.7	1.2	
	6	285.3	283.3	2.0	
	7	262.0	260.2	1.8	
St. Montan containing manganese and silicon.	*	354.2	351.2	3.0	A black unctous deposit under the rust.
	†	350.8	348.0	2:8	
Black pig iron, St. Montan.	1	280.4	276.9	3.5	On all the iron plates a black carbon deposit under the rust.
Charcoal iron of Ruelle.	2	326.9	323.8	3.1	
Gray cast iron.	3	371.7	366.7	5.0	
Specular iron.	‡	719.0	712.0	7.0	

* Soft. † Tempered. ‡ White crystalline.

TABLE E′.

Origin of the plates.	No.	Original wt.	Final wt.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Holtzer Works.	1	305 0	301.9	3.1	
	2	328.5	326.1	2.4	
	2a	336.8	334.7	2.1	
	3	309.0	305.8	3.2	
	4	325.5	322.8	2.7	
	5	343.3	340.3	3.0	Under rust of No. 5 dark gray deposit.
	6	343.4	341.8	1.6	
Tempered steel from Holtzer Works.	1	318.2	315.7	2.5	
	3	295.3	293.3	2.0	
	4	332.1	329.7	2.4	
	5	337.1	335.9	1.2	Faint gray coating
Soft steel of Montluçon	1	150.5	149.9	0.6	
	2	163.8	161.9	1.9	
Hard steel of Saint Montan.	1	285.8	284.5	1.3	
	2	362.6	360.0	2.6	

One other anomaly is noticeable, it is that tempered steel seems less corroded by sea water than the same steel untempered, while tempering invites the action of acidulated water.

The fifteen plates of steel from Unieux were also subjected to the action of sea water. The experiment lasted seventeen days. The water was renewed but once. At the end of the period the plates were treated for an hour with a $\frac{3}{4}$ per cent. solution of acid for the purpose of removing the rust coating.

The results are herewith given. (See Table E′, page 215.)

This experiment, like the preceding, shows that the action of sea water is the reverse of that of acidulated water upon tempered steel. The tempered plates are less acted upon than the untempered.

Annealed chrome steel is more corroded than carbon steel, while tempered chrome steel is affected less. Comparison of the steels of Montluçon and Saint Montan proves that the soft steels are less corroded by sea water than hard ones, which accords with the results of the trial of acidulated water, while moist air affected both to nearly the same degree.

DIMENSIONS OF TALL CHIMNEYS.

By L. PINZGER.

Translated from Revue Universelle des Mines for VAN NOSTRAND'S ENGINEERING MAGAZINE

THE formula frequently employed for the determination of the height of a chimney is:

$$u_a = \varphi\sqrt{2gh\frac{T_m - T_o}{T_o}},$$

in which U_a is the velocity of the escaping gases; $T_o = 273 + t_o$, the absolute temperature of the exterior air; $T_m = 273 + t_m$, the absolute mean temperature of the gases in the chimney (t_o and t_m being expressed in centigrade degrees); $g = 9.81$, the acceleration of gravity, and φ a coefficient of correction relating to the resistances opposed to the motion of heated gases.

M. Grashoff, in 1866, propounded a theory which renders it easy to take account of the causes which modify the movements of the products of combustion; more recently he has introduced a *résumé* of this theory in his work; *Construction des Machines.*

While the results thus obtained are methematically accurate, they have not yet taken a convenient form for practical use; partly because the principles of the mechanical theory of heat have not been generally adapted to the calculation of the movement of gases, and partly in consequence of the complex character of the formulas.

Thus the height h of the chimney cannot be deduced directly in terms of the other quantities; it is necessary to resort to approximate methods. In order to simplify the calculations, M. Grashof has constructed tables adapted for use in the case of chimnies and temperatures such as are commonly employed in practice. He has also established easy empirical formulas deduced from exact theory.

The writer proposes to briefly review the principles involved, and then to develop a method of calculation which shall possess the precision of Prof. Grashof's, but which will afford an easy practical formula.

The special function of a chimney is to cause a sufficient quantity of air to traverse the mass of fuel spread over the grate to insure its combustion—to make the hot gases circulate through conduits where they part with a portion of their heat—and finally to discharge these gases into the atmosphere with a velocity which shall diminish as much as possible the action of the wind upon the draft.

When the products of combustion are deleterious to health or vegetation, it becomes necessary to employ high chimneys, so that these products shall reach the earth only after being mixed with a large volume of air.

The cause of the draft lies in the difference of pressure in the gases in the fire, the flues, the shaft and that of the outside air at the level of the grate.

Let p_0 be the atmospheric pressure in kilograms per square meter at the level of the grate; p_1, p_2, p_3 and p_4 the pressures in the fire, at the entrance to the flue; at the outlet from the flue; in the horizontal shaft and at the base of the chimney; then we ought to have $p_0>p_1>p_2>p_3>p_4$ so as to insure draft.

It is necessary then, 1st, to obtain such a pressure p_4 at the base of the chimney as would be produced by a current of air required for the combustion; and 2d, to force the gas to the height of the chimney and discharge it with sufficient velocity.

The difference of pressure p_0-p_1 cannot be determined by theory alone, but the direct measure of it is obtained by the manometer. In stationary furnaces this difference corresponds to a water column of 3 to 20 millimeters, according to the thickness of the layer of fuel. In locomotives it may be as high as 100 millimeters. As the weight of a column of water having a base of one square meter and a height of one millimeter is one kilogram, it follows that p_0-p_1 may be equal from 3 to 20 kilograms per square meter.

If we let h_0 represent the height of a column of air of the temperature T_0 of which the weight upon one square meter of base is p_0-p_1, we shall have

$$(1) \qquad h_0=\mathrm{RT}_0 \log. \left(\frac{p_0}{p_1}\right)$$

R expressing the constant of the equation of gaseous condition, $pv=\mathrm{RT}$. R being equal to 29.3 for atmospheric air, more or less humid.

In taking $p_0=1000^k$ per square meter under average conditions of barometric pressure, and $\mathrm{T}=273+17=290$, a column of air of $h_0=4^m.25$ to 17^m will be equal to the pressure above mentioned.

The height h_0 will depend upon the quantity of the fuel burned in a unit of time—the thickness of the layer of fuel and upon the weight of the products of combustion.

Grashof assumes for the heat of soft coal the value $h_0=25\,\mathrm{G}_1\triangle^2$ in which G represents the quantity of gas produced by the combustion of 1 kilogram of fuel, and $\triangle$ the thickness of the layer of fuel.

If $\mathrm{G}_1=22$ kilograms and $\triangle=0^m.1$, we have:

$$h_0=25\times 22\times 0.01=5^m.50.$$

We may by this calculation find with sufficient exactness the values of the differences of pressure p_1-p_2, p_2-p_3 and p_3-p_4. Taking into consideration the relations existing between different systems of heating and of flues, we find:

$$\log\left(\frac{p_1}{p_2}\right)=\frac{1}{\mathrm{RT}_0}(1+\zeta_2)\frac{u_0^2}{2g}\cdot\frac{\mathrm{T}_2}{\mathrm{T}_0}.$$

$$\log\left(\frac{p_2}{p_5}\right)=\frac{1}{\mathrm{RT}_0}\frac{u_0^2}{2g}$$

$$\left\{\lambda\frac{l}{d}\left(\frac{\mathrm{Q}}{k\mathrm{FT}_0}\right)+\frac{\mathrm{T}_k}{\mathrm{T}_0}\right)-2\frac{\mathrm{T}_2-\mathrm{T}_5}{\mathrm{T}_0}\right\}$$

and $$\log\left(\frac{p_5}{p_4}\right)=\frac{1}{\mathrm{RT}_0}\zeta_3\frac{u_0^2}{2g}\frac{\mathrm{T}_5}{\mathrm{T}_5'},$$ whence

$$\mathrm{RT}_0\log\left(\frac{p_1}{p_4}\right)=\frac{u_0^2}{2g}\left\{\frac{(1+\zeta_2)\mathrm{T}_2+\zeta_5\mathrm{T}_5}{\mathrm{T}_0}\right.$$

$$\left.+\lambda\frac{l}{d}\left(\frac{\mathrm{Q}}{k\mathrm{FT}_0}+\frac{\mathrm{T}_k}{\mathrm{T}_0}\right)-2\frac{\mathrm{T}_2-\mathrm{T}_5}{\mathrm{T}_0}\right\}.$$

$\mathrm{RT}_0\log\frac{p_1}{p_4}$ expressing equally the height h_1 of a column of air at the exterior temperature T_0, subjected to the pressures p_1 and p_4 upon the lower and upper bases, and of which the weight measures the pressure p_1-p_4 on a square meter. Consequently

$$(2) \quad h_1=\frac{u_0^2}{2g}\left\{\frac{(1+\zeta_2)\mathrm{T}_2+\zeta_5\mathrm{T}_5}{\mathrm{T}_0'}+\right.$$

$$\left.\lambda\frac{l}{d}\left(\frac{\mathrm{Q}}{k\mathrm{FT}_0}+\frac{\mathrm{T}_k}{\mathrm{T}_0}\right)-2\frac{\mathrm{T}_2-\mathrm{T}_5}{\mathrm{T}_0}\right\}.$$

In this equation u_0 represents the velocity with which the heated gas passes the section f of the flues if they have the temperature T_0; T_2 and T_3 are the temperatures at the escape from the flue; T_k the absolute temperature (of water in a steam boiler); l the length of the flues; d their mean diameter $=\frac{4f}{\mathrm{P}}$; (P being the perimeter and f the section); F the heated surface of the boiler; Q the quantity of heat transmitted by this surface every hour; k the coefficient of conductivity of the surface for heat, which for boilers is about 20; ζ_2 and ζ_3 the coefficients of correction relating to the sudden changes in diameter and direction; (in steam boilers $\zeta_2=1.5$ to 25, and

$\zeta_3=0.8$ to 1); λ the coefficient of friction in the flues ($=0.08$).

Between the quantities F, Q, T_2, T_3 and

$$\text{(A)}\quad T_3=T_k+(T_2-T_k)e^{-\frac{kF}{Gc}}$$

$$\text{(B)}\quad Q=Gc(T_2-T_3)$$

in which G expresses the weight of the gases flowing through each section of the flues per second, and c is the specific heat, of which the mean value is 0.25 for bituminous coal fuel.

The weight of a column of air of the external temperature T_0 and a height h_0+h_1, representing the difference of pressure p_0-p_4 per square meter, the point is to determine the height h above the level of the grate to which the gases can be raised in the atmosphere. This would be the height for the chimney.

In this calculation there should be taken into account; 1st, the diminution of atmospheric pressure due to the height h; 2d, the diminution of the pressure of gas for the same height; 3d, the mechanical work consumed by the ascent of the gas through this height; 4th, the velocity of the escaping gas at the top of the chimney. This outflow cannot evidently take place unless the pressure on the escaping gas is at least equal to the atmospheric pressure.

In order to calculate the upward velocity of the gases in the chimney, we make use of the equation

$$\frac{udu}{g}=-ds-RT\frac{dp}{p}-\lambda\frac{ds}{d_m}\frac{u^2}{2g},$$

in which p is the pressure at the top of the chimney and d_m is the mean diameter.

If we represent by u_4 T_4 and p_4 respectively, the velocity, the absolute temperature and the pressure of the gas at the base of the chimney; also by u_a T_a and p, the same quantities at the upper outlet of the chimney; the integration of the preceding equation, if we take $T_m=\frac{1}{2}(T_4+T_a)$ for the mean temperature of the chimney, also $u_m=\frac{1}{2}(u_4+u_a)$ for the mean velocity, will give for the term relating to the resistance of friction

$$\frac{u_a^2-u_4^2}{2g}+\lambda\frac{h}{d_m}\frac{u_m^2}{2g}+h=RT_m\log\left(\frac{p_4}{p}\right).$$

On the other hand we shall have for the height h of a column of air, at the temperature T and under the upper and lower pressures of p_0 and p,

$$h=RT_2\log\left(\frac{p_0}{p}\right).$$

Combining the preceding equations we get:

$$RT_0\log\left(\frac{p_0}{p_4}\right)=h\,\frac{T_m-T_0}{T_m}-\frac{T_0}{T_m}.$$

$$\left\{\frac{u_a^2-u_4^2}{2g}+\lambda\frac{h}{d_m}\cdot\frac{u_m^2}{2g}\right\}.$$

The first number of this being only the value of h_0+h_1, the height of the chimney will be given by the formula:

$$\text{(3)}\quad h=(h_0+h_4)\frac{T_m}{T_m-T_0}+\frac{u_a^2}{2g}$$

$$\left\{1-\left(\frac{u_4}{u_a}\right)^2+\lambda\frac{h}{d_m}\left(\frac{u_m}{u_a}\right)^2\right\}\frac{T_0}{T_m-T_0}$$

in applying which to find approximate values of h, the following hypothesis is employed:

From the fundamental equation $pv=RT$, and from the equation $fu=G'v$ we deduce

$$u=G'R\frac{T}{pf},$$

G being the weight of gas in kilograms which passes each section of the chimney in a unit of time with the velocity u.

The influence of the change of pressure p upon the variation of the velocity u is so small that we may neglect it, while we have to regard the influence of change of temperature and of cross section, and we have with sufficient exactness:

$$\frac{u_4}{u_a}=\frac{T_4}{T}\frac{f_a}{f_4}$$

and

$$\frac{u_m}{u_a}=\frac{1}{2}\left(\frac{u_4}{u_a}+1\right).$$

We can calculate from equation (3) a first approximate value of h, after having assigned proper values to f_a f_4 and f_m, also to T_a according to the value of T_4 and according to the material of which the chimney is built. It is necessary also to determine $\frac{u_4}{u_a}$ and $\frac{u_m}{u_a}$, since in the term $\frac{h}{d_m}$, we substitute the approximate value $(h_0+h_1)\frac{T_m}{T_m-T_0}$.

From this value of h a more exact value of T_a can be deduced by employing the equation

$$T_a = T_0 + (T_4 - T_0)e^{-\frac{k_s F_s}{G_s c}}$$

in which k_s represents the coefficient of conductivity of heat for the sides of the chimney; F_s the area of the interior surface; G_s the weight of gas per hour passing any section of the chimney; $c=0.25$, the specific heat of the gaseous mixture.

For chimneys of masonry, we take $K_s = 1.4$ to 2 according to the thickness of the sides. For chimneys in plate or sheet iron $k_s = 6$; $G_s = 22B_n$; B_n being the weight of coal consumed per hour on all the grates whose fires lead to the same chimney.

From this value of T_a we easily deduce more exact values of T_m, $\frac{u_4}{u_a}$ and of $\frac{u_m}{u_a}$. We shall then be able to obtain a second approximate value of h, which may be regarded as a definite value.

We shall have for the magnitude of f_a, the entrance to the chimney,

$$f_a = \frac{G_s u_a}{3600\, u_a}$$

or $$f_a = 0.062 \frac{T_a}{u_a} \cdot \frac{B_n}{3600}.$$

In calculating $\frac{f_a}{f_4}$ there are three cases to be considered:

1st. The transverse section of the chimney decreases from the bottom upwards; $f_a < f_4$. (Fig. 1.)

2d. The transverse section is everywhere the same; then $f_a = f_4$. (Fig. 2.)

3d. The section increases from below upwards; then $f_a > f_4$. (Fig 3.)

When f_a is less than f_4, the ratio varies from 0.40 to 0.64 for chimneys constructed either in masonry or iron according to height. We may take therefore as a mean value $\frac{f_a}{f_4} = 0.52$.

The second condition of course gives $\frac{f_a}{f_4} = 1$.

For the third case we take $\frac{f_a}{f_4} = 1.5$.

With regard to the temperatures T_4 and T_a we may introduce as first values in the calculation

$$\frac{T_4}{T_a} = 1.06 \text{ for chimneys in masonry.}$$

and $$\frac{T_4}{T_a} = 1.10 \text{ for iron chimneys.}$$

For the velocity u_a with which the gas ought to escape from the chimney, we should take a rather large value for fear of interference by downward currents of air. The value of u_a should never be less than two meters.

Suppose that to calculate the dimensions of a masonry chimney, we admit that $\frac{T_4}{T_a} = 1.06$. We shall then obtain the following values:

For the first type, (Fig. 1)

$$\frac{u_4}{u_a} = 1.06 \,.\, 0.55 = 0.55; \qquad \frac{u_m}{u_a} = 0.775$$

thus $$h = (h_0 + h_1)\frac{T_m}{T_m - T_0} + \frac{u^2_a}{2g} \left\{ \lambda \frac{h}{d_m} \,.\, 0.6 + 0.7 \right\} \frac{T_0}{T_m - T_0}$$

for the second type, (Fig. 2)

$$\frac{u_4}{u_a} = 1.06; \quad \frac{u_m}{u_a} = 1.03$$

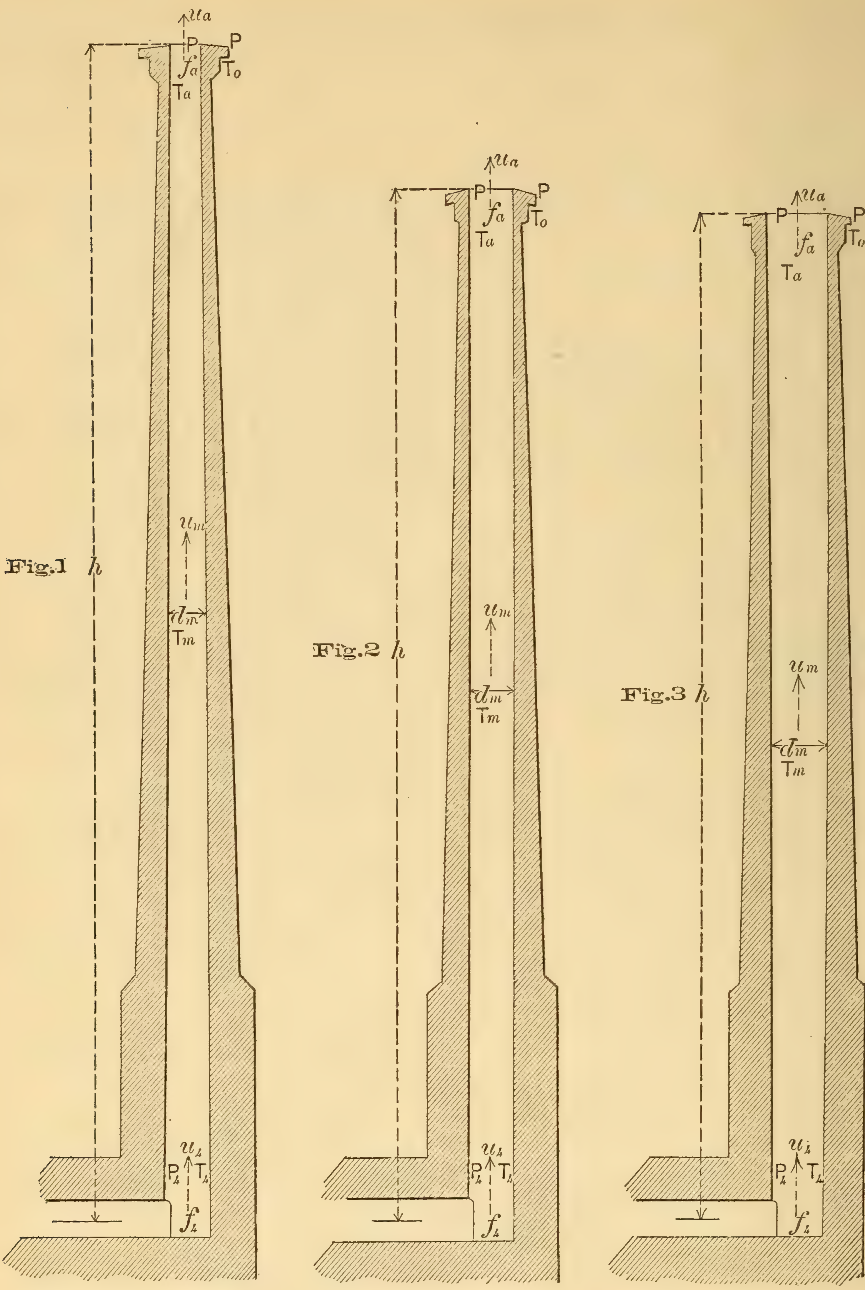
then $$h = (h_0 + h_1)\frac{T_m}{T_m - T_0} + \frac{u^2_a}{2g} \left\{ \lambda \frac{h}{d_m} \,.\, 1.06 - 0.24 \right\} \frac{T_0}{T_m - T_0};$$

for the third type, (Fig. 3)

$$\frac{u_4}{u_a} = 1.06 \,.\, 1.5 = 1.59; \quad \frac{u_m}{u_a} = 1.3;$$

then $$(h = h_0 + h_1)\frac{T_m}{T_m - T_0} + \frac{u^2_a}{2g} \left\{ \lambda \frac{h}{d_m} \,.\, 1.7 - 1.53 \right\} \frac{T_0}{T_m - T_0}.$$

It is easy to see that for all forms the total height of the chimney depends; 1st, upon the amount of resistance encountered by the gases in the flues and through the fuel; 2d, upon the velocity of escape of the gases. The second term is very small compared with the first, and as the variation of h for one type or the other depends on the second term, it is evident that the employment of type No. 3 can-



not lead to a notable diminution of the height which it is necessary to use in No. 1. On the contrary the height becomes even less for the chimney contracted in the upper part than for the cylindrical form or for that which enlarges upwards; if it is demanded that the velocity of escape of the gases u_a be the same for the three types, the reason is that for the first form the resistance due to friction is more feeble. The fact should not be lost that in consequence of the large value of u_4 for the 3d type compared with the value of the same function

for the 1st type, that the specific pressure p_4 would be much less in the first case than in the second; consequently the enlargement of the chimney towards the top would occasion a stronger current of hot gases at the bottom.

If on the other hand we desire that the value of u_4 be the same for the three types, for equal ratios of temperatures and pressures, we shall have for the chimneys:

Fig. 1, $u_a = 1.82\,u_4$,

Fig. 2, $u_a = 0.94\,u_4$,

Fig. 3, $u_a = 0.63\,u_4$.

Let for example, for the 1st form $u_a = 4$ meters consequently $u_4 = 2^m.2$.

Let in 2d form $u_a = 2^m.07$

and in 3d form $u_a = 1^m.39$

With this feeble velocity of escape, the draft of the chimney would be easily reversed by the wind.

Take as an example a brick chimney designed to discharge the products of combustion from three boilers of equal size. Allowing for each boiler a heating surface of 60 square meters, and requiring a consumption of 100 kilos of coal per hour; the length of the flues being 30 meters, and their mean cross section $f = 0.2$ sq. meter; we have $d = \frac{4f}{P} = 0.^m25$.

Let $T_2 = 1300°$; $T_k = 420°$; $G = 2200^k$, we shall have according to (A)

$$T_5 = 420 + (1300 - 420)\,e^{-\frac{10.60}{2200.0,25}} = 520°;$$

and according to (B)

$$Q = 2200 \times 0.25(1300 - 520) = 429000^c.$$

(The heat directly radiated not being included.)

For $T_0 = 280°$ we shall have:

$$u_0 = \frac{Gv}{3600f} = \frac{2200 \times 0.8}{3600 \times 0.2} = 2^m.45$$

and according to equation (2):

$$h_1 = 0.306 \left\{ \frac{3 \times 1300 + 1 \times 520}{280} + 0.08 \frac{30}{0.25} \left(\frac{429000}{20 \times 60 \times 280} + \frac{420}{280} \right. \right. \left. -2\,\frac{1300 - 520}{280} \right\} = 11^m.3.$$

Assuming $h_0 = 5^m.5$ we shall have:

$$h_0 + h_1 = 16^m.8.$$

Since the heated gases are cooled in passing through the horizontal conduit, we shall have to admit that the absolute temperature T_4 is only 500° at the base of the chimney; whence

$$T_a = \frac{500}{1.06} = 470° \text{ and } T_m = \tfrac{1}{2}(500 + 470) = 485°$$

We will take the velocity of escape of the gas, $u_a = 6$ meters, so that when only one boiler is used the velocity will be 2 meters; we then have:

$$u_a^2 = 1^m.835,$$

and equation (4) gives:

$$f_a = 0.062 \frac{470}{6} \cdot \frac{300}{360} = 0.405 \text{ sq. meters,}$$

$$d_a = 0.718, \text{ or nearly } 0.72 \text{ meters.}$$

In order to provide, in case of need for a fourth boiler, we will take:

$$f_4 = 4f = 4 \times 0.2 = 0.8 \text{ sq. meters.}$$

then $d_4 = 1.0$ meter, and

$$d_m = \tfrac{1}{2}(1.0 + 0.72) = 0.86 \text{ meters.}$$

We now have

$$\frac{u_4}{u_a} = \frac{500}{470}\,\frac{0.405}{0.800} = 0.54 \text{ and } \frac{u_m}{u_a} = 0.77.$$

The first approximate value then is

$$h = 16.8\,\frac{485}{485 - 280} + 1.835 \left\{ 0.08 \,.\, 0.593\,\frac{46}{0.86} + 0.708 \right\} \frac{280}{485 - 280}$$

$h = 39.665 + 8.133 = 47^m.798$, say 48 meters.

We obtain more exactly

$$T_a = 280 + (500 - 280)\,e^{-\frac{1.6.130}{6600.0.25}} = 473°.$$

This result accords so well with the value 470° introduced above in the calculations of T_a, that it will not be necessary to make any correction in the estimated height of 48 meters.

We have $u_4 = 0.54 \times 6 = 3.24$ meters for the velocity with which the gas enters a chimney whose cross section diminishes upward. (Fig. 1.)

If a cylindrical chimney be used (Fig. 2) for which the velocity of escape $u_a = 0.94 \times 3.24 = 3.046$ meters, the height h will be reduced to:

$h=39.665+0.473$

$$\left\{0.08\frac{42}{1.0}1.06-0.124\right\}\frac{280}{485-280}$$

$h=39.665+2.221=41^m.886$, or 42^m.

For a chimney, wider at the top than the bottom, for which $\frac{f_a}{f_4}=1.5$, we have

$u_a=0.63\ .\ 3.24=2^m.04$ and

$h=39.665+0.212$

$$\left\{0.08\frac{41}{1.12}1.7-1.53\right\}\frac{280}{485-485}$$

$h=39.665+0.999=40^m.664$, or 41^m.

The employment of the forms represented in figures 2 and 3 leads to a reduction in the estimated height of 6 or 7 meters, but the velocity of the escaping gases is reduced from 6 to 2 or 3 meters, which might lead to serious inconvenience when only one boiler is at work instead of three. In such a case a chimney of the third type, the velocity of the escaping gases would only be $\frac{2}{3}$ meter.

In order to diminish the downward action of the wind, the chimney may be surmounted by a conical cap. Many constructors do not approve of the widened top, although it allows of greater width to the orifice and therefore greater facilities for changing the direction of the currents of air at the summit of the chimney.

CENTRIFUGAL VENTILATORS FOR MINES.

By DANIEL MURGUE.

From Proceedings of the Institution of Civil Engineers.

Mine ventilators acting on the principle of a pump—known also as ventilators of varying capacity, or of displacement—formed the subject of two previous papers by the author, who is engineer of the Bessèges collieries in the department of Gard, in the south of France. Having subsequently acted as one of the three commissioners who reported upon the ventilation of the mines in that district, he is now able to make use of their report as a basis upon which to investigate the working of mine ventilators exhausting by centrifugal action. The characteristic features of centrifugal exhausting fans are—their extreme simplicity, whether revolving vertically or horizontally; the free passage for the air through them from the upcast shaft to the external atmosphere; and the large air-current they are capable of discharging from mines having large air-ways—all three of which are in marked contrast with the features distinctive of displacement ventilators.

Vacuum produced by Centrifugal Ventilators.—For investigating the theoretical efficiency of centrifugal ventilators in regard to the degree of vacuum they produce, the author recurs to his principle of "equivalent orifices." Thus, supposing the air-drift from the upcast shaft to the ventilating fan were closed by an air-tight sheet across its mouth, the revolving fan, though producing no current, would maintain an initial vacuum H in the confined space of the drift. If now an inlet orifice of area a be made in the sheet, the effective vacuum h of the current will be less than the initial vacuum H, by the amount of loss due to friction and eddies in the passage of the air current through the fan itself. Supposing the air discharged from the fan had to pass through an outlet orifice of area b in another sheet, presenting an obstruction equivalent to that encountered in its passage through the fan, then the obstructions presented to the ventilating current by the mine and by the fan respectively are replaced by two "equivalent" orifices, a and b, in two imaginary sheets, between which the fan works. The relation $\frac{H-h}{h}=\frac{a^2}{b^2}$ is readily deduced; whence the author arrives at the two general formulæ for the effective vacuum h, and for the effective volume V of air current per second: namely,

$$h=\frac{H}{1+\frac{a^2}{b^2}} \text{ and } V=\frac{0.65\,a\sqrt{2gH}}{\sqrt{1+\frac{a^2}{b^2}}}$$

where the vacuum h or H is measured

by the equivalent head or column of *air*, assumed at atmospheric pressure, and non-elastic within the usual small range of exhaustion produced by a mine ventilator.

Investigating next an expression for H, he shows that the vanes of any ventilating fan ought to become truly radial at their tips, instead of being inclined backwards; and that with the radial vanes $H = \frac{u^2}{g}$, where u is the velocity of the tips of the vanes, and the fan revolves within a casing having an expanding chimney, from which the air is discharged into the atmosphere with a velocity low enough to be neglected. Hence in a perfect ventilator, the air-column representing the initial vacuum H would theoretically be twice the head that corresponds with the circumferential speed of the fan. But the imperfections met with in practice require the insertion of a fractional coefficient B, less than unity, which the author calls the "manometric efficiency," to distinguish it from the fan's mechanical efficiency or useful effect proper. Putting therefore $H = K\frac{u^2}{g}$, the two foregoing formulæ become

$$h = \frac{Ku^2}{g\left(1+\frac{a^2}{b^2}\right)} \text{ and } V = \frac{0.65\, a\, u\sqrt{2K}}{\sqrt{1+\frac{a^2}{b^2}}}$$

For the usual English mode of stating the volume of the ventilating current in cubic feet per minute, and the vacuum in inches of water column, this ultimate expression for V holds good without alteration, provided the circumferential velocity u be taken in feet per minute and the areas a and b in square feet. But to get h in inches of water, while u is in feet per minute and g is 32.2 feet per second, a factor $\frac{12 \times 0.0012}{60 \times 60} = 0.000004$ is required, assuming with the author that the density of air is 0.0012 if that of water is 1.0000; and the ultimate expression for the vacaum then becomes

$$\left.\begin{array}{c}\text{Vacuum in}\\ \text{inches of}\\ \text{water-gauge}\end{array}\right\} = 0.000004 \times \frac{K}{g\left(1+\frac{a^2}{b^2}\right)} \times \left(\begin{array}{c}\text{circumferential velocity}\\ \text{in feet per minute}\end{array}\right)^2$$

Eliminating a, the two formulæ give $h = \frac{Ku^2}{g} - \frac{V^2}{2g \times (0.65b)^2}$; and writing M for the constant factor $\frac{1}{2\,g \times (0.65\,b)^2}$, the simple equation to a straight line is obtained, namely

$$h = H - MV^2$$

treating h as the ordinate, and the *square* of V as the abscissa. This equation presents the two-fold advantage, that it dispenses with the necessity for calculating the equivalent inlet orifice a, which represents the mine resistance to the pull of the ventilator; and that, by plotting graphically divers pairs of experimental values for h and V^2, it can be seen at a glance, and with great accuracy, to what extent these fall in with the straight line represented by the equation. On these accounts the author strongly recommends the adoption of this method for general use in observations or experiments on the working of ventilators.

Experimental Verification.—The validity of his theoretical conclusion was tested by the author by means of the data furnished from experiments made by the Gard Commission upon three ventilating fans in that district. For each of these, five different degrees of mine resistance were artificially presented to the pull of the fan, so as to cover the range of obstruction ordinarily encountered in mines having the smallest and the largest airways; the speed of the fan, the effective vacuum h produced, and the volume V of air-current, were measured. From these data was calculated the area b for the theoretically equivalent outlet orifice from the fan; and the ventilating current was reduced to correspond with an exactly uniform normal speed of 3,936 feet per minute for the circumference of each fan. The five pairs of values of h and V^2 were then plotted on a diagram; and the curve drawn through the five points so obtained was examined as to how nearly it was itself a straight line. The first ventilator, at Lalle colliery, Bessèges, was a kind of turbine of 12½ feet diameter, without casing, but working in a sort of semicircular wheel-pit, and discharging the air all round; its inlets were 6 feet diameter, and its width 4½ feet at center,

narrowing to only 2 feet at the circumference. The next, at Créal colliery of the Bessèges company, was $19\frac{3}{4}$ feet diameter and $3\frac{1}{2}$ feet wide, with inlet $11\frac{1}{2}$ feet diameter, in a casing with a short parallel chimney. The third, also at Bessèges, was a Guibal fan of $16\frac{1}{2}$ feet diameter and $6\frac{1}{2}$ feet width, with inlet 10 feet diameter, in a casing with adjustable shutter and expanding chimney, but without the latest improvement in the way of radial vanes.

With the Crèal fan running at $63\frac{2}{3}$ revolutions per minute, the five results accorded most closely with the foregoing theory, falling into a straight line without appreciable deviation. Against the highest mine resistance, represented by the smallest equivalent orifice of only 6.604 square feet area, the air current was 17,928 cubic feet per minute, with an observed water-gauge of 1.060 inch, instead of the theoretical vacuum of 1.059 inch. Against the lowest mine resistance, corresponding with the largest equivalent orifice of 20.922 square feet area, the air-current was 50.357 cubic feet per minute, with an observed water-guage of 0.832 inch, instead of the theoretical vacuum of 0.834 inch. The greatest difference between the observed and the calculated water-gauge in the five experiments did not exceed 0.002 inch. Tabulating with these Crèal results the highest and lowest of those similarly obtained at Lalle and at Besseges, the comparison stands as follows:—

As the mine resistance scarcely ever exceeds that represented by so small an equivalent inlet orifice as only 4 square feet, while in large mines, even in England, it rarely falls below what corresponds with the largest inlet here tried of nearly 30 feet square, these results clearly confirm the author's theory within the limits prevailing in practice. The exceptional deficiency of $\frac{1}{8}$ inch water-gauge in the vacuum observed with the Guibal fan at Bessèges, when working against the greatest resistance and producing the least current, is accounted for by the corresponding partial closing of the discharge orifice from the fan casing by the regulating shutter, whereby the circumferential friction of the outlet bears an increased proportion to its area, and the efficiency diminishes rapidly. Moreover the forward inclination of the blades at their inner ends, intended for cutting the entering air without a blow, suits only the normal current for which it is calculated; with a much lower current there is a rapid loss of effect and diminution of vacuum. With smaller currents too it becomes increasingly difficult to realize the pull due to the expanding area of passage between the fan-blades and along the chimney; indeed in mines with narrow air-ways the vacuum falls short of what would be inferred from that observed in larger mines, and is also very irregular, being found to fluctuate at short intervals. In the above experiments the actual discharge orifice

Ventilator.	Speed. Revolutions per minute.	Mine resistance. Area of equivalent inlet a	Air current. Volume per minute	Vacuum by water-gauge. Observed.	Calculated.	Greatest difference.
	Revolutions	Square feet.	Cubic feet.	Inch.	Inch.	Inch.
Lalle, 12.46 ft. diameter	100.52 *Circumferential speed 3,936 feet per minute.*	4.045 14.868	10,739 35,744	1.031 0.846	1.023 0.835	0.014
Crèal, 19.68 ft. diameter	63.66	6.604 20.922	17.928 50,357	1.060 0.832	1.059 0.834	0.002
Bessèges 16.40 ft. diameter	76.39	3.921 29.345	11,239 73,506	1.180 0.899	1.304 0.906	0.124

from each of the two fans at Creal and Bességes was in every instance adjusted to an area proportioned to the ventilating current.

The graphic method, of the foregoing equation to the straight line, gives the initial vacuum H corresponding with a closed inlet and no current; and thence the manometric efficiency represented by the coefficient K is known from the equation $H=K\frac{u^2}{g}$. By the same means is also ascertained the area of the equivalent outlet b, representing the obstruction encountered in the fan itself; which is no matter of theory, but depends chiefly on the width of the fan, the area for the passage of the current through it, and the size of its inlet. These several values for each of the same three fans are as follows:—

Whence it appears that in default of the true method, the approximation may be employed for obtaining a fair idea of the comparative efficiency with different fans. The expression for the approximate manometric efficiency is therefore

$$\frac{2982 \times (\text{observed water-gauge in millimeters})}{(\text{revs. per min.} \times \text{fan diam. in meters})^2}$$

$$\text{or } \frac{815603 \times (\text{observed water-gauge in ins.})}{(\text{revs. per min.} \times \text{fan diam. in feet})^2}$$

which is applied to the whole of the fifty-six fans tabulated for comparison. The general results so arrived at may be summarized as follows:

Eighteen fans without casing—by MM. Combes, Detoret, Guibal, and Lambert—give an average of only 0.327 approximate manometric efficiency, the lowest being 0.090 and the highest 0.496. Five fans with casing, but without chimney,

Ventilator	Lalle.	Créal.	Bességes.
Circumferential speed, feet per minute	3,936	3,936	3,936
Initial vacuum $H = K\frac{u^2}{g}$ when producing no current, water-gauge, inch	1.041	1.089	1.314
Manometric efficiency, represented by coefficient K	0.542	0.572	0.691
Fan resistance, area of equivalent outlet b, square feet	30.086	37.868	43.831

Comparative Manometric Efficiency.—In a comprehensive table the author has collected for comparison the data respecting the working of all the fans he could, to the number of fifty-six. But as it has generally been only the fan speed that has been varied in the experiments recorded, while rarely has any variation been tried in the mine resistance, it is impossible from data so incomplete to deduce the fan resistance and the true manometric efficiency K. To the latter, however, a rough approximation is arrived at by substituting the effective vacuum h, observed under the ordinary circumstances of working, in place of the theoretical initial vacuum H which would exist if there were no current. In the case of the three fans previously considered, the extent to which the approximate results thus arrived at fall short of the true is seen from the following comparison:—

	Lalle.	Créal.	Bességes.
True manometric efficiency K	0.542	0.572	0.691
Approximate manometric efficiency	0.468	0.524	0.569

range from 0.429 to 0.682, the average being 0.560; but in one of these instances, where the same fan had previously been without the casing, its addition seems to have yielded no better result, and the author is at a loss to account for the anomaly. Four early Guibal fans, with casing and chimney, but the chimney parallel instead of expanding, range from 0.284 to 0.626; the average, even omitting two exceptionally low results, does not amount to more than 0.606, which suggests the question whether the chimney when parallel is really of any good, and whether the result is not due simply to the casing. With expanding chimney and regulating shutter, twenty-seven Guibal fans show an approximate manometric efficiency ranging from 0.545 to 0.751, excluding one exceptionally low result; the average is 0.650 In one of these instances—namely the Guibal fan of 23 feet diameter and 5½ feet width at Crachet and Picquery colliery, near Frameries, Belgium—where the result had been only 0.491 prior to the erection of the chimney, it was raised to 0.654 by the expanding chimney; while with an-

other Guibal fan of the same dimensions at Grand Buisson colliery, where the chimney had previously been parallel and was afterwards lined to the expanding form, the result was thereby raised from 0.626 up to 0.711. Two blowing fans of very good construction show results as high as 0.720 and 0.732.

Though larger dimensions of fan do not seem sensibly to augment its approximate manometric efficiency, yet the author suggests their great size as perhaps the cause of the high results presented by the three Guibal fans at Gethin colliery, Cyfarthfa, at Lwynypia colliery in the Rhondda valley, and at Pelton colliery, county Durham; these fans are each 30 feet diameter and 10 feet wide, and their approximate manometric efficiencies are respectively 0.661, 0.696, and 0.709, in spite of the very spacious airways through the workings they ventilate. On the other hand, the new Guibal fan of forty feet diameter and 8 feet width with radial blades, at Crachet and Picquery colliery, gives only 0.668, which though good is not so high as it would seem might have been expected; here however the object of so large a size was less to increase the efficiency than to get from a moderate speed of revolution a very high circumferential velocity, on which depends the actual vacuum that an exhausting fan can produce.

Since the volume of the air-current is what forms the ultimate practical aim in mine ventilation, and since this volume varies as the square root of the vacuum, the author points out that there is really but little scope left for further improvement upon the large exhausting fans now in use, which realize already an approximate manometric efficiency reaching 75 per cent. For even if their manometric efficiency could be brought up to the limit of theoretical perfection represented by 100 per cent., the corresponding increase in the ventilating current could not amount to more than 16 per cent. upon the current already obtained in practice.

Direct-Propelling Ventilators.—Distinguishing by this designation the various ventilators with helicoidal blades, like those of a screw-propeller or of a windmill, the author speaks of them as now fallen into disuse; he investigates their action, and shows that for the degree of vacuum and for the volume of the ventilating current the same formulæ which apply to centrifugal fans hold good here again also. The data respecting the working of fourteen mine ventilators of this class are tabulated for comparison; and their approximate manometric efficiency is found to be very low, the highest not exceeding 0.230. Besides being otherwise defective in construction, all more or less throttle the air in its passage through them. As with the fans, here also large diameters are conducive to efficiency. Though evidently so inadequate for mines, these ventilators answer well for the far easier ventilation of large halls; a pair of them employed in conjunction at the Tracadéro palace, Paris, produced together a current of 118,660 cubic feet per minute, with no higher pressure than 0.236 inch water-gauge, of which each singly gave only half; the area of the equivalent orifice representing the resistance encountered by the current in the building would thus be nearly 100 square feet. With improvements in construction to increase its efficiency, the author thinks a direct-propelling ventilator might in some cases be advantageously employed underground for opening out colliery dip-workings, where the air-current is often very weak; it would occupy but little room at the pit bottom, and the water falling into the sump could be utilized to drive a turbine fixed on the ventilator axle.

Mechanical Efficiency of Centrifugal Ventilators.—The useful work U done by any ventilator is simply the product of the effective vacuum h multiplied by the effective volume V of the air-current; or $U = h \times V$. Bearing in mind that, although the effective vacuum h falls short of the theoretical initial vacuum H which the fan would create if there were no current, yet the deficiency is due solely to friction and eddies in the passage of the air-current through the fan itself, it is clear that, however great its imperfections, the fan is really doing the larger work corresponding with the higher vacuum H, out of which only the portion h is useful or effective. Hence the expression for the driving power P, required on the fan shaft, will be $P = H \times V + L$, where L is the loss due to friction of bearings and to friction of the air against the ventilator casing;

the latter friction is ascertained by measuring the extent of stationary surfaces passed over by the current, calculating the mean velocity of the current rubbing against those surfaces, and employing an experimental coefficient of friction, the value of which, according to D'Aubuisson, would be 0.0032. Taking care to express L in the same terms as P, and to adopt the same unit of time for both of these as for V, the French and English formulæ become

$$\left\{\begin{matrix}\text{Driving power P}\\ \text{in kilogrammeters}\end{matrix}\right\} = \left\{\begin{matrix}\text{initial vacuum H}\\ \text{in millimeters}\\ \text{of water-gauge}\end{matrix}\right\} \times \left\{\begin{matrix}\text{air-current V}\\ \text{in cubic meters}\end{matrix}\right\} + \left\{\begin{matrix}\text{loss L}\\ \text{in kgm.}\end{matrix}\right\}$$

$$\left\{\begin{matrix}\text{Driving power P}\\ \text{in foot-pounds.}\end{matrix}\right\} = 5.2 \times \left\{\begin{matrix}\text{initial vacuum}\\ \text{H in inches}\end{matrix}\right\} \times \left\{\begin{matrix}\text{air current V}\\ \text{in cubic feet}\end{matrix}\right\} + \left\{\begin{matrix}\text{loss L}\\ \text{in ft.-lbs.}\end{matrix}\right\}$$

The useful effect E or mechanical efficiency of the ventilator will therefore be represented by the ratio of the useful work U to the driving power P; or

$$E = \frac{U}{P} = \frac{hV}{HV + L}$$

Since the air-current V varies directly with the speed of the ventilator, while the initial vacuum H varies as its square, it is seen that the principal term HV, in the expression for the driving power, varies as the cube of the speed; whence it is not so easy as it seems beforehand for the speed of a centrifugal ventilator to be varied to any large extent. Moreover, if the speed of running be maintained constant, the initial vacuum H becomes constant also; and then the power depends almost wholly on the amount of the ventilating current V. Hence arises the seeming paradox, that, if the mine resistance be augmented, less power is required and the fan speed quickens; whereas if the current have freer access to the ventilator, more power is needed and the speed slackens. Again, since the frictional loss L varies much less rapidly than as the cube of the speed, it follows that the useful effect improves as the speed rises; and this conclusion is borne out by all experiments in which different speeds of ventilator have been tried. Another inference from the much lower rate at which the loss varies with the speed is that with increase of speed the useful effect will rise to a maximum; and will afterwards fall off again, owing to the increasing friction of the current through the fan, and the consequently slower increase of the effective vacuum h than of the initial vacuum H. This is very clearly brought out in the experiments made by the Gard Commission, and shows that for every centrifugal ventilator there is a certain mine resistance which corresponds with a maximum of useful effect.

As a mine ventilator is generally driven by an engine working direct on its shaft, it but rarely happens that the driving power can be measured by a friction brake; an indicator on the steam cylinder is most commonly employed, and the loss L in the foregoing expressions then includes the engine friction, thereby causing the useful effect of the ventilator to appear lower than it really is.

Tabulating the lowest and highest results obtained from a number of different mine ventilators, with which repeated experiments have been tried under varied conditions of speed and mine resistance, the author collects the following percentages of useful effect:—

Kind of ventilator.	Per ct. of useful effect.		
	Lo'est	Hig'st	Mean.
Two direct-propelling ventilators...........	22.0	32.4	26.0
Five centrifugal ventilators without casing (including that at Lalle)..	16.0	47.0	27.8
Two fans with casing but without chimney.	17.0	39.0	28.4
Three fans with casing and parallel chimney (including that at Créal)	32.0	51.7	37.9
Twelve Guibal fans with shutter and expanding chimney (includ'g that at Bessèges)..........	23.0	74.9	46.7
Three blowing fans....	11.0	64.6	31.0

In the Gard Commission's experiments, included among the above, the useful effect ranged from 27.7 to 47.0 per cent. at Lalle, from 42.2 to 51.7 per cent. at Créal, and from 25.0 to 49.4 per cent. with the Guibal fan at Bessèges. The results in the first and sixth lines are the true percentages, free of engine friction; those in the four intervening lines all include the engine, and thus fall somewhat short of the fan's true useful effect. The

experiments on the twelve Guibal fans were sixty-five in number; and as the lowest percentages were here obtained from trials made under exceptional and very unfavorable conditions, the author considers that 50 per cent. may safely be taken as representing for ordinary working the mean useful effect of fan and engine combined. With the 23 foot Guibal fan at Crachet and Picquery colliery the useful effect ranged from 16 to 22 per cent. without the casing; from 17 to 31 per cent. when the casing had been added, but without shutter or expanding chimney; and after these had also been added, it ranged from 30.7 up to 74.9 per cent. in fourteen experiments made under great variations of mine resistance. The fellow Guibal fan at Grand Buisson colliery gave a useful effect of from 32 to 33.4 per cent. with its originally parallel chimney; and afterwards, with expanding chimney, the range was from 42.6 up to 72.5 per cent.

Practical Conclusions.—Theory and practice concur, the author considers, in pointing to the Guibal centrifugal fan as the best exhausting ventilator. Its approximate manometric efficiency has been found to average 65 per cent. of perfection. Assuming this coefficient, from the effective vacuum desired is then deduced the proper speed for the tips of the fan blades, which constitutes the main datum for the construction of an exhausting ventilator. Whether this speed shall be obtained by a small quick fan, or by a larger and slower, is left free to be decided by considerations of room, cost, and mechanical simplicity; only guarding against any risk of the fan presenting too small an area of passage through itself for the current necessary in a large mine. As to the number of fan blades, the author believes there is every advantage in having them numerous, because the air is thereby better guided, while the strain on the blades and their tremor are less; the only limitation is that their aggregate thickness should not be so considerable as to throttle the passage of the air-current through the fan. For the power necessary to drive the fan, it will be safe enough simply to reckon this at double the useful work required to be performed in the ventilation of any mine, since the useful effect of the Guibal fan has been found to average about 50 per cent.

In appended notes the author amplifies certain portions of his theoretical investigations, and substantiates certain of the data he has employed. He also compiles a table of values ranging between 1 inch and 1⅝ inch water gauge for the theoretical initial vacuum which would be produced by a perfect fan running at any circumferential speed between the practical limits of 3,000 and 7,000 feet per minute.

THE LOAD-LINE FOR SHIPS.

"The Nautical Magazine."

Connected with merchant shipping, there probably never was a subject, having a greater degree of interest to all concerned in that great national industry, than the complicated and vexed question of freeboard—that point or mark upon the side of a ship to which she may be safely submerged, and beyond which it is avowedly dangerous to trespass.

It is, perhaps, one of the few things about a ship—even if all ships were alike in model—which cannot be determined by high scientific calculations; not, certainly, till such methods can further inform us on certain obscure points connected with wind and water; as, for example—first, how long and how *strong* a gale may blow, continuously, from *one* quarter of the compass; and, secondly, what elevation ocean waves may gather under such conditions, how fast they follow, and how abrupt their hollow and curling crests. Even with all such necessary information, probably not the wisest or most eminent of mathematical *savans* could, with all his learned figuring, advance us one little point beyond our present knowledge of the subject.

This present knowledge is said to be small, it is likely to continue so, if further explorations are to be conducted in the

mazy labyrinths of intricate calculations, as to the various altitudes and volume of wave crests, as, when driven by the storm, they sweep on the decks of deep or moderately loaded ships.

There is one important element in this still more important matter, which in the somewhat over-refined investigations hitherto conducted, has been a little, if not altogether, overlooked, and that is the condition and strength of the decks and their various openings and fittings; for it may be laid down as a most certain, self-evident fact, that if the decks are not thoroughly secure, the most liberal allowance of freeboard is thrown away. But little acquaintance with ships in their character of floating bodies is necessary to recognize the assurance that with but half an inch of "freeboard" a vessel will securely float, and continue to do so as long as her hull or her decks are strong enough to resist the battering effect of heavy seas—just as long, in fact, as she is secure against the invasion of water or other additional weight. There is one thing that we know, or should have learnt from the vast experience of the last ten or twenty years—an experience perhaps as gloomy and unsatisfactory as it has been rash—that the majority of "cargo steamers," as at present constructed and sent to sea, have already, long since, reached the limit of safety in loading, if, indeed, many of them have not got much beyond it. If this be not so, how are we to account for the great number of such vessels which annually disappear? There is, surely, unmistakable evidence to prove that something is wrong *somewhere*, for many of them are comparatively new ships. After two long centuries of experience of all kinds of ships, and over all seas, a vanished school of able and sagacious seamen laid it down that a good ship, fairly loaded and ably commanded, will live in any storm, excepting, perhaps, cyclones and hurricanes, and the accidents which they engender; she positively cannot sink, but is as certain to ride over these great rolling mountains of seething water as a well built church is of standing on its foundation.

This does not, however, apply to the all present generation of iron steamers; it had reference, especially, to first-class wooden ships, such as were the glory and pride of England some fifty years ago; ships which invariably carried high and bold sides. The experiences, however, of the present age of seamen, respecting many of the ships which now convey our commerce would lead nearly to the same happy conclusions. Take, for example the steamships of the great companies which traverse the wild and stormy Atlantic, than which there is no ocean more tempestuous, and we find that over a long period of years there has been, as far as we actually know, no loss from this cause —of foundering. A company, whose ships are as far above suspicion as was Cæsar's wife, can apparently truthfully declare that during upwards of forty years exposure on this ocean they have never lost the life of a single passenger by a case of foundering. So much for large first-class passenger steamers. Now, on turning to small sailing ships, what state of things do we find there? We are told by captains of yachts, decked fishermen, pilot boats, even many open boats, and also by the officers of small trading vessels, ranging from 100 to 300 tons, that when "staunch and strong" as they should be, and *fairly* loaded, they consider them even safer than big ships, and vastly drier, in a seaway.

It is also, we know, a common thing for open boats, heavily loaded with their crews, to live in a sea which has swallowed up the parent ship. It is further on record that Cook, on being given his choice of a ship for his voyage in quest of an Antarctic continent, put his hand on a small brig as being, in his opinion, *most capable* of wrestling with the howling storms of an untraversed and remote sea. Bearing all this in mind, there are only, mainly, about two conclusions to be come to respecting the enormous number of ships which now founder in the open sea or are missing. The first is, that from various causes, they are not seaworthy in point of construction; and, the second, that if seaworthy in this respect, they are overloaded. There are few men who understand what a ship is capable of doing who would care to assure her safety during a winter passage with but 18 per cent. of buoyancy. Not that this amount is in all cases too little, but for such extreme submergence there should be special models of ships, and special methods of deck formation and

construction with more length and greater submersion. There *must be* reform of model above board, and greater strength. This is as "certain as mathematics."

A cask of wine, with its top just awash, will float through many heavy gales and yet preserve its contents in good order; and any ship would do the same, if *strong enough*; thus it would appear that the solution of this matter is to be sought for as much in increased strength and security of decks, as in any other direction. There must, however, be no mistake made as to the *degree* of strength; for, to make a steamer of 1,000 tons as strong, proportionately, as a well-coopered, level wood cask—in such a situation—and to render her decks as free from obstructions, would simply be to make her next to useless as a freight earning machine. She must first, at all events, have a funnel, with its accompanying uptakes, ventilators, &c., and something in the shape of a bridge, and more or less of a deck, with hatchways and other indispensible fittings.

It would also be discovered by those who made the trial, that to submerge her further than 82 per cent. of her midship volume, notwithstanding the *many questionable* advantages of great sheer, would render a complete and radical change necessary, in the planning and fitting of her decks. A case has already been cited in this journal of a new steamer, on her first voyage, falling off into the trough of the sea and having her hatchways completely knocked in by the first sea which fell aboard, causing the immediate loss of that ship by foundering, the probable fate of great numbers which disappear.

Five and twenty years ago when ships, by having less length and greater freeboard, were far more bouyant than at present, it was a common spectacle, during a gale and heavy sea, to see substantial deck fittings brushed away like loose dust; and on board of ships which had been carefully loaded in accordance with Lloyd's rules. Piling timber on timber, and bolt upon bolt, is but labor in vain in the shape of hatchways and houses. Hardly anything in the guise of wood in low-sided ships can stand the breaking charge of a heavy sea when once getting above the hatchways. *Iron* might even be included in this category, when it is remembered that the substantial metal attachment of a heavy bell on the top of the Bishop Rock Lighthouse, 110 feet above high water springs, was broken off short by a "spray" from a S.W. sea, and even but a month ago, one of the great mail steamers, during a moderate gale, had her reefed mizen swept entirely away by a sea which fell right into the belly of the sail; the *boom* of this mizen was just 20 feet above the load-line.

Such, indeed, are old stories, but, nevertheless, true ones; they may serve, perhaps, to impress upon incredulous and virgin minds the fact that ships should be constructed to ride *over* the waves instead of through them. If we are going to sail our ships *under* the waves, we may begin as soon as we please to smooth them off on deck like the bilge of a wine cask, that is to say, if we want them to come home again. The vessels already mentioned represented the marine of this country in its transition state, from the stately old castles of the East India Company to the present iron cargo steamers; such ships when running before a gale and a true long rolling sea, would occasionally ship from 150 to 200 tons of water, which would come rolling in on each side as the crest of the wave swept by. Having from five to six feet of freeboard, and about five feet of bulwark above it, and although such buoyant fabrics compared with the present, they frequently found themselves in great jeopardy, and such a thing as a main-hatch giving way was not unknown.

It is a great mistake to suppose, as some ship-builders really do, that because a ship is *big* no heavy seas will ever run on board; in consequence of this fallacy they take all manner of liberties in designs and construction. It is difficult to believe there are such opinions in such quarters; yet it is so, notwithstanding. There is doubtless, less science imported into shipbuilding now than in former times, when iron for such work was a novelty, and when the worthy blacksmith, though occupying an indisputable position, played only second fiddle in the symphony; but the merest riveter should understand that the long, *low* steamers which are now turned out of our building yards are the wettest, and

in too many cases the most dangerous ships which ever put to sea.

A great ocean wave, however high or fast it may run, will lift up, bodily, any small ship or boat, but not so some 400-footer; and, as a matter of course, if it cannot lift, must run over some part of her.

Allowing such a wave to run at 35 miles an hour, and a great part of its crest, say 25 tons of water, to overwhelm her decks, we have at once a force equal to the *charge of a locomotive* against everything in the shape of an obstruction to its course. Yet it is common to see such ships putting to see with all kinds of trumpery and feeble fittings—such as obtained in high-sided ships of fifty years back—*feeble*, it should be said, in relation to the amount of freeboard, or bad weather to be encountered; and afterwards doleful accounts in the newspapers about "terrific weather, and fearful damage."

It is, perhaps, unnecessary to mention here how many large ships have lately foundered in home waters from such cause, and how their heavily loaded open boats have survived the ordeal which the parent was unequal to.

Respecting the great question of freeboard, it is highly probable that if a dozen of the most experienced and intelligent captains of, say, some of the Black Sea steamers, were about to insure one of their vessels against a winter passage, they would unanimously stipulate for the following conditions:

1st. That if about 300 feet long, she should have no less than 20 per cent. of buoyancy of midship volume, measured from the lower part of sheer of main deck, on leaving port.

2d. Moderate sheer, and camber of deck.

3d. To have poop and forecastle, and midship upper deck.

4th. The two former to be constructed more for throwing off heavy seas than for any other purpose; with windlass on main deck; extreme elevation of such constructions to be rather *under* than over the average, with great camber to their decks.

5th. Midship upper-deck to enclose engine and stoke-rooms, and all openings or passages thereto to extend from side to side of ship, with watertight door for passages, if desirable; and this upper-deck to have a length of from $\frac{1}{4}$ to $\frac{3}{10}$ of the extreme length of ship; to accommodate all or greater part of crew, and to be, in fact, for the most assailable and weakest part of the ship a fortress against water.

6th. Two feet of substantial iron bulwarks, with the same amount of guard rail above it.

7th. To have most substantial and approved deck-fittings, calculated to stand a strain of at least two tons to the square foot.

8th. Coamings of hatchways 30 inches high, and the hatches themselves fully as strong as the decks, with portable iron beams to support fore and after.

Such would, doubtless, be some of their most important requests.

No ship should have *any* allowance for midship deck erections, which did not embrace all the conditions of Art 5.

It is difficult to see why any allowance should be made for closed-in poop and forecastle, they *do* throw off end-on water, but not beam-seas, which are the most dangerous. Indeed, nearly any steamer might be overwhelmed and broken up amidships, *before* the supposed buoyancy in such end constructions began to act; and until they *do* begin to act, that is to say, until the water reaches them, they do actually by their simple weight oppress her.

If, for example, she is thrown on her beam ends, or over to 45°, which is about the most usual angle, they all the time are bearing her further over, and when their buoyancy did begin to tell, supposing they are closed in, the ship would probably, with her engine room full, be beyond relief.

The engine room of such a ship would, in event of an accident, begin to fill when over at an angle of 20°, but the buoyancy confidently looked for in the end erections would not commence till she was over at an angle of about 80°, or just on the point of sinking. Even with an end-on sea, when the crest was about amidships, and attacking and breaking up this vital part, these heavy erections are in the trough, and all the time bearing her down, like the uplifted arms of a drowning man. The allowance made for great sheer puts the ship very low in the water, *in* this vital part, and which in the

battle has to stand the brunt of the attack. In driving hard, a bow that will readily throw off water is better than a necessarily high one—for one or two feet of extra height make no appreciable difference in a heavy head sea; but such vessels do not "drive hard," and what they want more than any other thing is some kind of certain security, or sanctuary, when lying to or when things come to their worst. In lying to under sail they are comparatively dry forward and aft and very wet amidships, and invariably weakest at this, their supreme point of stability and buoyancy. By having a moderate sheer the poop and forecastle do not so powerfully affect the stability in lurching or pressing. But the great object in all such vessels, indeed in long vessels of any kind, is to have perfect strength and security amidships, for during a heavy beam sea that part of the ship can neither rise nor give way so readily as the ends when charged from windward. On the lee side a very few degrees of "list" either as a heavy lurch or pressed over by canvas, quickly puts the low-moulded main-deck under water, when of course buoyancy in that part ceases, and the first process of sinking or capsizing actually begins.

The upper and *complete* midship deck is a safeguard against this very liable accident, or, more properly, natural evolution. And it is not only the grain steamers which shift their cargoes by their constant lurching, for cargoes of all kinds are now taken in so hurriedly that a succession of heavy lurches during a gale of wind will speedily give any ship a very undesirable list, simply by the constant sag-sagging of a moderately slackly stowed cargo. Under such conditions the coals in the bunkers will themselves contribute four or five degrees

But to return again to freeboard, perhaps the most certain and simple way of coming quickly to a practical conclusion is to do as the Admiralty did with their monitors. Have a steamer ready, of the usual make up and trim, with a party of commissioners—royal or otherwise—on board, and waiting in some convenient harbor for a gale of wind; then let her go to sea, and let the gentlemen have, then and there, ocular demonstration of her performance. By carefully selecting from many well-known specimens, some possible basis may be secured upon which to work out other calculations. But whatever conclusion is come to, it is fully time that a great and rich maritime nation put an end to such grievous and discreditable loses which are now nearly daily posted up. Independently of the destruction of national wealth and prestige, it is scandalous that the children of sailors shall be made orphans, and their wives widows, in order to save to the well-fed millions of this prosperous country a paltry halfpenny in the price of a loaf, or sixpence in the cost of a coat. For in the supposition that freights would soon find their normal level, such is almost the only gained advantage; and nobody would be more desirous of seeing ocean steamers made as secure as the machinery of other great and successful manufactures than the general mass of honorable shipowners of Great Britain.

It has already been intimated that a most successful body of shipowners, during forty years of trading, have never lost a ship by foundering; and the same thing is still going on—without Government assistance.

It is useles to talk about "handicapping" English ships, for few ships load so deep, run such risks, or pay so well as those under discussion. They are, in fact, the "leaders of fashion" in this matter.

The thing can be done as surely, and will *pay* as surely, as it pays one, out of many great manufacturers, to have tennis grounds for his young men or grand pianos for his young women; or on the other hand, as it pays a successful costermonger to have *weight-worthy* wheels to his cart. As a rule, the more secure, and safe and perfect that the machinery of all industries *becomes*, the more successful are the manufacturers.

If anybody doubts this, he must at least admit that the harvest wagon wheels of a great and rich nation must be made secure, and the lives of the wagoners as safe as human hands and heads can devise.

There is a regular telephonic service on some of the Austrian secondary railroads, so that all of the stations can communicate with each other.

ON THE ADHESIVE STRENGTH OF PORTLAND CEMENT WITH SPECIAL REFERENCE TO AN IMPROVED METHOD OF TESTING THAT MATERIAL.

By ISAAC JOHN MANN.

From Selected Papers of the Institution of Civil Engineers.

This communication refers principally to an investigation of the adhesive or cementitious strength of Portland cement, and a description of improved method of determining its quality and value, with a view to establish a simple and generally recognized standard system of testing.

The author believes that it may be fairly assumed that the principal function of cement is to produce adherence, so as to convert loose or disconnected material into a solid coherent form; and that it may be further assumed that the economy with which this object can be gained will, *cæteris paribus*, be in proportion to the adhesive strength of the cement employed.

Reviewing the history of cement-testing, and the experimental researches connected with it, the conclusion seems almost inevitable that neither of the systems at present in general use—one depending on the cohesive strength of neat cement, the other on that of a mixture of cement and sand—are likely to become universal.

The principal test adopted by the author is one of adhesion. It has been used by him for some years with very satisfactory results, and he believes it will be found to possess elements which recommend it strongly for general adoption. The mode of applying the test is extremely simple, involving neither skill nor experience on the part of the operator, and most, if not all the complications of details incident to other systems can be avoided. The simple and inexpensive testing machine designed by the author consists of a steel lever with a knife edge bearing on the fulcrum; the strain is applied by a thumb-screw, and registered by an accurate spring-balance, the dial of which is provided with a maximum index-hand; the samples are cruciform in shape, and consist each of two pieces of sawn limestone or ground plate-glass, $1\frac{1}{2}$ inch by 1 inch, by $\frac{1}{4}$ inch to $\frac{3}{8}$ inch. When a sample is to be broken, it is placed on two vertical supparts under the end of the short arm of the lever. An adapter, with a steel center-point fitting into a small conical recess at the end of the lever, enables the strain to be brought on the sample with great accuracy and facility, and a cushion of soft wood or india-rubber fixed in the adapter checks the recoil after fracture. The center-point can be provided with a screwed end, to permit adjustment in case the cement joint should inadvertently be made thicker than necessary: this, however, is an unlikely contingency. Limestone or ground plate-glass are suitable for standard tests; the former because it is readily obtainable, of sufficiently uniform texture, and enters largely into construction; the latter on account of its being homogeneous and easy to obtain.

The cement to be tested is gauged to a suitable consistency, and applied with a spatula to one of the test-pieces; the other being placed in position, a slight pressure is sufficient to squeeze out the superfluous cement, which forms a fillet tending to protect the extreme edges of the joint from any "wash" that might occur in the act of immersing the sample, or at any time previous to setting, and which can be removed after the cement has set. When the samples are made they are numbered, and at once placed in water in shallow vessels, which are also numbered, or lettered, to facilitate identification. After fracture the remaining cement is removed from the test-pieces, which can be used as often as required.

The transition from the German or mortar test to one of adhesion is not so abrupt as would appear: in the former a great number of small pieces of stone are used, in the form of sand; in the

latter two pieces only are employed, and the test is thus simplified and better defined. The German test, however, is neither one of cohesion only nor of adhesion, but involves an indeterminate proportion of each, which cannot be considered desirable.

In his investigations of the cementitious or adhesive strength of cement, the author was necessarily obliged to devote considerable attention to the influence of the coarse particles. The test of adhesion alone, even without any qualification as to grinding, would be almost sufficient to determine the practical value of cement, the weakening effect of the coarse particles becoming at once apparent, as will be seen from the following tests:—

Table I.—Effect of Coarse Particles on the Cementitious or Adhesive Strength. Age of samples, twenty-eight days.

Percentage of coarse particles	0 fine only	20	40	80	100 coarse only
Average adhesive strength in lbs. per square inch.	101	84	57	34	18

Percentage of coarse in the unsifted cement, 49. Cohesive strength of the same after seven days, 430 lbs. per square inch. Number of tests, twenty. Test-pieces, sawn limestone.

The coarse particles referred to above were those stopped by a silk sieve of 176 meshes to the lineal inch, the meshes being about 0.004 inch by 0.004 inch; a sieve of such fineness, although not heretofore used in ordinary testing, has been found to afford more definite and reliable results than those having larger meshes.

Another series of tests gave the following results after seven days' immersion; the same sieve being used to produce the required degree of coarseness:—

Fine cement only	91	lbs. per sq. in.
25 per cent. of coarse particles	63	" "
75 " " "	26	" "
Coarse particles only	8	" "

Cohesive strength of the cement as supplied, 532 lbs. per square inch. Age, seven days.

Although these, and numerous other tests of a similar character, have been sufficiently satisfactory and conclusive, the presence of coarse particles may be regarded as introducing an element which might possibly help to interfere with perfect uniformity in results, and which might therefore with advantage be eliminated in a standard test; a simple and obvious alternate will be found sufficient to overcome the objection referred to.

It can hardly be doubted that the strength of a joint made with ordinary cement must be influenced by the fortuitous position which the coarse particles occupy; or, in other words, by the proportion of inert, or comparatively inert particles which happen to be in direct contact with the cemented surfaces. In accordance with this view, the author proposes that one of the standard adhesive tests should be applied after the inert particles have been removed by sifting through a standard sieve.

Cement, as at present received from the manufacturer, so far as concerns the cementitious strength capable of being developed in the period to which ordinary testing must of necessity be limited in practice, consists of a mixture of active, and inert, or extremely sluggish material, and the latter may be considered almost as foreign to the true cementitious portion, as so much sand.

This view is supported by the following experiment: The coarse particles stopped by a No. 176 sieve, amounting to 49 per cent. of the cement as supplied, were removed, and sand of approximately the same granulation substituted; the average adhesive strength of this compound or mortar, compared with that of the ordinary cement after seven days' immersion, was—

Sand and fine cement	49	lbs per square inch.
Ordinary cement	56	" "

Test-pieces, sawn limestone.

Separation of the inert and active portions would be manifestly desirable, as forming, in combination with an adhesive test of the latter, a simple means of arriving at a true estimate of both the cementitious and commercial value of any given cement, in much the same way as the value of an ore is estimated by the percentage of metal it contains. The author's attention was therefore directed to this part of the subject, with a view to discover, if possible, the degree of pulverization required to convert the

underground clinker into active cement, capable of developing its cementitious properties within the period allowed in practice for testing purposes.

Some of the principal results of the experiments, up to the present time, are contained in the following table; unfortunately others applying to lengthened periods had to be discarded, owing to accidental exposure to frost, which in many cases seemed almost te destroy the adhesive strengh. In several instances the coarse particles were rapidly washed in two or three waters, tô remove a minute quantity of fine cement which adhered to them after sifting. The No. 176 sieve, used to separate the coarse particles, was the finest that could be obtained, arresting from 38 to 50 per cent. of the ordinary cement. No. 103 sieve retained from 25 to 30 per cent.; the unsifted cements required from forty minutes to five hours to set in air, the temperature of which varied from 50° to 70° Fahrenheit. They were obtained from well-known manufacturers, and had an average cohesive strength, after seven days, of 425 lbs. per square inch, their average adhesive strength being 61 lbs. per square inch, and 84 lbs. per square inch after seven and twenty-eight days' immersion respectively.

A set of samples eleven months old, made from cement passing a No. 54 sieve and stopped by a No. 103 sieve, but not washed, and having therefore a minute quantity of very fine cement adhering, showed an average adhesive strength of 21 lbs. per square inch.

The figures given in the table are in some cases the average of six tests, but in general that of three. In these, as in all the other experiments on the adhesive strength recorded in this paper, the test-pieces, unless otherwise stated, were of sawn close-grained limestone, and were placed in fresh water immediately after being cemented together.

The above examples, although far from exhaustive, lead to the following conclusions:—

1. That so far as concerns a seven-days' test, the particles of cement stopped by a No. 176 sieve developed little or no cementing power during that period, and that even some of the less fine particles passing the sieve may be very deficient in cementitious strength.

Table II.—Adhesive Strength of the Coarse Particles of Portland Cement.

Age.	Average Strength in lbs. per sq in.	Degree of Pulverization.
7 days	1	Stopped by No. 176 sieve and washed.
7 "	0	54 per cent. of fine particles removed by washing only.
7 "	6	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
8 "	8	Particles which passed No. 176 sieve, after very fine had been removed by ten minutes' sifting.
8 "	0	Stopped by No. 176 sieve and washed.
8 "	6	Fine removed by No. 158 sieve; very coarse by No. 54 sieve.
9 "	0	Stopped by No. 103 sieve.
9 "	½	Stopped by No. 176 sieve and washed.
28 "	12	Particles which passed No. 176 sieve, after 46 per cent. of fine had been sifted out.
28 "	14	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
28 "	5	Stopped by No. 176 sieve.
28 "	18	" " "
28 "	20	Fine removed by No. 176 sieve; coarse by No. 103 sieve.
28 "	1	Fine removed by No. 103 sieve; very coarse by No. 54 sieve, and washed.
28 "	9	Stopped by No. 176 sieve and washed.
28 "	14	Fine removed by No. 158 sieve; very coarse by No. 54 sieve.
28 "	18	Fine removed by No. 176 sieve; very coarse by No. 54 sieve, and washed.
28 "	0	Fine removed by No. 176 sieve; coarse by No. 103 sieve.
28 "	2	" "
28 "	3½	Stopped by No. 176 sieve and washed.
28 "	24	Passed No. 176 sieve, after very fine had been removed by sifting for two minutes.
15 weeks	20	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
15 "	14	Stopped by No. 176 sieve and washed.

Nnmber of tests, eighty. Test-pieces, sawn limestone.

This was further shown to be the case by the following tests: A weight of about

300 or 400 grains of ordinary cement was placed in the seive (No. 176); after sifting gently for thirty seconds, a quantity of the very finest passed through, which was made into samples, the cement passing through the sieve during the following two minutes was rejected, the sifting being continued until sufficient cement to make four samples had passed, leaving, however, a large portion still in the sieve. The adhesive strength of the former samples was much greater than that of the latter when tested after seven days' immersion. This was repeated, using the cement of other manufacturers, with the same result.

2. That so far as concerns a twenty-eight days' test, the cement of different manufacturers varied in the cementitious strength of the particles stopped by No. 176 sieve, from nothing to 20 lbs. per square inch, their strength increasing but slowly in the longer periods, and probably becoming soon exhausted.

Vo. 176 and No. 103 sieves were of silk, the meshes being respectively about 0.0040 inch and 0.0075 inch square, and slightly smaller than those of the same numbers formed of wire. The author has since, through the courtesy of Dr. Michaelis, of Berlin, been furnished with a woven brass wire sieve, of almost the same fineness as No. 176 silk sieve. The time required to sift 400 grains weight, using a sieve about 4 inches in diameter, varied from fifteen to twenty minutes, the operation being facilitated by the addition of a few small round pebbles.

If any general conclusion can be drawn from the table, which represents only eighty tests, it would appear to be that the cementing energy of coarse particles develops much more slowly than that of the fine particles. The necessity of adopting a high standard of pulverization is also shown. For example, the particles which were sufficiently fine to pass a sieve of ten thousand six hundred meshes to the square inch, viz. No. 103, possessed less than one-fifth of the cementitious value of those passing a sieve of thirty-one thousand meshes per square inch, viz. No. 176.

The adoption of a high standard of pulverization need cause no apprehension to manufacturers, as the percentage of cement stopped by a standard sieve, and the price, can be mutually accommodated to meet all the commercial exigencies of the case.

The degree of pulverization of the cement supplied by some of the principal manufacturers fluctuates between comparatively narrow limits, as may be seen from the following table:—

Table III.

Maker.	Percentage by weight stopped by a No. 176 sieve	Maker.	Percentage by weight stopped by a No. 176 sieve
F	49	L	49
D	47	S (quick setting)	41
G	45		
G	46	S (slow setting)	47
H	43	Y	38
I	47		
K	50	General average	.. 45.6

A slight modification only is required to adapt ordinary specifications of cement to the terms proposed by the author.

The next point requiring investigation is the adhesive or cementitious strength of the fine particles. For this purpose the finest sieve obtainable, viz. No. 176, was used, and the cement sifted until an inappreciable amount escaped through the meshes.

In the two following tables will be found some of the principal results ob-

Table IV.—Adhesive Strength of Fine Cement Sifted through No. 176 Sieve. Age, seven days. Test-pieces, sawn limestone.

Manufacturer ..	A	B	B	C	D	D	E	B	C
Cementitious strength in lbs. per square inch	61	101	101	69	73	91	85	100	84
Manufacturer ..	A	F	G	A	H	I	K	F	G
Cementitious strength in lbs. per square inch	70	57	65	74	63	83	66	81	82

Number of tests, sixty-two. General average, 78 lbs. per square inch.

Table V.—Adhesive Strength of Fine Cement Sifted through No. 176 Sieve. Age, twenty-eight days. Test-pieces sawn limestone.

Manufacturer.	H	C	D	I	B	H	F	L	L	K
Cementitious strength in lbs. per sq. in.	71	121	66	84	110	100	77	88	109	105

Number of tests, thirty-eight. General average, 93 lbs. per square inch.

tained from cements of ten different manufacturers, requiring from forty minutes to five hours to set in air. Extremely quick and very slow-setting cements were reserved for Table XII.

Considerable differences are apparent, not only in the cements of different manufacturers, but also in different cargoes from the same maker, and occasionally in the same cargo. Most of the tests relating to longer periods were unreliable from exposure of the samples to frost; a limited number, which escaped exposure to frost, had an average adhesive strength of 116 lbs. per square inch after thirteen weeks' immersion, while others of a different manufacturer, which had been in water for six months, had a strength of 113 lbs. per square inch. A set of samples, which had been some months immersed before being frozen, and therefore better able to resist injury from this cause, had an average cementitious strength, after fifteen months, of 173 lbs. per square inch, one of the samples exhibiting the highest development of adhesive strength which the author has yet observed, namely, 240 lbs. per square inch. The weight of fine cement from which this cementitious energy was developed did not exceed 5 grains. This strength is equivalent to nearly 16 tons per square foot; but few, if any, masonry-joints are ever subjected to a tearing strain of this severity.

Owing to the author preferring to make all the tests himself, and to the limited time at his disposal, the averages represented by each of the figures in the tables have been derived from three to six tests. In ordinary testing, however, it would be desirable to take the averages from not less than six tests.

Although the test proposed to be applied, after removing the inert particles by an extremely fine sieve, will probably give the most trustworthy and uniform results, the adhesive test should be also applied to the cement as received from the contractor, and as actually used in construction, this test alone being almost sufficient to enable a correct estimate to be formed of the value of the cement.

In the following tests of the cementitious strength of ordinary cement the extremes of quick and slow setting were avoided. In every case, unless otherwise stated, the cement was immersed in fresh water immediately after the test-pieces were joined:—

TABLE VI.—ADHESIVE STRENGTH OF ORDINARY CEMENT AS RECEIVED FROM THE MANUFACTURER. Age, seven days. Test-pieces, sawn limestone.

Manufacturer.	G	K	I	F	F_1	G_1	G_2	H
Cementitious strength in lbs. per sq. in.	76	57	51	69	71	73	59	56
Manufacturer.	G_3	W	F_2	W_1	W_2	G	F_2	K_2
Cementitious strength in lbs. per sq. in.	52	50	54	59	41	37	51	56

Number of tests, sixty. General average, 57 lbs. per square inch.

The cements of well-known makers were used, and with one or two exceptions the results are tolerably uniform, considering that nearly 50 per cent. of the material tested was practically inert, and its position relative to the surfaces of the test-pieces uncertain. The highest and lowest figures in the table were obtained from the same cement; it was however the slowest in setting, and nearly twelve months elapsed between the two tests. The test-pieces should in every case be scrupulously clean, and allowed to remain in water for a short time before using.

Table VII. shows some of the principal results obtained from a twenty-eight-day test of ordinary cement.

TABLE VII.—ADHESIVE STRENGTH OF ORDINARY CEMENT AS RECEIVED FROM THE MANUFACTURER. Age, twenty-eight days. Test-pieces, sawn limestone.

Manufacturer.	H	H_2	K	K_1	F	F_1	W	W_2	G	G_1
Cementitious strength in lbs. per sq. in.	78	57	48	71	98	69	91	84	103	75

Number of tests, thirty-six. General average, 78 lbs. per square inch.

Estimated by their cohesive strength the cements were all of good quality. The differences are not so great as those in the preceding table, and no doubt can be partly accounted for in the same way. Of course the cements of different makers could not be expected to show perfectly uniform results, but the averages would probably be only slightly altered if obtained from a larger number of tests.

Table VIII. shows the results of a limited number of tests after thirteen weeks' immersion of the samples.

TABLE VIII.—ADHESIVE STRENGTH OF ORDINARY CEMENT, AS RECEIVED FROM THE MANUFACTURER. Age of samples, thirteen weeks.

Average strength in lbs. per square inch.	Remarks.
110	Test-pieces, sawn limestone. Samples made with and immersed in sea-water.
113	Test-pieces, ground plate glass. Fresh water.
60	Test-pieces, sawn limestone. Fresh water.
110	Test-pieces, sawn limestone. Fresh water.

Average 98 lbs. per square inch.

A series of samples which had been immersed for seven months had an average strength of 128 lbs. per square inch. Another set which had been frozen, had a strength after twelve months of only 40 lbs. per square inch, while some samples of the same cement, but with the coarse particles removed by a No. 176 sieve, although similarly frozen and of the same age, had an average strength of 107 lbs. per square inch. The comparative cementitious strength of the sifted and unsifted cements tested by the author is given in Table IX.:—

TABLE IX.

COMPARATIVE CEMENTITIOUS STRENGTH OF SIFTED AND UNSIFTED CEMENT.

Description.	Averages in lbs. per sq. inch.		
	Age, seven days.	Age, twenty-eight days.	Age, thirteen weeks.
Cement with the coarse particles removed by No. 176 sieve....... .	78	93	116
Ordinary cement as received from the manufacturer..........	57	78	98

These results are satisfactory, particularly with reference to their bearing on the subject of the deposition of concrete or rubble-in-concrete, in deep water, a method of construction which can be applied with such economy and rapidity of execution as to supersede the slow and costly methods which involve the use of cofferdams or large blocks. The adhesive strength shown by the author's experiments indicates the great stability and monolithic character of structures in which Portland cement is employed as the cementing material, and immersed before setting. In several instances when the test-pieces consisted of comparatively soft but otherwise sound stone, such as Portland and sandstone, the adhesive strength of the cement was sufficient to tear small fragments out of the surfaces of the test-pieces, the age of the samples being only twenty-eight days, and the breaking-strain slightly below the average of ordinary unsifted cement (Table XV.).

In drawing any inference from Table IX. it should not be forgotten that to produce the strength shown in the second line of figures involved the use of, at least, four times the quantity of cementing material required to produce the superior strength shown by the figures in the first line, or those referring to the sifted cement, owing to the difference in the thickness of joint caused by the presence of the coarser particles. The averages given above are derived from tests applied at various times to the cements of twelve different makers. An abstract of some of the principal tests applied to the same cement, or that made by one of the manufacturers, is given in Table X. (page 239).

The relation, if any, between the cohesive and adhesive, or cementitious strength of neat cement seems to be extremely obscure, as might be anticipated from the fact that the presence of coarse particles within certain limits increases the former but diminishes the latter. The following examples will serve to show that the ordinary seven-days' test of tensile or cohesive strength is unreliable as an exponent of the cementitious value. (Table XI., page 239).

The author believes that the cohesive test has not unfrequently led to the rejection or condemnation of excellent cement. For example, in experiment No. 9, Table XI., the low tensile strength of the cement would probably have caused

TABLE X.—ABSTRACTS OF PRINCIPAL TESTS APPLIED TO THE SAME CEMENT. Test-pieces, sawn limestone.

Age.	Average adhesive strength in lbs. per square inch.	Remarks.
1 day...	15	Ordinary cement.
3 days..	36	" "
7 " ..	76	" "
7 " ..	73	" " 2d set of samples.
7 " ..	91	Fine only, separated with No. 176 sieve.
7 " ..	63	Fine, with 25 per cent. of coarse added.
7 " ..	26	Fine, with 75 per cent. of coarse added.
7 " ..	8	The coarsest particles that passed through No. 176 sieve.
7 " ..	0	Stopped by No. 176 seive
28 " ..	1	" " " another set of samples
28 " ..	88	Ordinary cement.
28 " ..	84	" " gauged with and immersed in sea-water.
" ..	120	Ordin'ry cement gauged with and exposed to air and sea-water each alternate day.
15 weeks.	20	Fine, removed by No. 176 sieve, very coarse by No. 54 sieve.
15 " ..	14	Stopped by No. 176 sieve and washed.

Average strength of cohesion of the ordinary cement is supplied after seven days, 480 lbs. per square inch. Pulverization, 45 per cent. of the ordinary cement stopped by No. 176 sieve.

TABLE XI.—COMPARISON OF ADHESIVE AND COHESIVE STRENGTH.

No.	Description.	Average Strength in lbs. per square inch.	
		Adhesive.	Cohesive.
1	Ordinary cement, age 7 days.	59	532
2	" " "	51	336
3	Fine cement sifted through No. 176 sieve, age 7 days.	94	428
4	" " " "	57	345
5	" " " "	65	500
6	" " " age 28 days.	105	500
7	" " " "	109	387
8	" " " "	84	428
9	" " " "	110	309
10	" " " "	85	320

its rejection, while its cementitious strength was considerably above the average of good cements.

The next point requiring investigation is the effect of quick and slow setting on the adhesive strength, which is capable of being developed within the limits of the time available for ordinary testing purposes. Whether the time occupied in setting affects the ultimate strength of adhesion or otherwise must be reserved for future investigation.

Table XII. contains some of the author's experiments relative to this part of the subject (p. 240).

It will be noticed that, with one or two exceptions, the quick-setting cement manifested a greater development of adhesive strength than the slow, while in the case of cohesive strength quick-setting seems generally to produce an opposite effect. In experiments 1, 3, 5, 7, and 8, slow setting was produced by air-slaking. In Nos. 2, 4, and 6, the cements used were sent by different manufacturers as samples of quick and slow-setting cement, and were not exposed to atmospheric influence.

The time of setting was arrived at by the following means: A vertical steel needle moving freely in guides, and having a flat point $\frac{1}{16}$ inch in diameter, was loaded so as to weigh 1 lb. When the pressure of the point made no visible mark on the surface of the gauged cement it was considered to be set. This is approximately the same pressure as that of the finger-nail, but it has the advantage of being more definite and reliable. Nos. 7 and 8 were samples of the same cement, and tend to show that, although the cementitious strength of the slow-setting samples developed slowly in the shorter interval, it subsequently appeared to gain on that of the quick-setting. In the examples referred to, the quick-setting gained only 3 lbs. in the second three months, while the slow-setting increased 20 lbs. in the same period. Experiment No. 2 was an extreme case, the cement showing incipient signs of hardening before the gauging was completed. The proportion of water used in gauging was limited to that required in each case to bring the cement to the same consistency.

The effect of very rapid setting is apparent in the case of orchard or quick-

TABLE XII.—ADHESIVE STRENGTH OF QUICK AND SLOW-SETTING CEMENT. Test-pieces, sawn limestone.

No.	Degree of pulveriza-tion.	Age.	Time setting in Air.	Average strength in lbs. per square inch.	Remarks.
1	ordinary ..	7 days ..	40 mins.	57	As received from makers.
"	" ..	7 " ..	8 hours.	29	Cooled by air-slacking.
2	fine ..	7 " ..	10 mins.	58	As received from makers.
"	" ..	7 " ..	5 hours.	85	" "
3	" ..	7 " ..	40 mins.	101	" "
"	" ..	7 " ..	3 hours.	41	Cooled by exposure to air.
4	ordinary ..	7 " ..	1½ "	46	As received from makers.
"	" ..	7 " ..	3¼ "	33	" "
5	" ..	28 " ..	40 mins.	71	" "
"	" ..	28 " ..	24 hours.	48	Cooled by exposure to air.
6	fine ..	28 " ..	10 mins.	67	As received from makers.
"	" ..	28 " ..	5 hours.	121	" "
7	" ..	13 weeks ..	30 mins.	110	" "
"	" ..	13 " ..	14 hours.	71	Cooled by exposure to air.
8	" ..	6 months ..	30 mins.	113	As received from makers.
"	" ..	6 " ..	14 hours.	91	Cooled by exposure to air.

Note.—"Fine" cement was that which passed No. 176 sieve.

setting Medina, some samples of this cement showing an average adhesive strength of only 10 lbs. per square inch after seven days' immersion. Similarly, plaster of Paris had a strength of 12 lbs. per square inch after twenty-eight days in air, and Keene's cement 18 lbs. per square inch after seven weeks in air.

As regards a seven or twenty-eight-days' standard test, quick-setting Portland cement can be dealt with either by providing that it shall bear a somewhat higher strain, or by bringing the time in setting within defined limits by air-slaking; the former might possibly lead to complication, the latter is apparently more likely to meet the requirements of the case. The probability is, however, that if a carefully fixed standard of cementitious strength were generally adopted, neither of the alternatives mentioned would have to be resorted to, manufacturers finding it to be their interest to produce cement of sufficient uniformity as regards setting, and fit for immediate use.

In connection with this part of the subject, the proportion of water used in gauging requires consideration. As in the author's system of testing so small a quantity of cement is gauged, he at first used a glass dropping-tube, furnished with a small flexibie tube and pinch-cock, the weight of the drops being previously ascertained; the weight of cement to be gauged was generally 200 grains. However, the proportion of water that would enable the cement to be spread easily and uniformly on the test-pieces varied slightly with almost every sample, and the fine-sifted cement required more water than the same cement unsifted. The weighing of the water was therefore abandoned as introducing complications and refinements unsuitable to the practical requirements of a standard test. In all the tests recorded in this paper (unless otherwise specified) the cements were gauged to approximately the same degree of consistency, an operation which involves but little skill or experience. The test-pieces were also immersed in water previous to making the samples, and the cement was applied while the surfaces were wet.

It was noticeable, in the case of the longer testing-periods, that after fracture the cemented surfaces of the test-pieces, and the thin wafer of cement between them, were quite dry; and in the case of wrought iron the surfaces were clean and bright, exhibiting no signs of oxidation; these and other considerations would lead to the conclusion that after taking the necessary quantity of water into combination, the cement resists all further absorption.

The length of time of setting in air is complicated by the effect produced upon it by heat and humidity; four samples of

the same cement were gauged with the same proportion of water, two of them being placed in rather warm, dry air, the others in cool and somewhat damp air, when the former set in twenty-seven minutes, the latter in four hours, the needle test being used in each case.

Such interferences are avoided in the author's system, by immersing the test-pieces immediately after the samples are made.

There appears to be a considerable difference in the hardening produced in air and water in the same time; for example, one cement of good quality was gauged in the usual manner, part being replaced in air and part in water; the former set in three hours. On examining the samples eighteen hours after gauging, the testing-needle, when loaded with $2\frac{1}{4}$ lbs., produced a visible mark on the sample immersed in water, but required to be loaded with $14\frac{1}{4}$ lbs. to produce a similar impression on the sample which had remained in air. As hydraulicity is the most important feature in Portland cement, if it should become necessary to take the time of setting into account, it would perhaps be more consistent and useful to note that time with respect to water rather than air.

Table XIII. contains some of the author's observations of the relative time occupied by cement in setting in air and water, ascertained by the test-needle before referred to.

Nos. 1 and 7 were sent by manufacturers as samples of their quick-setting cement, Nos. 2 and 8 of their slow-setting; Nos. 3 and 4 were from the same cement, but gauged with a minimum and maximum amount of water respectively.

The only inferences that can be drawn from these experiments appear to be that fine-sifted cement sets faster, both in air and water, than ordinary cement, and that no definite relation exists between the respective times required to set in air and water. To ensure accuracy in the above results involved much time and trouble, and it would be advisable to eliminate such observations from any universally adopted tests.

The influence of water in gauging, on the cementitious strength, is somewhat capricious; an excess of water frequently produces an increase of strength compared with cement gauged very dry. Sometimes, however, the samples possessed the same strength in both cases, the samples being, in every instance, immersed as soon as they were made.

The strength of adhesion of Portland cement to different substances varied considerably; the roughness or smoothness of the cemented surfaces, however, did not affect the strength as much as had been supposed.

Table XIV. contains most of the results obtained by the author relative to this part of the subject.

TABLE XIII.—RELATIVE TIME OCCUPIED BY CEMENT IN SETTING IN AIR AND FRESH WATER.

No.	Temperature. Fahrenheit.	Time required to set in air.	Time required to set in water.	Remarks.
	°	H. M.	H. M.	
1	41	1 35	3 30	Ordinary cement.
2	41	3 15	4 30	" "
3	50	0 35	2 30	" " gauged very dry.
4	50	1 40	2 45	" " " very wet.
5	50	0 23	0 55	Sifted through No. 176 sieve.
6	53	0 30	1 15	" " " " gauged rather dry.
7	54	0 9	0 20	Special quick-setting.
8	53	5 0	22 0	" slow-setting.
9	55	0 20	2 0	Ordinary cement.
10	34	3 0	16 0	" "
Average............		1 38	5 34	

No. 5 similar cement to Nos. 3 and 4, but sifted.
No. 6 similar cement to No. 2, but sifted.

TABLE XIV.—STRENGTH OF ADHESION OF PORTLAND CEMENT TO VARIOUS MATERIAL.

Material.	Average adhesive strength.				Remarks.
	7 days.	28 days.	13 weeks	6 months.	
Bridgewater brick..	19	..	..	..	Ordinary cement.
" " ..	24	66	..	..	Sifted through No. 176 sieve.
Slate (sawn) ..	49	..	..	..	Ordinary cement.
" " ..	53	82	..	62	Sifted through No. 176 sieve.
Portland stone ..	26	50	..	..	Ordinary. Fragments torn out of surface.
" " ..	29	62	..	55	Sifted through No. 176 sieve. Fragments torn out of surface.
Ground plate glass..	..	102	113	..	Ordinary cement.
" " ..	..	..	145	..	Sifted through No. 176 sieve.
Plate iron ..	23	68	..	..	Ordinary.
" ..	44	66	..	..	Sifted through No. 176 sieve.
Sandstone ..	..	49	..	..	Ordinary. Fragments torn out of surface.
Polished marble ..	38	..	..	..	Ordinary cement.
" " ..	52	71	..	75	Sifted through No. 176 sieve.
" plate glass..	47	40	70	..	Ordinary cement.
" " ..	55	49	51	..	Sifted through No. 176 sieve.
Granite (chiselled)..	41	..	..	..	Ordinary.
" " ..	78	97	153	..	Sifted through No. 176 sieve.
Limestone, sawn ..	57	78	98	..	Ordinary cement.
" " ..	78	93	116	..	Sifted through No. 176 sieve.

Total number of tests (omitting those of sawn limestone), one hundred and eighty-two.

The best Portland cement, obtained from five leading manufactnrers, was used in the course of these experiments.

The cement adhered to very hard surfaces, such as granite and ground plate glass, with much greater strength than to softer material, as Bridgewater brick, Portland, and limestone.

The strength of adhesion to polished surfaces was also remarkable; in the case of polished plate glass, the average adhesive strength of fine cement in one set of samples was, after twelve months' immersion, 125 lbs. per square inch; the surfaces of the thin wafer of sifted cement joining the test-pieces, particularly in the longer periods, possessing the same fine polish as the glass, so much so as to suggest the use of polished cement for ornamental purposes. Some experiments made with oak, both in air and water, showed so small an amount of adhesion as to be hardly appreciable.

In comparing the results obtained from ordinary cement and that sifted through No. 176 sieve, the relative quantities of the cementing material, and other considerations referred to, should be borne in mind. Some experiments were made in which the cement was gauged with, and immersed in, sea-water, to compare with others made at the same time, in which fresh water was used. The results were as often favorable to one as the other, the averages being almost identical; the number of tests, however, was but forty, and the time from one week to five weeks. A few tests were also made to try the effect, on ordinary cement, of breaking the bond; the results showed an average strength of 8 lbs. and 32 lbs. per square inch, after seven days and thirteen weeks respectively, the bond being broken (*i.e.* the test-pieces disconnected and then replaced in position) twenty-four hours after the samples were made, but the limited number of experiments considerably diminish the value of these results.

The most suitable shape for the test-pieces had to be determined by experience, involving repeated trials; the first shape suggested was that ultimately adopted, namely, cruciform. It was, however, discarded at first, owing to the want of a suitable testing-machine. In the preliminary experiments a slab of stone was introduced into the neck of the ordinary cohesive sample, an arrangement which, unknown to the author, had been adopted by Dr. Michaëlis about the same time. This was found cumbersome

and tedious; it was also difficult to ensure perfect contact between the stone and cement, and it almost precluded any lengthened series of tests with the fine and coarse particles of cement, owing to the time required to sift so large a quantity as would be necessary. Cubes of stone, with holes drilled in the sides to receive suitable clips were next tried, but they were also cumbersome and expensive, and the results likely to be unsatisfactory. The majority of the experiments referred to in this paper were made with test-pieces, one of which was 2 inches by 2 inches by $\frac{5}{8}$ inch, having a hole rather less than $\frac{5}{8}$ inch in diameter drilled through the center, the other being $1\frac{1}{16}$ inch by $1\frac{1}{16}$ inch by $\frac{5}{8}$ inch. So that, deducting the area of the hole, the cemented surfaces had an area of 1 square inch. The cement was placed on the smaller piece, which was then fixed centrally on the larger, and a slight pressure used to ensure perfect contact; a steel pin, fitting the hole loosely, afforded the means of applying the force necessary to separate the test-pieces. This arrangement works satisfactorily, but involves a little trouble in removing the superfluous cement pressed out from between the test-pieces into the hole in the under one, and which, if not removed before testing, might possibly cause the end of the steel pin to be wedged.

To render such a contingency impossible, the cruciform shape was adopted, and the author believes it will be found to meet all the requirements for accurate testing. The testing-machine already described permits the use of either drilled or cruciform test-pieces. The neatness, accuracy, and facility with which tests can be made by the author's method, he ventures to think, will be appreciated by those who have had experience in testing cement by its cohesive strength, either neat or mixed with sand.

The advantages of a universal test, recognized alike by manufacturers and consumers, can hardly be overrated; at present manufacturers are considerably perplexed and inconvenienced by the varying conditions imposed by engineers, and others, but would be willing to work to a carefully arranged practical standard if such were agreed upon.

The total number of adhesive tests made by the author has exceeded one thousand two hundred, a number which, however, must be considered somewhat limited when the nature, properties, and uses of the material are taken into account.

In adopting the adhesive test, the usual specification of the quality of English Portland cement requires to be modified to the following effect:

"The cement shall be ground so that not more than [45] per cent. shall be stopped by a No. [176] silk sieve, and its average adhesive strength after twenty-eight days' immersion shall be as follows:—

Cement passing No. 176 sieve not less than [95] lbs. per square inch.

Cement as supplied for use not less than [75] lbs. per square inch.

Six tests being employed in each case."

If desired a seven days' test can also be specified. Such a modification will involve but little extra trouble to manufacturers, some of whom now produce cement of considerably finer pulverization than that above indicated.

With reference to a standard test, the author's investigations and remarks may be briefly summarized as follows:

That the cementitious or true value of Portland cement can be best determined by testing its adhesive strength.

That the degree of pulverization is probably the only other condition, the practical importance of which will warrant an introduction into a standard system, which should therefore include a standard sieve. That a sieve having one hundred and seventy six meshes to the lineal inch will be found sufficient for all practical purposes.

That so far as regards the limited time which can be allowed in practice for testing before use, an estimate of the quality of cement can be best obtained by testing the cementitious strength of the fine particles capable of developing most of their strength within the period referred to, but that this should not prevent an adhesive test from being applied to the cement in its ordinary condition.

That any complications arising from differences in the adhesive strength produced by quick and slow-setting should, if possible, be eliminated from a standard system of testing. With respect to the time to be allowed for testing, the author

believes that the recognized periods of seven and twenty-eight days can be made sufficient and should be retained, longer periods being desirable, but precluded by the practical inconvenience involved. If the seven-days' test was satisfactory, and necessity should arise, the cement could be used without further delay; but, as a rule, judgment should be reserved until after the result of the twenty-eight-days' test was known. As regards what has been called the "weight-test," the author believes it might with advantage be omitted, but if desired, the density could be ascertained in the manner described by the author in a former paper.

PHYSICAL SCIENCE IN RELATION TO ARCHITECTURE.

By MR. J. SLATER, B. A.

From "The Builder."

Analogous to the motion of fluids is the motion of the air, and upon a correct knowledge of the laws regulating this motion a great deal depends. Although the fact that air is an elastic fluid, ponderable, and subject to the action of natural forces, has been known for many years, it is only comparatively recently that this knowledge has been turned to account in relation to architecture, and that really scientific schemes of warming and ventilation have been devised. Even within our own recollection it used to be considered quite enough to provide an outlet for foul air, and if it was found practically that the foul air refused to avail itself of the outlet, the fault lay in the "nature of things," and the architect and builder were perfectly free from blame. But as soon as it was clearly understood that, after a maximum has been attained, it is just as impossible for air under ordinary pressure to make its way into a room, as for a vessel of a certain size to contain more than a certain quantity of water; or that no matter in what position an opening from an inner apartment into the external air exists, if the temperature of the apartment be higher than that of the external air, the cold air will have a tendency to rush in, rather than the hot foul air to go out—as soon, I say, as these facts were appreciated, it became easy to lay down simple rules of ventilation, which rules have lately been applied with much success. If mechanical means of drawing away the foul air are provided, the place of the air that is drawn away must be taken by fresh air, and it is only necessary to arrange the inlets carefully, and see that the incoming air is not contaminated, to insure good ventilation. To take another instance, which was forcibly brought home to us all by the terrible Tay Bridge disaster, the pressure of air in motion—that is, of wind—on the roofs of buildings, is a subject well deserving scientific study. Fortunately, in this country, we have not to take such excessive precautions against wind as in other climates; but the inconvenience and danger caused by a high gale in London are very serious, and it is probable that a little extra care would make all roofs much less likely to be stripped by the wind than is now the case. The effect of a gale of wind acting upon a sloping roof is two-fold; it never strips off tiles or slates at once, but any gust of wind of exceptional force is followed by a momentary vacuum, during the existence of which the atmospheric pressure inside the roof is greater than that outside, and this being the case, the tiles or slates, if simply nailed to battens, are lifted at the weakest point, their edges, when a following gust strips them off. If this theory is correct, it follows, as a matter of course, that tiles or slates tightly nailed to close boarding must be much better able to resist the interior force tending to lift them up than if only nailed to battens. This turns out practically to be the case. There is only one other matter in connection with the air that I would mention, and that is, that the state of the air in any building materially affects its acoustic properties. Researches and experiments made by

Professor Tyndall and others, have proved beyond question that the waves of sound are most interfered with when they have to pass through strata of varying densities, and hence there is no doubt that the more effectually we ventilate our public buildings the better will their acoustic properties be. A knowledge of the scientific principles which govern the transmission of sound will enable us to prevent the passage of sound through walls, which is one of the greatest annoyances that dwellers in terraces have to put up with, and if the habit of living in flats should increase, this question will become even more important. I have frequently been struck with the small effect pugging under floors has in preventing sound being heard in the lower rooms, and this is because the pugging is nearly always too tightly packed. Any loose substance, such as sawdust or soft hair felt, in which the vibrations of the air lose themselves, and get broken up, would have far more effect; and it would not be a difficult matter to arrange party-walls so that they should be absolutely impervious to sound. *Optics* is another branch of science that immediately affects us, as the laws of the incidence and reflection of the rays of light must govern the size, and more particularly the exact position, of windows in any building, and in town architecture the value of a building depends very largely on the amount of light obtained for the various rooms.

I must now say a few words on the subject of that combination of the various branches of science to which I have already alluded, known as *Sanitary Science.* This was announced to us a few years ago as a new gospel, but it emphatically is nothing more than a knowledge of Nature's laws. This subject has been treated so exhaustively at the Institute within the last twelve months, that I shall not occupy your time to any extent this evening upon it. Sanitary science is a striking example of the way in which all branches of physical science are allied to and bound up with one another. As was remarked by an eminent scientific man a few years ago—"No one science is so little connected with the rest as not to afford many principles whose use may extend considerably beyond the science to which they primarily belong," and no one can possibly tell how our common every-day household arrangements may be affected by any new discovery in science, remote as it may appear at first. Nothing could have seemed less likely to affect house-planning and the sanitary arrangements of a building, than the researches of an eminent French chemist into certain diseases of animals, and yet it is upon the discovery of M. Pasteur that these diseases were caused by the presence of minute germs in the atmosphere, which upon finding a suitable *nidus* became active death-bearing organisms, that the whole germ-theory of disease rests; and this theory, as soon as it was proved to be scientifically accurate, gave the clue to the cause of that fatal disease typhoid fever—a disease which modern civilization, in the shape of extensive systems of drainage, rendered more fatal than it ever has been before, because it made its approach more insidious. Dirt and filth of all kinds are perceptible at once, and can be easily removed; but sewer gas, which will find its way into a house, however clean it may be kept, unless certain precautions are taken, is a far worse enemy; but this enemy can now be attacked with a certainty of success, the precautions necessary to be observed to prevent its entry into our houses being perfectly well known and easily carried out. Until, however, it was known that that this sewer-gas brought with it the germs which caused typhoid fever, all attempts at improving our system of drainage were abortive, because no one knew what had to be guarded against. The condition in which a number of large houses in the West End are now, or were till very recently, is a striking proof of the evil that may be done through want of accurate scientific knowledge. There can be little doubt that without ventilation of the drains, and the cutting-off of all house-drains from the main sewers, an elaborate and complicated system of drainage, like that of London, is the most noxious and pestiferous lethal engine that could be devised, and that we are now able to escape from it is solely due to the progress of physical science. As I have already referred to that part of sanitary science which relates to ventilation, I need not allude to it further; but I must say a word or two as to

Warming. Now the phenomena of heat are a great deal more complicated than many people suppose, and if I were to attempt to explain the scientific principles involved in the transmission of heat, I should most probably lead you astray in a fog of my own creating; but if any one wants to investigate the subject for himself, he cannot do better than consult that admirable text book on the theory of heat by Clerk-Maxwell. It, is however, only quite recently that these scientific principles have been recognized in the manufacture of stoves; but the Smoke Abatement Exhibition has doubtless done much good in this respect, as it has led people to think about the best mode of combustion, and the best method of radiating heat. Recent investigations have shown that light and heat are one and the same thing, but perceived by us through a different channel, and the "theory of exchanges," as it is called, explains why we use polished silver for a teapot, lampblack for stoves, and why we whitewash the roof of a house to keep it cooler in summer, though it is not generally known that the same application tends to keep the house warmer in winter by diminishing its power of radiation. While on this subject I will briefly refer to one very important fact. If the air through which radiant heat passes be perfectly pure, it is almost completely diathermanous; that is, it does not get heated itself, although it transmits the radiations; but the more impure it becomes, the more it stops the radiations, and takes up heat itself. Hence, the purer we can maintain the air in any place of assembly, and the freer from dust, the less effect will the heat which is being radiated into the room have in raising the temperature of the air itself. This shows the great desirability of filtering by means of horse-hair, wadding, or something of that sort, all the air which passes into a room through the ventilating openings.

I have left to the last that branch of physical science the practical developments of which are of most recent birth, and yet which bids fair to be of the greatest importance to mankind, and some knowledge of which will soon be a necessity for architects: I allude to *Electricity.* Although it is many years since the phenomena of electricity were first observed, and their wonderful character commented on, it is only within the last few years that its properties have been turned to practical account; but lately the progress has been so rapid that there can be little doubt we are on the brink of a more gigantic revolution than the one effected by the invention of the steam-engine, in its practical form rather more than a hundred years ago. The number of points at which our work is brought into contact with the inventions of electricians is increasing daily. A few years ago electric bells were about the only things in connection with houses that called for any knowledge of the subject, and these were generally looked upon as expensive toys; but there are now so many different patents for electric bells, and the simplicity of the system is becoming so widely known and appreciated, that I suppose there are few of us who have not been consulted on the subject by clients, and a slight acquaintance with the principles involved, enabling us to form an opinion as to the probability of a battery lasting, as to the good or bad method of insulating the wires, and various other points, is very desirable. otherwise we are completely in the hands of the men whom we employ, and are unable to exercise the slightest supervision over the work. But the subject that is now engrossing so much attention is the lighting of houses and shops by electricity; and you may be sure that before long the question as to the best method of artificially illuminating the new buildings we design will be one that every architect will be expected to give an opinion upon. Electric light systems are divided into two great branches,—the arc-light system and the incandescent systems,—and the question as to which is the more suitable in any particular case will have to be decided according to the conditions which have to be complied with in that case; hence some knowledge of the advantages and drawbacks of each system is essential. Before leaving the subject of electricity, I may mention the report, recently printed, of the Committee on Lightning Conductors, which is a good example of the benefit arising from a combination of scientific with architectural knowledge. Any one who carefully studies that report will gain a knowledge of the subject that it would

have been absolutely impossible to acquire before its publication.

Let me now mention a matter which appears to me of considerable importance to our profession, but which I can only glance at now,—and that is, the preliminary education of an architect. I have a very strong opinion that the majority of architectural students leave school too early. We call ours a profession, and rightly so, as it is one of the most honorable and arduous of professions. If a boy is destined for the profession of law or medicine, not only is he kept at school till he is eighteen or nineteen years of age, but in nine cases out of ten, he has, in addition, a university course, if not at one of the older universities, at such institutions as University College; but it is no uncommon thing for boys to be articled to an architect at fourteen or fifteen years of age, before the preliminary education can possibly have been completed; and I believe the results of this system are altogether bad. No amount of after-study can compensate for the loss of the two or three years at school or college after the age of sixteen. The compulsory examination recently set on foot by the institute will, I hope, do much to obviate this, and I should be glad to see the scheme of examination framed so as to offer a premium for having passed certain public examinations. For example: that candidates who have passed the senior Oxford or Cambridge non-gremial, or the London matriculation, should be exempted from taking certain simple scientific and literary papers that would otherwise be compulsory, and in this way I believe much good might be done. I have endeavored to show in how many ways a knowledge of physical science may be of advantage to an architect, and I can quite imagine that some such objection as this has occurred to your minds while I have been speaking: "Granting that such knowledge would be beneficial, still the range of subjects is so vast that it would be impossible to gain a thorough acquaintance with them." This I fully admit; but my point is, that a slight acquaintance with these subjects is much better than none at all. As was pointed out by Dr. Siemens some few months ago, a little knowledge is *not* a dangerous thing under certain conditions: these are that the little knowledge be well digested, and that its limits be kept always clearly in view. Although it may seem paradoxical to say so, yet it is a fact that a little knowledge of a subject will often enable a man to see clearly that he knows nothing at all about certain branches of that subject; whereas, if he had no knowledge at all, he might think he knew all about it. There are so many matters closely allied to architecture which are daily becoming more and more important, for which supplementary contracts are obtained, that if we are absolutely ignorant of them, we are wholly at the mercy of the individuals who are employed to do the work, so that I do not think any one should be deterred from gaining a slight acquaintance with scientific subjects because of the impossibility of mastering them. Just as a slight knowledge of a language will not enable us to go at once and read the books written in that language, but will, at any rate, help us to verify a quotation; so a slight knowledge of science will assist us to understand somewhat of its language, and most important of all, will prevent our being the victims of gross deception. I remember hearing a friend of mine, who has had a thorough scientific training, once make a remark which struck me as being a very forcible one. He said,—"There are a great many branches of science that I know nothing about, but I will defy any man to talk nonsense to me on any scientific subject whatever for five minutes without my finding him out." Another objection has probably occurred to you of this kind. What is the use of an architect's taking time to study certain subjects, when he can always call in a specialist who has made an exhaustive study of the same subject? This objection is a very plausible one, and requires to be examined somewhat in detail. In the first place, questions often crop up unexpectedly, which require a decision of some sort to be arrived at at once, and it frequently happens that a specialist is not at hand to advise. In such a case, a slight acquaintance with a subject might enable the architect at any rate to meet the difficulty temporarily, and then, if it should happen that an expert has to be called in, no harm would have been done. Then comes the question of expense,—which is frequently a serious matter to the client,—and here I am touching on a

subject that was referred to a few months ago by Mr. Cole Adams in his interesting paper on Barnacles. Specialists invariably charge heavy fees, and quite rightly. If a man has taken the trouble to master thoroughly any one branch of his profession, and limits his chance of employment to that branch, he ought to charge highly for his knowledge and experience, and I quite admit that in many cases an architect would act wisely to call in a specialist to advise with him, just as a medical man calls in a consulting physician in urgent or difficult cases; but as the family doctor ought to be able to treat all cases not requiring serious and exceptional treatment, a patient would have good cause for complaint against his medical man were he to advise calling in a physician for every little ailment: so it seems to me a client may justly complain if he finds his architect unable to decide what course to adopt in matters of drainage and other scientific subjects which do not present any great difficulty. He imagines that for the fee which he has to pay he is going to obtain skilled advice and assistance throughout the whole of the work which he entrusts to his architect, and will be very likely to grumble at having to pay several extra guineas for an extraneous opinion. There is further one very decided danger in consulting a specialist. You have probably all heard of the physician who discovered a certain disease that was named after him, and after his discovery it is said that no patient ever consulted him, but he found out that the man was suffering from that very same disease; just in the same way a specialist who has made any one subject a special study is almost certain to have strong opinions as to the causes which lead to the existence of a certain state of things, and he will therefore be very likely to find out the existence of these causes, and to advise expensive remedies which *may* not really have been required; whereas an examination by an unbiassed man who has had a scientific training, and is not apt to jump at conclusions hurriedly, may very possibly discover some simple cause for what is wrong that a very slight expense may put right. What is likely to be the effect on the general public,—and you must remember that the general public are made up of individuals, and that the way in which we treat our individual clients is a factor in the estimation in which the profession as a whole is held by the public,—what is likely to be the effect on the public if they find that when the architect is consulted on some faults of drainage, he says, "Oh, Mr. So-and-So is an authority on this point, and you had better call him in," or when he is asked his opinion as to the cause of defective ventilation, he refers to some other eminent authority? Will not the public gradually get to have a low opinion of the profession generally, and begin to question the necessity of employing an architect at all? Are not some of the articles that have recently appeared in some of the papers a proof that this opinion is becoming held more or less? Ought we not, then, to do our utmost to prevent such an opinion gaining ground? We can never tell what matters may be referred to us in the ordinary course of our practice, and we must always remember that the specialist knowledge of yesterday is the common knowledge of to-day.

It may possibly be objected that there are many cases such as I have mentioned which do not come within the legitimate province of an architect, and that if he were to attempt to qualify himself for undertaking such work, he would only be intruding into the domain of the engineer. I hold that any work appertaining to the structural stability of a building, to its sanitary condition as regards drainage, warming, or ventilation, and generally to its suitability for the purposes for which it is intended, is most distinctly within the province of the architect, and if he gives up this to others and endeavors to retain only the artistic part of the work, he will soon find that this is gone too. We do not find that engineers make any scruple about designing the ornamental features of a bridge or any other erection they may have to construct. We find auctioneers and surveyors taking to themselves the title of architect, and designing the houses that are to cover the estates which they have to sell and develop. We are beginning to find plumbers and builders calling themselves sanitary engineers and architects; and how is it that they gain the ear of the public? Simply because the public think that if they go to

a man of this sort they go to a practical man; whereas, if they go to an architect they will find only an artistic *dilettante* who has to go to some one else for advice upon practical points. Depend upon it, if we are to hold our own in the keen competition which now exists, it will not be by taking any so-called high ground as to our being artists and nothing else, but by convincing the public that we are practically acquainted with every branch of our profession, that we can meet the plumber and the surveyor on their own ground, and by showing that, in addition to practical knowledge, we can give that artistic finish which is the result of a cultured education, and which can never be acquired by any one without the patient study to which the early years of the architectural student's life should be devoted. There can, however, be no greater mistake than to think that there is anything derogatory to the true artist in being practical, and in having a knowledge of science. As a proof of this I should only need to point to that admirable address delivered so recently by the leader whose loss we all deplore, to show how Mr. Street, artist as he was in his every fibre, worked strictly on a scientific basis, and his works are a standing proof that the true artist never sacrifices strength to beauty, or lays on ornament for the sake of doing so. But as a matter of fact the greatest artist cannot dispense with scientific knowledge. The sculptor may have the highest manual dexterity, the figure painter the lightest and most delicate touch with the brush; but without a knowledge of the anatomy of the human figure each will be powerless to produce a statue or a painting that shall live as a thing of beauty for ever. So with the landscape painters; the greatest of them are those who live with nature and study her inner workings, and not only her surface aspects; who watch the growth of flower and shrub, and note the effect upon them of storm and sunshine, who ascertain the causes by which the various tints in the atmosphere are brought about,—in a word, who make themselves thorough masters of one or more branches of physical science.

The value of scientific training is by no means limited to its direct results; those that are indirect are of equal importance. A man who has acquired a scientific habit of thought will always exercise a close observation of the facts that come under his notice; he will feel that of the causes which have led to the existence of any state of things that he may be called upon to examine, those that lie on the surface, and are most obvious, may very probably not be the real ones; he will not allow himself to be led away by superficial similarities; and, above all things, he will not be hasty in jumping at conclusions. Useful as these qualities are to every one, they are of especial value to an architect in what I may call the collaterals of his profession. When once a man gets into regular practice, he will find that designing buildings forms but a part—sometimes a small part—of his work, which frequently consists of reporting upon the condition of buildings, settling disputes, giving evidence in courts of law, and such matters as these; and herein the indirect qualifications, which are the results of scientific study, will be of the utmost service.

I have thus endeavored to show you, very imperfectly, how intimately we architects are interested in the advance of physical science. As I said at the commencement, so I would repeat now, that I hope I shall not be considered as in the least opposed to our being artists: it has been my endeavor to show that art and science are not antagonistic, but rather twin sisters who ought never to be separated, and it is to any tendency to separate the two that I am opposed. Captivating as art is to her votaries, science is equally so to hers; and, in conclusion, I think I may safely promise this, that he who takes up the study of any branch of science connected with architecture will never regret having done so, but will find in the pursuit of it growing delight and an ever increasing fascination.

ENGLISH milling engineers are introducing square rope belts, which are said to be very suitable for transferring power. They are made in strips with "step" joints, screwed together; the sides of the rope leaving the pulley groove without loss of power. It is stated that a one and a-half inch rope, at 4000 feet a minute, has driven over 100-horse power.

A SECOND SUEZ CANAL.

From the "Nautical Magazine."

The question of providing additional means of transit through the Isthmus of Suez bids fair to excite almost more interest, and to arouse even more national feeling than were produced by the original scheme for the construction of the present canal. For some time past it has been evident that increased canal accommodation must be provided. At the recent meeting of the Association of Steamship Owners engaged in the Eastern trade it was stated that the traffic had increased from 4,500,000 tons in 1880 to 7,000,000 tons in 1882. And at the last half-yearly meeting of the Peninsular and Oriental Steam Navigation Company, the chairman, Mr. T. Sutherland, made serious complaints with regard to the inadequacy of the canal to meet the growing requirements of the Eastern trade. It was stated that the company are now compelled to start their vessels from London twenty-four hours earlier than formerly, in order to insure their reaching Suez in time for the arrival of the mail from Brindisi. This is entirely owing to the increasing delays in the canal, something like three days being now occupied in traversing a distance of ninety miles.

In short, the necessity for an extension of the present accommodation is every day becoming more pressing. There is every reason to believe that our trade with the East and Australia is still far below the highest point it is destined to reach. With a fuller development of the means of internal communication in India and China, there would seem to be no definite limit to its further growth. For example, it is estimated by competent authorities that India will be able to double her present export of wheat within the next few years. Great reductions are being made in the rates charged on the different railways, and it seems likely that before long Indian wheat growers will be able to enter into a serious competition with growers in America. Already India sends us 20,000,000 cwt. of wheat, and if this amount were doubled her export would be equal to two-thirds of the total wheat imports of the United Kingdom. The great increase which is taking place in the amount of shipping engaged in the East shows plainly that trade generally in that part of the world is in a flourishing state, and fully justifies the assumption that no long period will elapse before 10,000,000 tons of shipping per annum will have to be passed through the canal.

The question now calling for decision is not whether additional accommodation is required, but who is to have the privilege of providing it. That M. de Lesseps should assert an exclusive right on the part of the present company to carry out any further works which may be necessary is not surprising. The position of the company, as defined by the concession granted by Said Pacha in 1854, is certainly a strong one; but, in addition to this, the undertaking has proved successful, even beyond the most sanguine expectations of its talented projector, and it is not to be supposed that any scheme for constructing a rival canal will be regarded with indifference by those who are receiving handsome dividends from the present monopoly. Sixteen per cent. was the rate of profit for 1882, and M. de Lesseps, we believe, not long since declared his intention of "making the fortunes" of the present shareholders. It is evident, however, that the claims made by M. de Lesseps on behalf of the existing company will not be allowed to pass unchallenged. The British shipping trade is beginning to feel, not only the inconvenience arising from the inadequacy of the canal to the requirements of the traffic, but also the weight of the dues from which the "fortunes" of the shareholders are being derived. Moreover, the payment of interest at the rate of 16 per ct. upon an investment that may be regarded as perfectly secure is not without an influence on capital; and, as there are no practical difficulties which would impede the construction of further works, it is natural that capitalists should show a desire to take advantage of the opportunity now presented

for obtaining a share in the general undertaking.

The problem to be solved by the various authorities concerned is doubtless somewhat intricate, involving as it does, questions of private right, of national feeling and of grave international importance. The position of the present company is tolerably clear. M. de Lesseps relies entirely on the concession made by Said Pacha in 1854 which conferred on the company the exclusive right of making and maintaining a canal through the Isthmus, and extended this privilege for a term of 99 years from that date. The Egyptian Juridical Committee who have recently had the matter under consideration have expressed the opinion that this concession places the company in an unassailable position; but it seems to us that, in one sense, their position is even too good. If Said Pacha had granted a monopoly for a moderate term, the grounds for raising awkward questions with regard to their precise rights would have been somewhat less favorable than they are under the present conditions. The aasumption that Said Pacha, who happened to be the deputed ruler of Egypt in 1854, had the power to settle for a period of 99 years a question of the gravest importance to the whole world, is one by no means easy to uphold by force of argument. If he had merely authorized the construction of a new railway across the Isthmus, and had given a guarantee to the proprietors that for a term of 99 years no canal should be made which would compete with their undertaking, even M. de Lesseps would doubtless have been able to find good reasons for disputing the soundness of the arrangement Yet such a case would have been precisely similar to the present.

As regards the construction of any further works, the position of the canal company is not, however, so strong as would at first appear. In the first place they can do nothing towards enlarging the channel without the consent of the Egyptain Government. The canal could not be widened unless a grant of additional land were made, and whether this would be allowed or not would be for the Egyptain authorities to decide. M. de Lesseps is well aware of this, and he also knows that, as the British Government are at present complete masters of the situation in Egypt, it would be useless for him to seek any further concession without their approval Moreover, by Article 2 of the concession of 1854, the Egyptian Government reserved the right to appoint the Director of the company, so that practically it has the power to exert a very material influence on the policy to be adopted with regard to the future management of the undertaking.

But, as a matter of fact, this question is not to be decided by any verbal, or semi-legal, quibbles with respect to the exact import of any concession that may have been granted by Said Pacha. The Suez Canal has now become a great international highway, and the point at issue is, whether a particular monopoly is to be allowed to exist to the advantage of the whole civilized world. The pretension of M. de Lesseps that the Canal Company should have the power to dictate terms with regard to the right of way through the Isthmus for a period of 99 years, is too extravagant to be maintained. Even during the recent Egyptian war it was found necessary to interfere with the independence of the company, and what was done then would certainly be done again whenever the necessity of the occasion arose. The so called private rights, whether of individuals or of companies, cease to exist as soon as they clash with national interests, and from the working of this rule the Suez Canal Company will unquestionably not be exempt.

As regards the passage through the Isthmus of Suez there are two distinct questions involved. In the first place the requirements of the shipping trade call imperatively for a large extension of the present accommodation, as well as for a reduction in the present heavy dues; and, in the second, in the interests of this country it is essential, as stated in Article 1 of the Draft International Agreement issued by the Foreign Office in January last, that the canal shall "be free for the passage of all ships in any circumstances." With respect to the first consideration, the construction of a second canal seems to be the only course for providing a satisfactory solution of the present difficulty. M. de Lesseps calculates that by means of certain alterations in the existing canal, accommoda-

tion could be secured for a traffic of 10,000,000 tons per annum; but, even if this could be done, the provision would be only temporary. It is tolerably certain that, within the next few years, the Eastern shipping trade will surpass this amount if due facilities for its development are provided; consequently it is useless to look to any enlargement of the present canal for a permanent settlement of the question. It appears to us that nothing short of an arrangement which would enable the traffic to be maintained continuously in both directions can be deemed satisfactory; and it is evident that only by means of a second canal would this be possible. As long as only a single channel exists there will always be the risk of delays and temporary blocks in the traffic from a variety of causes, and for this reason, if for no other, the construction of a second canal may be regarded as highly necessary.

But, in addition to the question of convenience, there is also the subject of the canal dues to be considered. And here it must be borne in mind that, if any appreciable reduction is to be made in the charges which now weigh so heavily on the Eastern shipping trade, it is essential that any additional means of communication which may be provided should be under the control of an independent company. As long as a monopoly exists the Isthmus of Suez will continue to be the mine of wealth it is at the present time, and the shareholders who control the right of way will certainly not cease to endeavor to "make their fortunes" at the expense of the marine carying trade. If the United States, by any ill-judged concession, had granted to the first Atlantic Telegraph Company the sole right of laying submarine cables between their ports and Europe, for example, it is easy to imagine the nature of the inconvenience that would have resulted. And the Suez Canal is an exact parallel. A monopoly in the one case would have long since proved intolerable; and in the other there is nothing which can possibly render it less burdensome.

It is evident that shipowners in this country are now fully alive to the true position of affairs with respect to the Suez Canal. At the recent meeting of steamship owners interested in the Eastern trade, the representatives of 3,000,000 tons of shipping passing through the canal per annum were present, and a sum of £20,000 was at once subscribed towards the expenses of preliminary inquiries, with a view to the commencement of a new undertaking. And we believe there is at present a vast amount of capital ready for investment as soon as the construction of another canal has been decided upon. That this should be the case is natural, for the rate of profit is certain to be good, while the risk is practically *nil.*

As regards the national interests involved in this particular question, it is clear that the English Government are now masters of the position, and that they will be almost compelled by the force of circumstances to turn their advantage to account. Full credit must be given for the honesty of their declaration that Egypt is to be occupied only temporarily; but the present aspect of affairs seems fully to warrant the assumption that, unless some unforeseen contingency should intervene, our occupation will be prolonged indefinitely. At all events an abandonment of the country is at present quite out of the question, and it would be deplorable if England were not to utilize the opportunity now offered for removing what may at any time become a very serious obstacle to the free use of her great Indian route. We are well aware of the importance of a friendly understanding between this country and France; but, in the endeavor to preserve this intact, it is highly necessary that certain plain facts should not be lost to sight. M. de Lesseps is justly entitled to the world's esteem for the courage and genius he displayed in bringing his work to a successful issue, in spite of the opposition he had to encounter from this side of the English Channel, and his French supporters may claim full credit for their sagacity in assisting to carry his scheme into effect. We have to admit that England's former policy in this matter was erroneous; but our gratitude to the founders of the canal must not be carried to excess. We must now deal with the facts as they stand. And these facts are simple. The Suez Canal has become a maritime highway as important as the Straits of Gibraltar. In consequence of

the growth of British commerce, which supplies four fifths of the total traffic, it has become inadequate to the demands for accommodation. It is the key to England's most important possession; but it is in the hands of an independent company. It is now admitted on all hands that new works must be undertaken, and the question is whether England is to allow this company to hold the right of way through the Isthmus as its own private monopoly, or whether she will insist that when the new channel is cut it shall be placed either under her own direction, or be made subject to an international arrangement which would insure just and equitable treatment for all.

With all due respect to M. de Lesseps, it seems to us that his claim to tax the Eastern carrying trade for the purpose of "making the fortunes" of certain shareholders, is not more defensible than was the practice of the petty German chiefs who, in the middle ages, levied black mail on the vessels passing down the Rhine in front of their castles. There is in this country no desire to deprive the company of its just rights. It is entitled to generous treatment in return for the great services it has rendered to the world, and this we have no doubt it will receive. But that it shall be allowed to have entire control over the right of way through the Isthmus until the year 1953, simply for the purpose of enriching two or three generations of private shareholders, is a pretension altogether too extravagant to be maintained, and one, moreover, which England could not allow without ignoring her most vital interests in the East, nor without completely reversing the policy she has already spent several millions sterling in maintaining.

DEVIATIONS IN COMPASSES OF IRON SHIPS.

By STAFF-COMMANDER E. W. CREAK, R. N.

Abstract of a Paper recently read before the Royal Society.

The period comprised between the years 1855-68 was one of active research into the magnetic character of the armor-plated and other ships of the Royal Navy, and iron ships of the Mercantile Navy.

Among other contributions to this subject, a paper by F. J. O. Evans, Staff-Commander, R.N., F.R.S., and Archibald Smith, F.R.S., was read before the Royal Society in March, 1865, relating to the armor-plated ships of the Royal Navy, and containing the first published results of the system of observation and analysis of the deviations of the compass established four years previously.

From lack of observations in widely different magnetic latitudes the authors of that paper were unable to define the proportions of the semicircular deviation arising from vertical induction in soft iron, and that arising from permanent or sub-permanent magnetism in hard iron.

During the last fifteen years vessels of all classes—except turret ships—have visited places of high southern magnetic inclination or dip, and the analysis of the deviation of their standard compass has been made, showing the constants of hard and soft iron producing semicircular deviation.

The constants of soft iron provide a means of *predicting* probable changes of deviation on change of magnetic latitude for certain vessels of the following classes, and others of similar construction.

1. Iron armor-plated ships.
2. Iron cased with wood.
3. Iron troopships.
4. Iron and steel ships cased with wood.
5. Composite-built vessels.
6. Wooden ships with iron beams and vertical bulkheads.

These vessels were all in a state of magnetic stability previous to the observations which have been discussed, and their compasses have had the semicircular deviation reduced to small values, or corrected in England by permanent bar magnets.

This correction may be considered as the introduction of a permanent mag-

netic force acting independently, and in opposition to the magnetic forces of the ship proceeding from hard iron.

It is now proposed to consider the effects of a change of magnetic latitude on the component parts of the deviation.

Semicircular Deviation.

On semicircular deviation from fore-and-aft forces, time has but little effect, and the greater part of it is due to permanent magnetism in hard iron which may be reduced to zero for all latitudes by a permanent magnet.

A second but small part of this semicircular deviation proceeds from sub-permanent magnetism in hard iron. It is subject to alterations slowly by time, from concussion, and from the ship remaining in a constant position with respect to the magnetic meridian for several days, and is more intensely affected by a combination of the two latter causes.

Deviations from sub-permanent magnetism which have temporarily altered in value as described, return slowly to their original value on removal of the inducing cause.

The principal cause of change in the semicircular deviation on change of magnetic latitude, in corrected compasses, arises from vertical induction in sot iron, which changes directly as the tangent of the dip.

In standard compasses judiciously placed with regard to surrounding iron this element of change is small, and similar in value for similar classes of ships.

With very few exceptions, nearly the whole of the semicircular deviation from transverse forces is due to permanent magnetism in hard iron subject to the same laws as that proceeding from fore-and-aft forces.

In the exceptional cases alluded to, there is a small part due to vertical induction in soft iron, changing directly as the tangent of the dip.

Quadrantal Deviation.

This deviation, caused by induction in horizontal soft iron symmetrically placed, does not change with a change of magnetic latitude. Time alone appears to produce a gradual change in its value during the first two or three years after the ship is launched, when it becomes nearly permanent.

The diminution of the mean directive force of the needle, which is common to all vessels of war, improves slowly at first by lapse of time, and finally assumes a permanent value.

Relative Proportions of Hard and Soft Iron.

It has been found that the relative proportions of the hard and soft iron affecting the standard compasses of twenty-five vessels examined differ considerably, even in ships of similar construction.

This difference may be accounted for by the compasses not being placed in the same relative position in the ships considered as magnets of various forms, and containing numerous iron bodies introduced during equipment.

General Conclusions.

The following general conclusions have special reference to the standard compass positions of the six classes of vessels previously mentioned:

1. A large proportion of the semicircular deviation is due to permanent magnetism in hard iron.

2. A large proportion of the semicircular deviation may be reduced to zero, or corrected for all magnetic latitudes, by fixing a hard steel bar magnet or magnets in the compass pillar, in opposition to, and of equal force to, the forces producing that deviation.

3. A very small proportion of the semicircular deviation is due to sub-permanent magnetism which diminishes slowly by lapse of time.

4. The sub-permanent magnetism produces deviation in the same direction as the permanent magnetism in hard iron, except when temporarily disturbed (1) by the ship's remaining in a constant position with respect to the magnetic meridian for several days, (2) by concussion, or (3) by both combined, when the disturbance is intensified.

5. To ascertain the full value of changes in the sub-permanent magnetism, observations should be taken immediately on the removal of the inducing cause.

6. In the usual place of the standard compass the deviation caused by transient vertical induction in soft iron is

small, and of the same value (nearly) for ships of similar construction.

7. The preceding conclusions point to the conditions which should govern the selection of a suitable position for the standard compass with regard to surrounding iron in the ship.

The following note is in answer to a correspondent's query recently addressed to us; the subject, as he says, has not been mentioned in any of the works treating on the Deviation of the Compass:

Practical Rules for the correction of the semicircular deviation at sea when the compass has been previously corrected by magnets. If, in the lapse of time, or through any considerable change of magnetic latitude, it is found that the deviation card no longer correctly indicates the deviation, you can correct the semicircular deviation by attending to the following rules. It is of course taken for granted that you know the use of *Time Azimuths, or of distant terrestrial objects*, in bringing your vessel's head to any of the cardinal points, *correct magnetic;* then you will move the magnets, as required, for two *adjacent* cardinal points, and subsequently test the correction for the remaining two cardinal points.

I. For ship's head brought *correct magnetic* North or South, the correcting magnet lies athwartship, with its N. marked (*red*) end to starboard or port, according to the original deviation. Then proceed to move the correcting magnet as follows:

(*a*) *Thwartship magnet with N. marked end to starboard—*

Ship's head N., with E. dev. (*i.e.* compass N. to starboard) magnet nearer.
Ship's head N., with W. dev. (*i.e.* compass N. to port) magnet further off.
Ship's head S., with E. dev. (*i.e.* compass S. to starboard) magnet further off.
Ship's head S., with W. dev. (*i.e.* compass S. to port) magnet nearer.

(*b*) *Thwartship magnet with N. marked end to port—*

Ship's head N., with E. dev. (*i.e.* compass N. to starboard) magnet further off.
Ship's head N., with W. dev. (*i.e.* compass N. to port) magnet nearer.
Ship's head S., with E. dev. (*i.e.* compass S. to starboard) magnet nearer.
Ship's head S., with W. dev. (*i.e.* compass S. to port) magnet further off.

II. For ship's head brought *correct magnetic* East or West, the correcting magnet lies fore-and-aft, with its N. marked (*red*) end aft or forward, according to the original deviation. Then proceed as follows:

(*c*) *Fore-and-aft magnet with N. marked end aft—*

Ship's head E., with E. dev. (*i.e.* compass E. to starboard) magnet further off.
Ship's head E., with W. dev. (*i.e.* compass E. to port) magnet nearer.
Ship's head W., with E. dev. (*i.e.* compass W. to starboard) magnet nearer.
Ship's head W., with W. dev. (*i.e.* compass W. to port) magnet further off.

(*d*) *Fore-and-aft magnet with N. marked end forward—*

Ship's head E., with E. dev. (*i.e.* compass E. to starboard) magnet nearer.
Ship's head E., with W. dev. (*i.e.* compass E. to port) magnet further off.
Ship's head W., with E. dev. (*i.e.* compass W. to starboard) magnet further off.
Ship's head W., with W. dev. (*i.e.* compass W. to port) magnet nearer.

The character of the heeling error may be well expressed in a *tabular form*, according to whether the N. point of the compass needle is drawn to windward, or to leeward, as follows:

For Heeling Error, on Courses,	When N. end of compass needle is drawn to windward; heel to port gives	heel to starboard gives	is drawn to leeward; heel to port gives	heel to starboard gives
Bet. E. & W., through N. .	E. or +	W. or −	W. or −	E. or +
Bet. E. & W., through S. .	W. or −	E. or +	E. or +	W. or −

And where the ship or steamer may

have taken a *strong* list, owing to a shift of the cargo, substitute "high" side for windward, and "low" side for leeward.

REPORTS OF ENGINEERING SOCIETIES.

American Institute of Mining Engineers. The Autumn meeting of the Institute will be held in Troy, N. Y., during the second week in October. This meeting will be mainly devoted to the reading and discussion of papers.

Members who wish their papers fully discussed at the meeting can have them printed and circulated in advance, if they are sent to the Secretary early in September.

ENGINEERING NOTES.

Some of the Russian soldiers have been busy lately. The Jabinsk-Pinsk Railway, 120 miles in length, has been constructed by one corps, and although it was necessary to erect no less than sixty-nine bridges, two of which were of considerable size, the whole line was finished in five months, and cost the Government only £3,400 per mile. The Trans Caspian Krasnovodsk Railway is also making rapid progress, and will be completed as far as Kizil Arvat by the middle of June. In spite of the scarcity of materials, and the obstacles presented by the Trans Caspian steppe, the line will only cost about £4,200 per mile. A *Times* correspondent says it has been found by experience that the storms and sand drifts in the steppe cause very little damage to the railway, only sometimes delaying a train for a few hours.

At the meeting of the American Society of Civil Engineers in New York, May 16th, Mr. F. J. Cisneros, who recently visited the Isthmus of Panama, presented an informal statement of the progress of the work on the Panama Ship Canal. He stated that the purchase of the Panama Railroad by the Canal Company seemed to promise most excellent results. In reference to the canal, he said that the line had been completely staked, cross sections taken, and the location made and stakes set for definite work for a large portion of the line. The line is entirely cleared and grubbed from Kilometer 40 to the mouth of the Rio Grande, and is rapidly advancing to other points. The valley of the Chagres has been surveyed, and it has been found that the high water lines above the high dam will cover an area of about 6,750 acres, and that the volume of water stored will be about 1,000,000,000 cubic meters. Actual work upon the canal has been commenced at six points. The contractors, Messrs. Slaven & Co., for dredging the canal from Colon, have their first "Hercules" dredge in place, and will commence work directly. The Canal Company has been working with two French machines, at the rate of 1,000 cubic meters per day for each machine. The Franco-American Trading Company has contracted for the excavation of about 10 kilometers of the canal beyond the Bay of Panama. Its machines are being built at Lockport, N. Y. There are now about 6,500 men on the work, chiefly Jamaicans, Carthagenians, and a few Martiniqueans.

Separate System of Sewerage.—Mr. Albert W. Parry, M. Inst. C. E., borough surveyor, read a paper on the separate system of sewerage as carried out at Reading. The system could not be regarded as complete, so far as surface water is concerned, as all the sewers are not new. A system of sewers for the disposal and utilization of sewage was completed about seven years ago, and the old sewers, which formerly conveyed both sewage and surface water, are now used only for surface water, and in streets where there were no new sewers new ones are being laid. The urgent need of keeping rain water out of the main sewers had been shown by the sewage being diluted and its volume increased, causing augmented cost and difficulty of dealing with it on the sewage farm. After describing the system of sewerage, he mentioned that the hard rule of excluding all surface water from the sewage sewers was relaxed in cases where there are small enclosed areas in the rear of houses, where a second grate or trap, if fixed for receiving rain water, would offer equal facilities for the emptying of slops. The number of houses was 8,700, the average quantity of sewage pumped was 998,277 gallons per day, and the water supplied to the town was about 1,813,000 gallons daily.

A discussion ensued, which lasted nearly three hours. Mr. Gordon (Leicester) stated he had partially adopted the separate system, and was in favor of getting storm water to the natural outfall for the water of the district. Col. Jones (Wrexham) advocated the separate system, and said that in that way alone could the difficulty of disposing of sewage by irrigation be successfully accomplished. Surface water should go into the drains, and water supplied for domestic purposes only into the sewers. Mr. Jerram (Walthamstow) stated that for two or three years the system had been tried there, and had proved a failure. Sewage and dirty slops were being constantly connected with the surface drains, when repairs were needed, so that it was impossible to work the system satisfactorily. Mr. Lemon (West Ham) declared there was no perfectly separate system carried out in England, and if it were, it would not last a month. It was only applicable to some towns, and only where sewage was applied to the land, or had to be pumped, and then purified by precipitation and some chemical process. It was an extraordinary stretch of authority to compel houses to have drains for sewerage and for surface water. The only extent to which the system could safely and wisely be carried was, in suitable situations, to carry off into the natural outfall the water falling in open streets, that side of the roofs of houses and places under the sole control of the local authority. Mr. Pritchard (Birmingham and London) expressed similar views and Mr. Angell (West Ham) agreed that the separate sys-

tem should be limited to streets, open spaces, and the street sides of houses. It would be folly to attempt to carry it further. Mr. Parry replied at some length, but the general feeling was evidently strongly adverse to anything like an attempt to compel all houses to have two sets of drains, one connected with the sewers for sewage and house slops, and the other for surface drainage and inoffensive water.

The South Pass Improvements.—Captain W. H. Hener, Corps of Engineers, has submitted to General Wright his annual report of the progress of the work on the improvement of the South Pass of the Mississippi River, from which the following extracts have been taken:

"Except for five days in July, 1882, there has been a channel between the jetties having at least a depth of thirty feet of water in it, and the twenty-six feet deep channel in the jetties had during the year, except for nine days in July, 1882, a least width of 200 feet. At present there is a depth of thirty-one feet in the jetties, and the least width of the thirty-feet channel is ninety feet. The least width of the twenty-six-feet channel is 240 feet. In the pass itself there is a channel twenty-seven feet deep, and the twenty-six-feet channel in the pass has a least width of 160 feet. In other words there is now a channel at least 160 feet wide, and having a least depth of twenty-six feet of water in it, from the Gulf into the main river. This is the best channel that has ever been found since the jetties were constructed. But eighteen days' dredging has been done on the work during the year, of which five days was in the pass, nine days in the jetties, and four days on mud lump outside of the jetties. The thirty-feet channel within the jetties has much improved during the year. For a small portion of the year the narrowest part of this channel was only fifteen feet in width. This has increased until now its least width anywhere is ninety feet. The improvement is attributed to the construction of an inner jetty, built parallel to and about 200 feet inside of the east jetty. The length of this inner jetty is 6,810 feet. While the river jetty has improved the channel in the jetties, it has reduced the width of the waterway between the jetties to 630 feet. Before the wing dams, cribs and inner jetty were built the waterway was about 1,000 feet in width. In September last a cyclone passed over the jetties and worked much damage to the east jetty, about one-half mile in length of the concrete wall on this jetty being badly broken, and solid blocks of concrete weighing twenty-eight tons being displaced. The channel within the jetties, however, remained uninjured.

"Surveys made during the year beyond the ends of the jetties extending out to 100 feet depth of water show a very little change to have occurred on what is sometimes called the bar. On the jetties proper no work has been done during the year. Within them work has been confined to building the inner jetty and five wing dams projecting from the east jetty. In the pass proper eleven new wing dams have been built, one at Crane Island, three near Goat Island, and seven near Bayou Grande, varying from 20 to 250 feet in length. At the places the pass was wide and shoaler than in the narrower parts of the pass. In fact, the depth of water in the channel was hardly an inch more than the twenty-six feet of depth required. After the dams were built the current rapidly scoured out the crests of these shoals until a depth of thirty-two feet of water was obtained.

"At the head of the South Pass there is now a fine channel four hundred feet wide and having a least depth of thirty feet. The channels at the heads of Southwest Pass and Pass a l'Outre are also increasing in depth, but the bars at the mouths of these passes are reported as being very shoal. That at Southwest Pass is reported as having only a twelve-feet channel through it, while Pass a l'Outre bar is said to have but eight feet. Both of these passes are now so little used that but little is definitely known about them, except where our surveys cut into them near their heads. During the year ten vessels grounded in the pass, jetties, or near the jetties, but in every instance they were out of the channel, which was wide, deep and practicable.

RAILWAY NOTES.

Railway Improvements.—Mr. Westmacott, in his address to the Institution of Mechanical Engineers, said, in regard to our present railway systems:

"Touching upon speeds, the mind naturally reverts to railway traveling. Here, however, it would seem as if for the present we had reached a maximum. It is surprising how soon the speed of the locomotive was brought up to something approaching its present limit. Geo. Stephenson was laughed at in 1825 for maintaining that trains would be drawn by a locomotive at twelve miles an hour, but the Rocket herself attained a speed of twenty-nine miles an hour at the Rainhill competition in 1829, and long afterwards ran four miles in 4½ minutes. In 1834 the average speed of trains on the Liverpool and Manchester Railway was twenty miles an hour; in 1838 it was twenty-five miles an hour. But by 1840 there were engines on the Great Western Railway capable of running fifty miles an hour with a train and eighty miles an hour without. In 1841 we find Stephenson himself ranged on the side of caution, and suggesting that forty miles an hour should be the highest regular speed for trains. Now, it is a remarkable fact that the highest speed at which locomotives run in ordinary practice scarcely seems to have been raised during the last twenty-five years; on the other hand, the weight of the trains has been perhaps doubled. Although the average running time of express trains has in many cases been improved, this has been almost entirely due to their making fewer stoppages. At the same time the speed occasionally attained is very great. Engines on some of our principal lines have repeatedly run fifteen miles in twelve minutes, or at a speed of seventy-five miles an hour, and express trains run regularly at fifty-three miles an hour. It does not follow, however, that there is never

to be any increase in the speed of trains, and it seems a point well worth consideration in what way the time of transit between important centers of trade can be shortened.

What are the causes which have tended to prevent any improvement in this particular? In the first place it may be said that the permanent way would suffer seriously by further increase in speed; but this could surely be overcome in time by improving the permanent way itself, which also remains very much in the same condition and of the same construction as it was twenty-five years ago. Again, it may be said that the running at a higher speed would require more powerful engines, and hence that trains now worked by a single engine would require two, or would have to be spilt up into two trains at a great increase in running expenses. This however assumes that it is not possible so to improve the engine that it shall be able to exert a considerably higher power withont an inadmissible increase in weight. By utilizing a larger part of the total weight of the engine as adhesion weight, it would be easy to obtain the amount of adhesion required for the increased tractive force; and for this purpose Mr. Webb's compound locomotive (to be described by the author in a paper he has prepared for this meeting) which enables the number of driving wheels to be increased without the use of coupling rods, appears to merit particular attention.

Another point in which improvement may possibly arise in the future should be noticed. On the Russian railways, where both coal and wood are dear, the burning of petroleum has now taken a practical form. Our member, Mr. Thomas Urquhart, has been very successful in this direction, and is now running locomotives regularly which use only petroleum refuse, and which show a marked economy over coal or wood. To test the point, he prepared three locomotives of exactly the same type, and started them on successive days under exactly similar conditions of weather, train, and section of road. The trips were made both ways, and the results per verst, including fuel required in lighting up, were as follows:

	copecks.
Anthracite, 52.9 Russian pounds... cost	26.35
Wood, 0.0107 cubic sashin, cost....	23.54
Petroleum-refuse, 27.36 Russian pounds, cost	11.64

There is thus in this instance an economy of at least 50 per cent. on the side of petroleum, the boiler pressure being from 120 lb. to 130 lb., and the gross load over 400 tons. At the same time the weight of fuel used, as against coal, is diminished by about 50 per cent., which is a most important item.

Although petroleum is scarcely a product of Western Europe, we have to notice on the other hand the progress which has lately been made in the extraction of oil as a waste product from coal, &c. Mr. Jameson has extracted as much as nine gallons per ton from mere shale. It is suggested that markets for such oil will be difficult to find; but it seems allowable to hazard the idea that we may hereafter see our locomotives even in England, running with oil fuel, which would be at once much lighter and much more easily renewed than the coal which is used at present, and get rid of the intolerable nuisance of smoke and dirt. There might in fact be an oil tank and a water tank side by side at every stopping station, and the engine would replenish her store of fuel at the same time as her store of water.

NEW LOCOMOTIVE SIGNALLING APPARATUS.—On June 30, a series of trials was made, upon the branch line of railway from Messrs. Evans & Co.'s Haydock Collieries to Earlstown Junction, of a new automatic locomotive signalling apparatus patented by Messrs. Croft & Lomax. The apparatus consists of a tappet fixed to a sliding bar, which communicates by means of a bell crank with a disc signal upon the engine, in front of the engine driver. In the 4-foot way a metal box is sunk in which an inclined plane is raised or lowered from the signal cabin, as the signals are at "danger" or the reverse. If at "danger" the tappet alluded to in the apparatus strikes upon the inclined plane, and releases a weight communicating with bell crank, which moves a red light, and also an arm danger signal on the engine, and at the same time blows a whistle. Neither of these can be altered until they are attended to by the engine man by means of a lever, so that it would be impossible for him to overlook or neglect the signal, as his attention would of necessity be drawn to it by the continued whistling, which wonld only cease when put out of action by the driver. The "distant" and "home" signals are exactly repeated on the disc carried by the engine, so that in foggy weather the driver is able to distinguish which signal is indicated by simply looking at the dial on his engine. The tests were made at varying speeds, and in every instance the signal was correctly given. The trials were witnessed by a number of gentlemen interested in the matter, including Mr. John Higson, mining engineer, of Manchester, and the general opinion expressed was one of satisfaction with the results. The apparatus is one that can be readily applied at a small cost, and, judging from the tests made on Saturday, there is little doubt it would contribute towards the safe working of railway traffic, especially in foggy weather, and in working the colliery sidings which crowd upon the main lines in some of the mining and mineral districts.

ACCORDING to experiments made upon the Hanover, Cologne, and Minden Railway, fir sleepers injected with chloride of zinc required a renewal of 21 per cent. in eleven years; birch sleepers injected with creosote required a renewal of 46 per cent. at the end of twenty-two years; oak sleepers injected with chloride of zinc required a renewal of about 21 per cent. at the end of seventeen years: while the same kind of sleepers in their natural state required a renewal of at least 49 per cent. at the end of a like period. The conditions in each of these cases were very favorable for obtaining trustworthy proofs. The subsoil of the line was good; the non-renewed sleepers showed, when cut, that they were. in a sufficiently good state

of preservation. Upon another line where the oak sleepers were not injected, it was necessary to renew them in the proportion of 74 per cent. at the end of twelve years; these same sleepers injected with chloride of zinc required a renewal of only 3.29 per cent. at the end of seven years; whilst those injected with creosote rerequired a renewal of only 0.09 per cent. at the end of six years.

On the 1st of June, at 7.30 p.m., the new quick railway service between Paris and Constantinople *via* Vienna and Giurgevo, came into operation. For the present it will be a biweekly service both ways, leaving Paris at half-past 7 p.m. on Tuesdays and Fridays; and the train will consist of three saloon carriages, fitted with forty-two beds, a refreshment saloon, and a sufficient number of luggage vans, in which the luggage will be so arranged that it can be examined in the vans by the Customs officers at the frontier stations, thus avoiding the delay and annoyance unavoidable when the luggage has to be removed from the train. There will be no change of carriages between Paris and Giurgevo, and it is expected that the entire journey between Paris and Constantinople will be completed in about seventy-five hours.

The French Government owns 2316 miles of railroad, including a large number of short lines in various parts of the country; but there are 1260 miles in lines which form something like a single system. These are chiefly roads which the companies that undertook them were unable to complete, because they did not seem likely to be profitable. Some of these lines the Government lease to the great companies, some to companies organized especially to work them, and some it works itself. As a whole, they are very unprofitable, the working expenses being 96 per cent. of their gross earnings. On many lines the expenses are more than the gross earnings—30 per cent. more on one line, and 17 per cent. more on others. Including interest, this system has cost £1,600,000 more than it has brought in during three years.

The following description has been given of a wire railway in connection with the coal mining industry established near the Hersteigg, the products of which it brings to the main line belonging to the Southern Railway of Austria. In its alternating rise and fall during its distance of 3,000 yards there is a useful excess of incline of about 142 yards, which, it is said, suffices to keep the line in self-acting working, after it has been started by means of the 12-horse power engine provided for that purpose. When there is no return load to be sent to the mine, the speed of the train can be regulated by a brake. Under these circumstances the cost of working the line is estimated at about 5½ cents per ton of coal. In its general arrangement the railway forms a straight line, and consists of two drawing ropes and the train rope. The line which is used for conveying the coal to the station is 1.10in. thick, and is composed of nineteen steel wires, each 0.18in. in diameter. The line on which the coal vessels are returned to the mine is only 0.66in. thick, the nineteen steel wires of which it is composed being only 0.13in. thick. Both ropes consist of wires about 765 yards long, coupled to each other, and for the ropes a breaking strength of 73 tons per square inch section is guaranteed. At the ends of the ropes weights of five tons and three tons are applied in the usual way for obtaining the proper tension. The distance between the seventeen supports varies from 60 to 400 yards. The train rope is 0.6in. thick, and consists of twelve soft steel wires of 0.07in. in diameter, and runs at a speed of about $1\frac{5}{8}$ yards per second. The vessels which convey the coal follow each other at a distance of about 83 yards. Thus thirty-six are always on the way to and the same number coming from the station. Each vessel contains about 10 bushels, or about a quarter of a ton of brown coal, the total quantity carried per hour being about 17½ tons. The cost of the line was about £5,000.—*Engineer.*

In a report to the Board of Trade on the explosion of the boiler of a tank locomotive at Summerlee Ironworks, Coatbridge, in March last, Mr. J. Ramsay says: The locomotive was made for Messrs. the Summerlee Iron Company by Mr. Andrew Barclay, engineer, Kilmarnock, in October, 1869, and was, therefore, upwards of thirteen years old. The inside of the barrel plating is reduced by corrosion to about ¼ in. thick, and over the whole of the surface of the plates that can be seen there is a great deal of pitting; but grooving along the inner edge of a longitudinal seam of the barrel —in the vicinity of the fore left-hand wheel of the locomotive—which has reduced the plate at that part to about $\frac{1}{16}$ in. mean thickness, is undoubtedly the cause of the explosion. Grooved plates are not by any means as strong as thin sound plates of equal thickness, and even if they were as strong they cannot sustain with safety the enormous working stress of 35,000 ℔. per square inch of cross-plate section, which was about the stress on the grooved part of the plate when the steam pressure was at 112 ℔. per square inch. It is, therefore, not surprising that the explosion occurred. Mr. T. W. Trail adds: It is evident that the boiler was worn out and the inspection inadequate.

ORDNANCE AND NAVAL.

Steel in its Relation to Modern Guns.—At a recent meeting of a number of artillery and naval officers at Karlsborg, Sweden, Captain John Bratt, of the Swedish Artillery, read a paper on "The Steel Industry and its Relation to the Manufacture of Modern Guns." The author has for many years been the Government inspector of Swedish gun factories, and has paid many visits to the gun factories of Russia, Germany, and France. In his paper, having given an account of the importance of iron in modern civilization, the author stated that there was no other raw material which had been subjected to such a successful process of refining. It was in its most important and interesting form—viz. steel—that he intended to deal with it on this occassion. Captain Bratt proceeded to show, by drawings and diagrams,

the metallurgical processes and methods of refining in use at the present moment. Having referred to the various kinds of steel and their manufacture, the author urged the necessity of subjecting all cast steel, of whatever kind, to a mechanical process of treatment by which the cavities which are caused by the gases contained in every steel bath are entirely removed. The steel, he said, should be perfectly close and homogeneous in order to be suitable for manufacture. The means of obtaining this indispensable quality was the steam hammer. The largest at present in use were those at Le Creusot, Essen, and Perm (Russia). The latter rested on the largest block of cast iron in the world. It had a cubic contents of 83 cubic meters, and contained 700 tons of pig-iron. The difficulties, the cost, and, in some instances, the danger of forging great blocks of steel made it a matter of moment to discover some method whereby the gases in the bath might be removed and a homogeneous steel produced. Such a method was discovered in 1870, and had been perfected at Terre-Noire, and consisted chiefly in adding a flux of silicon in the Martin furnace immediately before the steel is tapped. The author showed some samples of steel made at Bofors, in Sweden, by that method. One was taken from the hearth immediately before, and the other just after, the silicon was added. The former had a surface similar to a fracture, and was covered with blisters, whereas that of the latter was perfectly smooth. The Bofors Ironworks were the first Swedish works which had procured the Terre-Noire patent, and thus the first producers of this kind of steel in Sweden; and the method had a special interest to those assembled by the fact that guns of Bofors steel had been manufactured with the most satisfactory result, which led him to believe that Sweden would very soon make her own guns. The author next gave an account of Krupp's manufacture of forged steel guns. The Essen works had in 1848 employed 72 men; in 1882 their number was 16,000, while some years ago they had in five months turned out no less than 1,400 pieces of artillery. In twenty-four hours the works could roll sufficient rails for a Swedish mile of railway (six English miles). Captain Bratt then referred to his personal study of the Krupp method. He had been present at the casting of guns at the foundry which had been established by Messrs. Krupp near St. Petersburg. He stated that the ingots for some of the largest guns numbered up to 500. He then described the heating of the metal for forging, and the difficulties attending this operation, the forging under the steam hammer, whereby the cast metal is compressed to under four times its original size, and, finally, how the gun, after being bored and turned, is made red-hot and hardened in oil. The author next gave an account of the experiments which had during the last few years been made in Sweden, to solve the question of producing first-class guns of close cast steel by the Terre-Noire method. The trials made included the bursting of a smooth-bore 4-lb. muzzle-loading gun. It had shown a very high degree of resistance, and had, in fact, only been burst by loading it right up to muzzle. No less than 1,041 shots had been fired from a 12-centimeter rifled breech-loader which was at last burst under the excessive pressure in the chamber of 5,500 atmospheres, while the normal one was from 2,000 to 2,100. The last experiment was the firing of three 8-centimeter guns of the new model gun of the Swedish artillery. Each of these guns had, without suffering in the least degree, fired 2,000 shots, with normal charges. Two of them were then, after 152 and 154 attempts had been made, burst, under a pressure in the chamber of 5,000 atmospheres, the normal one being 1,800. The third gun could not be burst, but only cracked in the breech. All these guns had been cast at Bofors, and were finished at the gun factory at Finspong. In conclusion, Captain Bratt stated that lately a competition had sprung up between these two works, which had before worked in concord. This was caused by the fact that the problem, whether first-rate steel guns could be made in Sweden, had been solved, and that these two works desired in future to be independent of each other in gun making. At Bofors there was now erecting the plant required for finishing guns, and at Finspong a steel foundry. Both had received orders from the government, and he trusted that at no distant date they would also receive them from foreign governments.

IRON AND STEEL NOTES.

Basic Steel at Amsterdam. — England, France, Germany, Austria, and Belgium have all alike contributed specimens of basic steel to the Amsterdam Exhibition. The specimens have been got together by Messrs. Thomas & Gilchrist, the inventors of the process, and they testify to a capability by this metal which, viewed in the light of earlier knowledge in this branch of metallurgy, is remarkable. The British specimens from the Patent Shaft and Axle-tree Company's works, at Wednesbury, are designed to show the applicability of the steel to locomotive tubes, tin plates, Galloway tubes and rivets. France contributes striking illustrations of the ductility and the malleability of the metal. The specimens of plate from the Creusot Works have borne the severest punishment, including the flanging 6in. by 6½in. deep of a central hole and the edges of a plate 59in. thick and 165lb. in weight, and beaten out, the unflanged portion into a stride or A-shape, while the flanged edge is almost elliptical in outline, and the hole circular. The capability of the angle iron appears in the sample bent hot into an S-shape, the upper possessing a smaller radius than the lower curve. The quality of the rails, made also by Messrs. Schneider, is told in the one complete twist on a length of 5ft. 9in., weighing 110lb. The handling of the basic plant by the Hoerde Hutten Verein, Germany, is shown mainly in a large collection of rail sections, tram rails, and sleepers, together with some plates. Similar sections come from the Teplitzer Walzwerk of Teplitz, Austria. The tenacity of the steel comes out in the manner in which the specimens have submitted to cold bending and twisting. Demonstrative of the different degrees of hardness which the metal

can be made to acquire is the circumstance that the carbon in the rails is 0.35 per cent., and that in the rolled sleepers 0.08 per cent. Test pieces made from the sleeper material have a limit of elasticity from 13.4 to 14.6 tons per square inch, a tensile strength of from 25.4 to 27.6 tons per square inch, an elongation of between 29 and 33 per cent., and a contraction of area of between 53 and 63 per cent. Tubes flanged and bent close, cold, yet without sign of fracture, are the contribution from a second Austrian steel works, the Witkowitzer Bergbau. The Belgian contribution is an assortment of wire from the finest to the ordinary thick from the Les Acieries d'Anglear, of Renory, Ougree. The content of carbon in this wire is from 0.12 to 0.15 per cent. The same firm likewise show steel wire upon their own account.—*Engineer*.

Russian Basic Steel.—By Sergius Kern, M.E., St. Petersburg.—It was a very interesting object for the writer to test the qualities of the basic steel of Russian manufacture. Near St. Petersburg, the Alexandrovsky Steel Works are commercially working the basic process in Siemens-Martin furnaces.

The plate steel welds quite like iron; in fact the Nevsky Works, St. Petersburg, rolling the ingots, make, out of the remaining scrap, piles which, heated to a welding heat, are rolled into capital plates for different purposes.

The following are the results of trials of the steel plates:

Unannealed Plate.

Thickness in Inches.	Breaking Weight: Tons per Square Inch.	Elongation in 8 Inches. Per cent.
½	26	29
Annealed Plate.		
½	22	36.25

The chemical composition of the steel runs as follows:

	Per cent.
Carbon	0.10
Manganese	0.43
Phosphorus	0.02
Sulphur	0.02
Silicon	traces
Copper	none

I am informed that the raw materials charged into the furnace contain, on the average, 0.75 per cent. of phosphorus.

I am very happy to state that the great invention of Messrs. Thomas & Gilchrist is worked in Russia in such a satisfactory way.

BOOK NOTICES

PUBLICATIONS RECEIVED.

Monthly Weather Review for April and May. Washington: Office of Chief Signal Officer.

Papers of the Institution of Civil Engineers.—Through the kindness of Mr. James Forrest we are in receipt of the following:

Covered Service Reservoirs. By William Morris, M. I. C. E.

The Behavior of Steam in the Cylinders of Locomotives during expansion. By Daniel Kinnear Clark, M. I. C. E.

Slipway for Pleasure Boats. By Charles James More, M. I. C. E.

Tests of Riveted Joints. By Charles Henry Moberly, M. I. C. E.

Weights of Structures Estimated Graphically. By Joseph Heywood Watson Buck, M. I. C. E.

Wire-Rope Street Railroads. By William Morris, M. I. C. E.

Long-Distance Telephony. By George M. Hopkins.

The Electrical Transmission and Storage of Power. By Dr. C. William Siemens, F. R. S.

Summit-Level Tunnel of the Blaenau-Testiniog Railway. By William Smith, M. I. C. E.

Machine Tools. By Alexander McDonnel, M. I. C. E.

Irrigation in Colorado. By Patrick O'Meara.

Signal Service Notes.—No. 4.—The Use of the Spectroscope in Meteorological Observations. By Winslow Upton, A. M.

No. 5.—Work of the Signal Service in Arctic Regions. Under direction of Maj.-Gen. W. B Hazen. Washington: Office of Chief Signal Officer.

Steam Heating. An Exposition of the American Practice of Warming Buildings by Steam. By Robert Briggs, M. I. C. E. Science Series No. 68. Price 50 cents. New York: D. Van Nostrand.

At no time has the demand for a knowledge of the details of the steam heating system been more urgent than now. No other plan accomplishes the heating of whole neighborhoods from a single source; and as this promises to be the method of the future, the inquiry for specific information has been lately very brisk.

This little treatise of the late Mr. Briggs is not new. It has been previously published on both sides of the Atlantic, but the previously available has been exhausted, and the want for such information as this essay affords is more plainly manifest than ever before.

It is put in a compact form as becomes a practical essay, and the mechanical details are illustrated.

Practtical Electric Lighting. By A. Bromley Holmes. London and New York: E. & F. N. Spon. Price $1.50.

This is an outline of the Science of Electric Lighting, beginning with the simplest facts and phenomena and extending to later applications, including the use of secondary batteries.

Of course, in only 154 pages nothing is fully treated; but enough is given for the purposes of the reader who requires only a brief statement of the whole subject. The number of such persons at present is certainly large.

Water Supply. By Wm. Ripley Nichols. New York: John Wiley & Son. Price $2.50.

This important subject is here treated from a chemical and sanitary standpoint. The power of water to dissolve various substances is first considered in an introductory chapter. Then in order are presented: Chapter I.—Drinking-Water and Disease. Chapter II.—Water Ana-

lysis. Chapter III.—Rain Water as a Source of Supply. Chapters IV. and V.—Surface Waters as Sources of Supply. Chapter VI.—Ground Water as a Source of Supply. Chapter VII.—Deep Seated Water as a Source of Supply. Chapters VIII. and IX.—Artificial Improvement of Natural Water. Chapter X.—General Considerations.

The treatise forms a most valuable supplement to the works which treat water supply from the engineering point of view only, and is quite as important to the engineer.

ELEMENTARY APPLIED MECHANICS. Part II. By Thomas Alexander, C. E., and Arthur Watson Thomson, C. E., F. R. S. London: Macmillan & Co.

In this second volume of their Applied Mechanics the authors have treated the subject of transverse stress and strain in a systematic manner. Bending movements and shearing forces are considered for the different conditions of loading usually met with, the loads being fixed or moving, or both combined.

In treating of bending and shearing, the common forms of cross-section are given, together with some not much employed. The calculus is employed in a few cases, but students not familiar with it will be able to apply the results.

POCKET MANUAL FOR ENGINEERS. By John W. Hill. Providence: William A. Harris. Price $1.50

The author of this manual has become widely known as an expert in matters relating to mechanical engineering. His reports upon trials of engines and boilers are referred to as familiar facts by engineering writers on both sides of the Atlantic.

Mr. Hill only claims for the book that it "contains a limited number of facts, data and formulæ, together with several practical illustrations in steam and mechanical engineering, which, it is hoped, will be of value to the profession and others."

It is fair to presume that the collection of formulæ and tables is just what is needed by the working mechanical engineer.

THE SEVENTH VOLUME OF THE SANITARY ENGINEER. New York: The Sanitary Engineer. Price $3.00.

Among the articles of permanent value may be mentioned "Letters to a Young Architect on Heating and Ventilation." By Dr. J. S. Billings, U. S. A.

"Steam Fitting and Steam Heating." By "Thermus." A series. (Illustrated.)

"The Edison System of Wiring Buildings for the Electric Light." (Illustrated.)

Illustrated descriptions of the sanitary arrangements in the residences of Cornelius Vanderbilt, Esq., the Berkshire Apartment House, Home for Aged Females, and the Duncan Office Building.

"The Steam Heating Companies in New York." Illustrated description of.

Full abstract, with illustrations, of the records in the "M'Closkey Patent suit for Trap Ventilation."

"The New York Water Supply;" a series of articles on the suppression of waste of water, giving the experience of European cities in attempting to deal with this problem, the practice now in vogue there, and the situation tn American cities. These articles will be found of great value to water works authorities and all who are interested in this question.

A discussion of the various projects for increasing the water supply of New York, including the Croton Aqueduct scheme, appears in almost every number in this volume.

"Atlantic Coast Resorts." A report by E. W. Bowditch, C. E., to the National Board of Health.

"How the Plumbing Law is enforced in New York." A description of the methods employed by the department.

"Germs and Epidemics." By Dr. John S. Billings, U. S. A.

"Malaria" (a series). By George M. Sternberg, Surgeon U. S. Army.

"Gas Fitting in an Office Building;" description of work in Mills Building.

"American Practice in Warming Buildings by Steam." By the late Robert Briggs, M. Inst. C. E.

The value of these articles is undoubted, and the plan of putting the collection in a permanent form is an excellent one.

The reader of the volnme may feel assured that he finds there presented the thoroughly scientific methods of sanitary reform.

MAN BEFORE METALS. By N. Joly. New York: D. Appleton & Co. Price $1.75.

This is a late addition to the International Scientific Series. The subjects treated are: Prehistoric Ages—The work of Boucher de Perths; The Bone Caves; The Peat Mosses and Kitchen Middens; The Lake Dwellings and the Meraghi; Burial Places; Prehistoric Man in America; Man of the Tertiary Epoch; The Great Antiquity of Man.

Part II. is devoted to Domestic Life, Industry, Agriculture; Navigation, and Commerce, The Fine Arts, Language and Writing, Religion, The Portrait of the Quaternary Man.

The work is a compact and skillful treatise, presenting an epitome of the latest discoveries and best writings on the subject of the earlier stages of development of the human race.

MINERAL PRODUCTS OF THE UNITED STATES. —A report entitled "The Mineral Resources of the United States" is now in press, and will shortly be published, by Mr. Albert Williams, jr., chief of the Division of Mining Statistics and Technology, United States Geological Survey, Hon. J. W. Powell, Director. This report is for the calendar year 1882 and the first six months of 1883. It contains detailed statistics for these periods and also for preceding years, together with much technical and descriptive matter. The compilation of special statistics has been placed by Mr. Williams in the charge of leading authorities in the several branches, and the results will therefore be accepted with confidence. An abstract given in the prospectus, presents a summary of the productions of the following

Coal, iron, gold, silver, copper, zinc, lead, tin, nickel, cobalt, mercury, manganese, petroleum, salt, marl, mica, soapstone, brick and tile, lime, cement, graphite, and precious stones.

A MANUAL OF MARINE ENGINEERING, COMPRISING THE DESIGNING, CONSTRUCTION, AND WORKING OF MARINE MACHINERY. By A. E. Seaton, Lecturer on Marine Engineering to the Royal Naval College, Greenwich, M.I.N.A., M.I.M.E., &c. London: Charles Griffin & Co., 1883.

The wonderful activity which has prevailed during the last few years in the shipbuilding industry, and the rapid development of the marine engine, has created an opening for a thoroughly good and comprehensive work dealing with the application of theoretical principles to the design and construction of marine machinery, especially in its later forms, and this opening has found a worthy response in the volume before us, in which may be discerned the results of much close study and practical work among marine engines. Although the work is thoroughly up to date, and deals with such recent examples as the Arizona and the City of Rome, yet it traverses the whole area of its subject, beginning with a general explanation of the object aimed at by the marine engineer, viz., the propulsion of a ship through the water, and the means by which this is effected. It then turns to the subject of power and the methods by which it is measured and expressed in the case of the steam engine, considering the various sources of loss and the means by which they may be minimized or overcome by the aid of skillful design. The next point is naturally the resistance of ships and the indicated horse power necessary for speed, and this, although not strictly within the province of the designer of engines, is treated at considerable length, and all the best approved methods of calculation are given. The power of the engines having been thus determined, the author discusses the space occupied by them, pointing out the peculiarities of each type in this respect, and the kind of work for which it is best suited. Two chapters are then devoted to the consideration of the action of the steam in the cylinders, and the comparative advantages and disadvantages of high and low grades of expansion, compounding and non-compounding, the use of two, three, four, and six cylinders, the ratio of cylinder capacity, and the like. Leaving this somewhat theoretical subject we find several successive chapters appertaining to mechanical details, each principal part of the engine being treated separately, ample information being given concerning the strains to which it will be subject, together with many reliable formulæ for the determination of its dimensions, and tables of examples compiled for actual practice.

As might be anticipated, the question of valves and valve gears receives considerable attention. The author begins with a short historical account of the various kinds that are employed, and then explains the method of designing different kinds of valves. From this he proceeds to the link motion and describes the different modified arrangements that have been offered in place of it, such as Hackworth's, Marshall's, Joy's, and Sell's gear. We then come to the portion devoted to boilers, which occupies four chapters. The first relates to combustion and evaporation, the second to the general design, the third to construction and detail, and the fourth to mountings and fittings. After the boilers there follows a considerable amount of miscellaneous information relating to fitting the engines to the ship, to steam pipes, reversing gear, turniug gear, governors, gauges, feed heaters, and materials.

Thus our readers will see that this important subject of marine engineering has been treated with the thoroughness that it requires, and that no department has escaped attention, while the observations on each bear evidence of being the outcome of a quick, practical mind, trained in the workshop and drawing office, and not in the lecture room. The volume is well printed and liberally illustrated, some of the engravings, however, being scarcely so good as the book deserves. The frontispiece, showing the engines of the s.s. Arizona, is by the way, copied from our own columns, a fact which the author has omitted to mention.—*Engineering.*

MISCELLANEOUS.

ADHESIVE POWER OF NAILS AND SCREWS.— The extensive use to which nails and screws are put in construction lends considerable interest to any records of experience tending to discover their holding power. Haupt, in his "Military Bridges" gives a table of the holding power of wrought-iron 10d. nails, 77 to the pound, and about 3 inches long. The nails were driven through a 1-in. board into a block, and the board was then dragged in a direction perpendicular to the length of the nails. Taking a pine plank nailed to a pine block with eight nails to the square foot, the average breaking weight per nail was found to be 380 pounds. Similar experiments with oak showed the breaking weight to be 415 pounds. With 12 nails to the foot square the holding power was 542½ pounds, and with six nails in pine 463½ pounds. The highest result obtained was for 12 nails to the square foot in pine, the breaking weight being in this case 612 pounds per nail. The average strength decreases with the increase of surface. Tredgold gives the force in pounds required to extract 3d. brads from dry Christiana deal at right angles to the grain of the wood as 58 pounds. The force required to draw a wrought iron 6d. nail was 187 pounds, the length forced into the wood being 1 inch. The relative adhesion when driven transversely and longitudinally is, in deal, about 2 to 1. To extract a common 6d. nail from a depth of 1 in. in dry bench, across grain, required 167 pounds; in dry Christiana deal, across grain 187 pounds, and with grain, 87 pounds. In elm the force required was 327 pounds across grain, and 257 with grain. In oak the figure given was 507 pounds across grain. From further experiments it would appear that the holding power of spike nails in fir is from 460 to 730 pounds per inch in length, while the adhesive power of screws 2

in. long, 0.22 in. in diameter at the exterior of the threads, 12 to the inch, driven into ½ in. board, was 790 pounds in hard wood, and about one half that amount in soft wood.

Substitute for Guttapercha.—A German chemist, Herr Maximilian Zingler, has just patented a new process for manufacturing a substitute for guttapercha. About 50 kilogrammes of powdered copal, and 7½ to 15 kilogrammes of sublimed sulphur are mixed with about double the quantity of oil of turpentine, or with 55 to 66 liters of petroleum, and heated in a boiler provided with a stirring apparatus to a temperature of 122 degrees to 150 degrees C., and stirred until completely dissolved. The mass is then allowed to cool to 38 degrees C., and is then mixed with about three kilos of casein in weak ammonia water, to which a little alcohol and wood spirit has been added. The mass is then heated to the former temperature (122 degrees to 150 degrees C.) until it is a thin fluid. It is then boiled with a 15 to 25 per cent. solution of nutgall or catechu, to which about half a kilo of ammonia has been added. After boiling for several hours the mass is cooled off, washed in cold water, kneaded in hot water, then rolled out and dried. It is claimed that the product is produced much cheaper, and cannot be detected from the real article. It is said to wear equally as well.

An Asphalte Mortar.—The *Centralblatt der Bauverwaltung* describes a patented composition made at a factory in Stargard, Pomerania, which has for some years past been used with perfect success on the Berlin-Stettin railway for wall copings, water-tables, and similar purposes requiring a waterproof coating. The material is composed of coal tar, to which are added clay, asphalte, resin, litharge and sand. It is, in short, a kind of artificial asphalte, with the distinction that it is applied cold, like ordinary cement rendering. The tenacity of the material when properly laid, and its freedom from liability to damage by the weather, are proved by reference to an example in the coping of a retaining wall which has been exposed for four years to the drainage of a slope 33ft. high. This coping is still perfectly sound, and has not required any repair since it was laid down. Other works have proved equally satisfactory. In applying this mortar, as it is termed, the space to be covered is first thoroughly dried, and after being well cleaned is primed with hot roofing varnish, the basis of which is also tar. The mortar is then laid on cold to thickness of about three-eighths of an inch, with either wood or steel trowels, and is properly smoothed over. If the area covered is large, another coating of varnish is applied, and rough sand strewn over the whole. The waterproof surface thus made is perfectly impregnable to rain or frost, and practically indestructible. The cost of the material laid is estimated at not more than 5*d.* per square foot; and it is stated that this price can be reduced by at least 1*d.* for large quantities put down by experienced workmen.

An exchange says: The Reading Railroad Company has been testing an ingenious device for lighting the platforms and steps of railroad cars at night, and also station platforms in the vicinity of the car steps. The object of the device is obtained by means of a lantern placed under the steps of the car. The rise of each step is provided with a window of thick plate glass, through which the light illuminates the steps. In the back of the lantern is set a door which has a bull's-eye of suitably colored glass, through which the light also shines, and may serve as a substitute for the danger and other signals usually placed upon the platform or railing of the rear car. The lamp inside the lantern is an ordinary double-wick burner, and for the purposes of illumination on the trial trips mineral sperm oil was used. The lamp appears to have withstood the shocks of coupling and the jars incident to the application of air brakes to the train, going through tunnels and passing moving trains without a noticeable flickering of the flame from excessive drafts, or a dislocation of any part of the lamp from shocks. It not only lit up the steps and a space of 5 ft. to 6 ft. on either side, but also the ground beneath and around them, thereby enabling passengers to see both the steps and the platform when the train was drawn up at a statian. Apart from guarding against accidents and consequent risk of life, other advantages are claimed.

Scale of Hardness of Common Vessels.—A new scale for comparing the hardness of metal has been compiled by Mr. Galliner, who in his experiments used small cylinders with conical points, and passed them loaded up to five kilogrammes over polished plates of the respective metals, from which they did not materially differ in hardness. Thus he found, indicating the hardness of pure soft lead by 1, that tin is represented by 2; hard lead by 3; copper 4 to 5; metal for bearings (85 copper, 10 tin, and 5 zinc) 6; tempered cast iron 7; fibrous wrought iron 8; grey cast iron 10 to 11; mild steel 12 to 13; crucible steel, blue 14, violet 15, straw color 16; hard bearing metal (83 copper, 17 zinc) 17; and very hard steel 18.

The Ohm and "Mho."—During his recent lecture at the Institution of Civil Engineers, Sir William Thomson proposed the term *mho* for a unit of conductivity. Conductivity is the reciprocal of the resistance which is measured in ohms, and *mho* is found by saying "ohm" into a phonograph, and then turning the drum backward. How Sir William finds the reciprocal of a value equivalent to a word spoken backwards he does not explain, and, on the whole, we think he has chosen a somewhat fanciful way of finding a new electric term. Nevertheless, a convenient unit word for conductivity would be useful. We may add that in Sir Willam's opinion electricians may safely take the British Association unit of resistance as 0.9868 of the true ohm defined as 10^9 centimeters per second, or the Siemens unit as 0.9413 of the ohm.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXVIII.—OCTOBER, 1883.—VOL. XXIX.

ON THE DEFENSE OF HARBORS BY SUBMARINE MINES.

By LIEUT. CHARLES SLEEMAN, R. N.

From the "Journal of the Royal United Service Institution."

It is with the greatest diffidence that I venture to read a paper this afternoon on any component part of that vast and comparatively undeveloped subject of torpedo warfare, and most certainly should never have offered so to do, had I not received a very flattering and encouraging request from the Council of this Institution to prepare a similar paper at the end of 1879, when I was on the eve of departure for China, and therefore unable to comply, and had I not also received valuable assistance from many able authorities on torpedo matters.

The magnitude of my subject, and the short space of time at my disposal, will only allow of a mere cursory glance at most of the details of harbor defense, and but little more at those of submarine defense, which really constitutes the main feature of my paper, and it is on these latter details that I look for some clear light to be thrown by the discussion which I hope may be invoked by to-day's work.

I will here mention that, with the exception of Captain Long's lecture on "Naval Blockade," and the one on "Torpedo Boats" by Mr. Donaldson, there has been no paper read at this Institution on any of the various branches of torpedo warfare since 1875, and only some half-a-dozen since its foundation.

The various items constituting a system of defense for harbors, river mouths, &c., may be broadly classified as follows:

1. Fortifications.
2. Vessels of war.
3. Guard and torpedo boats.
4. Submarine defense.

The sheer impossibility of preparing a paper which should contain an adequate treatment of each of these items, in a manner worthy to be presented to a scientific naval and military audience such as are usually gathered here, decided me not to even attempt a partial diagnosis of the first three of those items, but to devote my paper to the last but not least important of them, namely, "*Submarine defense.*"

I think it best to mention here that, on the completion of my paper, I shall, by the kind permission of Captain McEvoy, explain the manipulation of two of his many inventions, being his "Single Main System of Torpedo Defense," and his "Submarine Detector." The inventor is a gentleman who has devoted the greater part of his life to the invention and improvement of torpedos and other military matters, and who is well known to many of you.

In discussing the subject of submarine defense, it will be best for the sake of greater clearness, to divide it into its

constituent parts; these may be defined as—

A. *Systems of fixed mines.*

B. *Locomotive torpedo submarine boats.*

C. *Submarine boats.*

D. *Passive obstructions.*

In dealing with these points, I prefer to leave that one prefixed (A) until the last, constituting, as it does, the main feature of submarine defense, and therefore requiring special and close attention.

I will then first treat of—

B. *Locomotive torpedo submarine boats.*—This part may be further divided into *controllable* and *uncontrollable* boats; the former class being represented by the Lay torpedo-boat, the latter by the Whitehead or fish torpedo-boat.

These representative submarine weapons being well known, and time pressing, I will not attempt to describe them, nor enter into the question of their merits and demerits. With regard to the Lay torpedo-boat, I have authority for stating that it has been of late considerably improved, its speed having been greatly augmented, and its manipulation much simplified. I believe it is generally conceded that a *controllable* locomotive submarine torpedo-boat is the form best suited for the requirements of harbor defense.

Then I come to the question of—

C. *Submarine boats.*—Up to the present time no practical adaptation of this mode of progression under the sea has been wrought out, though I am told of a submarine boat now under trial in Sweden that is to be a perfect success. It is to Russia that we should look for a solution of this difficult problem; for there the matter has of late years received the closest attention, but as yet without any definite practical result. I confess to having very little faith in submarine boats ever being brought to that state of practical efficiency which is needed for the work they are intended to perform in connection with torpedo *defense*, such as the destruction of fixed mines and their cables, &c., owing to the insuperable difficulty of piercing the intense gloom of the surrounding matter, when proceeding beneath the surface of the sea. Of course if, by the aid of electricity or other power, this difficulty of vision be overcome, and also that of the motor for these craft to such an extent that it becomes a feasible and safe matter to start from a ship in the offing in one of these subaqueous craft, pick up the harbor entrance, and moving about at a considerable depth, cut the *steel-wire* mooring ropes and *armored* branch cables of the submarine fixed mines, then these craft will most certainly demand the bestowal of far more attention than I think they deserve as at present constructed and manœuvred. This brings me to question—

D. *Passive obstructions.*—This mode of barring a channel or other entrance, either by the sinking of weighted ships, piles, frames, or other methods, will no doubt prove extremely useful in many instances. The action and construction of such obstructions are, I consider, sufficiently well understood as not to require any special attention in this paper.

I must here explain that the subordinate positions I have *seemingly* assigned to the foregoing particulars of submarine defense, is due to the short space of time apportioned to my paper, and not to any want of belief on my part of their *great value* under *special* circumstances.

I will now proceed with the important question of—

A. *Systems of fixed mines.*—By a system of fixed mines I intend to be understood all methods of submarine defense necessitating the employment of fixed or anchored mines, and such I consider to form the backbone and most important factor of any scheme of coast defense that may be devised; the question (A) may then be resolved into the minor ones of—

1. *Systems of self-acting fixed mines.*
2. *Systems of dependent electrical fixed mines.*

I will first treat of the systems considered under section (1): these may be subdivided into *electrical* self-acting and *mechanical* self-acting fixed mines—the former including those whose firing agent is electricity—the latter those whose firing agent is mechanism.

The special application of a system of self-acting fixed mines for coast defense purposes is that of defending certain isolated and other portions of the defensive ground, where, for economy's sake, or for some other cause, it is not necessary to advert to the dependent form of defense.

The one great objection to any system of self-acting fixed mines is that the ground protected by them becomes a source of danger alike to friend and foe, and also the danger that has hitherto attended the planting and picking up of such mines; as regards this latter source of danger, so strongly condemned by Major Parnell in his lecture on "Coast Fortifications," where he says, "As for mechanical mines, they are hardly worth the mentioning, and are sources of danger to their employers rather than to those they are employed against," Captain McEvoy has, by some simple yet very practical improvements, I venture to assert, almost entirely expunged this objection. Were it not for the question of economy—the bugbear of all schemes of defense—I should advocate the entire *disuse* of self-acting fixed mines, and wholly depend on the dependent electrical fixed mines for coast defense, but as this economy question cannot be ignored, then this branch of submarine defense requires the most careful consideration.

Dealing first with the question of electrical self-acting fixed mines, we notice that but very few attempts have been made to devise anything practical in this line, for the reason that where any system of electrical mine contains within itself the power of firing its fuse, an inherent risk of premature explosion must always, in a more or less degree, be present, and this source of danger exists, though in the latter in a minor degree, in the only two inventions of this kind that claim special notice, viz., the Hertz and the McEvoy systems of electrical self-acting fixed mines. The former has been in use for some considerable time, especially by German torpedoists, and it was employed by the Russians on the Danube in 1877, when it was on one occasion successful, whilst the McEvoy system has been only lately perfected.

The Hertz system may be roughly described to consist of a mine case, on the head of which are screwed five lead cylinders; inside of these are placed five glass tubes containing chlorate of potash; beneath the lead cylinders, on the inside of the mine, are affixed five dry batteries; on either of the lead cylinders being struck by a passing vessel the glass tube is broken, and thus the battery rendered active and the mine exploded. This form of self-acting fixed mine I consider to be a most dangerous one, either to plant or to pick up; the latter fact I can vouch for, having had the unpleasant and dangerous task of clearing a part of the Danube in which some Hertz mines had been laid by the Russians. Its uncertainty I can also testify to, having seen one of the Hertz mines that had been torn from its moorings by the screw of a passing Turkish monitor, and drifted on to the bank of the river without exploding; one of its lead cylinders was considerably bent, but not sufficiently so to smash its glass tube. I was also once unwittingly anchored for some time over one of these mines, whilst on board the turret vessel "Hifsi Rahman," in company with the late Captain Manthorp, R. N., which, we subsequently learnt from the Russian officers, they had attempted to explode by every means in their power, without, I am happy to say, succeeding.

MC EVOY'S.
ELECTRIC SELF-ACTING MINE.

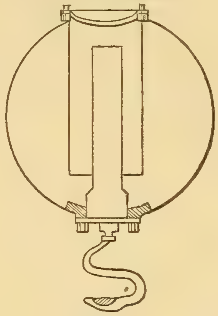


Fig. 1

Captain McEvoy's electrical self-acting fixed mine, a section of which is shown in Fig. 1, consists of a small battery and a circuit closer placed within the mine case, also ingenious electrical arrangements for the prevention of premature explosions; as will be seen, he depends for the action of his mine on the contact of a hostile ship with the actual mine itself, and not with any particular excrescence. Captain McEvoy has not permitted me to exhibit this clever invention here; but I may mention that General Abbott, of the United States Torpedo Department, has ordered one of these mines for trial in America, as he considers it to be the most perfect one of its kind.

To eradicate the inherent danger I have before spoken of that exists in the ordinary electrical self-acting fixed mines, a system known by the name of the "sunken battery" was introduced. Here a set of five or more buoyant fixed mines containing circuit closers are connected with a voltaic battery, which is inclosed in a strong metal box, and sunk near its mines. This is an old idea, but it has hitherto failed, owing to the difficulty of obtaining a reliable battery; but with the present efficient state of the Leclanché, this difficulty has been overcome. I will now proceed with—

Mechanical self-acting fixed mines.—Of these there are numerous and varied kinds, principally devised by the Confederates, and of them the "Singer" falling weight invention has been more extensively used, and oftener successful, than any other species. Now, this form of mine Captain McEvoy has taken in hand of late, and, I venture to say, so improved it, that this mechanical self-acting fixed mine may now be considered as capable of supplying the want that has been long felt in submarine defense, namely, a mechanical self-acting mine that can be moored with perfect safety, whose action is certain, and that can be recovered with the minimum amount of risk. The original falling weight has been altered in shape and manner of resting on the head of the mine case. This new form of weight is prevented from falling off until a mine has been placed in position by a very simple mechanical contrivance; and to enable the recovery of one to be effected in perfect safety, another arrangement has been introduced, whereby, though the weight may be knocked off, yet the mine will not be fired. I am not at present at liberty to divulge anything further in connection with this newly improved mine, as it has not been patented. If the proper precautions be observed when carrying out the work of recovering self-acting fixed mines, such as waiting for smooth and dead low water, employing an experienced and careful boat's crew, and also using the submarine detector, there should be no danger or difficulty whatever in picking up the "McEvoy improved Singer self-acting mine;" and this I aver with the experience I possess of such work with *unknown* self-acting mines.

I will now make some remarks on the subject of "Dumb Mines," or "Dummies." The reason originally advanced for the employment of such things was to increase the difficulties and tediousness attendant on any attempt that might be made by an enemy to clear a space protected by fixed submarine mines. Now, this should most decidedly be the only duty delegated to dumb mines, instead of which there seems to be a most pernicious tendency at the present time towards the use of dummies in the place of actual mines, and to trusting the safety of a harbor entirely to moral in the place of real defense. There is, no doubt, a possibility of utilizing the moral effect of submarine defense for the purpose of deterring the attack of a hostile fleet, but to depend on the safety of any place by such means alone, is, in my opinion, radically wrong, and is opposed to all the principles of real defensive warfare. I think it is hardly necessary to mention here, that were it England's misfortune to be drawn into a European war, then any of her opponents' harbors thus morally protected would soon fall into the hands of her fleet. This concludes my remarks on systems of self-acting fixed mines, the details of which want of time prevents me from more fully discussing; not the less, I must impress on torpedoists in general, the great importance that attaches to this branch of submarine defense; for, owing to the necessity of economy, electrical dependent fixed mines will be used as sparingly as is consistent in harbor defense, and their proper places supplied by the cheap self-acting mines, or possibly by the still cheaper dummy.

I now proceed to the study of section (2)—

Systems of dependent electrical fixed mines.—Of these, under certain circumstances, four different species have hitherto been employed, namely, the "ground" and "buoyant" fixed mines, and these combined with their circuit closers in separate buoyant cases.

The advantages claimed for any *ground* system of fixed mines are—the practicability of employing heavy charges, no liability to damage by the contact of passing vessels, and their certainty of position always secured.

Now, though none of these advantages

may be possessed by the buoyant system of fixed mines—unless it be the latter one—yet, owing to the uncertain and complicated operations attendant on the work of exploding a ground fixed mine at the exact instant of the hostile ship being within its destructive area, I prefer to depend entirely on the buoyant system for the protection of harbors by submarine defense; and I consider it most unadvisable and generally useless to employ the ground system of fixed mines. In the case of a mine charged with 500 lbs. of gun-cotton and moored at its most effective depth, the radius of its destructive effect is, even with that large charge, only some 25 feet.

In the event of unforeseen circumstances rendering it impossible to use the system of buoyant mines, then I would rather trust to moral effect and self-acting mines than go to the trouble and expense of laying down a system of ground mines, necessitating the employment of any known method of observation firing, which at best can only be utilized at certain times.

I use the expression "known," for Captain Watkins, in his lecture on "Rangefinders," at this Institution, mentioned the invention of a position-finder, which he stated was capable of automatically connecting up and exploding ground mines within less than 9 feet of vessels moving at a speed of from 8 to 9 knots per hour. But, notwithstanding this invention, I cannot see any necessity that could arise where aught but the buoyant system need ever be employed, and thus I propose to do away entirely with the ground system of dependent electrical fixed mines, and also with those combinations of mines and separate circuit closer cases, and by so doing to adhere to one of the fundamental principles of torpedo warfare in all its branches, viz., *simplicity*, a principle, I venture to say, far too often overlooked.

My preference for the sole adoption of simple buoyant fixed mines in any system of submarine defense by dependent mines is based on the following grounds:

I consider *firing by contact* to be the only truly practical and effective mode of carrying out submarine warfare, possessing, as it does, the needful elements of success for the destruction of a vessel, namely, the delivery of the whole force of the explosion, at the right instant, and in the weakest part; and, therefore, I regard buoyant fixed mines as wholly and solely contact-fired mines. Then, following out this reasoning, I fix the maximum charge of such mines at 100 lbs. of gun-cotton or other explosive compound, which amount is some 40 lbs. in excess of the charge considered necessary for the contact-fired Woolwich fish torpedo.

By thus limiting the charge and making the mines contact-fired ones, I obtain for the buoyant system the following advantages: greater certainty of destructive effect; less expense of material; and simplicity, in placing in position, as it is unnecessary to exactly plant each mine in the actual position assigned to it, as would be the case where any system of ground dependent electrical fixed mines is employed.

A buoyant mine such as I have described is shown in Fig. 2.

IMPROVED
BUOYANT ELECTRIC CONTACT MINE.

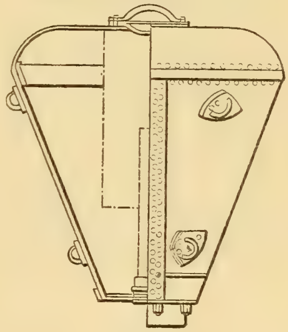


Fig. 2

Then comes the question of "mooring" buoyant mines. This has hitherto been considered a matter of great difficulty, and no doubt, under certain exceptional circumstances, would prove a troublesome business. Most of the difficulties hitherto present in effectively mooring such a mine have been to a considerable extent overcome by the employment of a steel wire mooring rope, which prevents it spinning around, and which would be found strong and pliant enough to ensure its being retained in its proper position under ordinary circumstances, the weight of the anchor having been carefully calculated according to the position

of the mines, depth of water, flow of tide, etc.

In exceptional cases a mode of mooring such as is shown at Fig. 3 might be adopted with advantage.

There still remains to be devised some practical method of retaining a buoyant mine at its normal depth in places where the rise and fall of tide is very great. Mooring groups of mines at different depths, increasing with their distance from the base of operations, rectifies to some extent this fault, but necessitates a considerable increase in the number of mines, and therefore in the cost of the defense.

Another point in connection with the use of buoyant mines is the necessity of constructing them in such manner that they be rendered impervious to the constant ramming they would most probably re-

DEEP WATER MOORINGS.

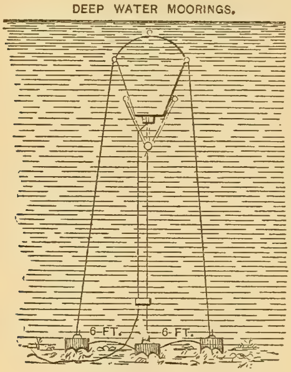


Fig. 3

ceive from the passage of friendly vessels if planted at the entrance of an important harbor; and the chances of accidents occurring from this cause must be carefully guarded against by forming them of watertight compartments filled with cork, or some other method of rendering them unsinkable.

Having thus briefly discussed the various items of submarine defense, I will now treat the subject in its entirety; and to enable me to do this clearly, and, I hope, effectively, I have prepared rough plans of a submarine defense of a river entrance and open seaport, both of which places will be probably recognized and familiar to many of my audience. I have preferred this method to the one usually adopted of creating a plan to suit the particular system of submarine defense under discussion.

Fig. 4 represents the plan of a system of submarine defense for what I term a "close" harbor, while Fig. 5 shows similarly the defense of what I term an open harbor. The former is, of course, more adapted to such a mode of defense than the latter one.

I will first take the case of the close harbor, Fig. 4. To the North is situated a large island named Isle North, which it is impossible to circumvent by any but very light draught boats, and therefore very simply secured from rear attack. Flanking it is an extensive mud flat. To the West is another large island, Isle West, also secure from rear attack. Between these two islands flows a deep-water main channel, which is divided into East and West channels by the center shoal and S′ islands. The East channel ranges from three-quarters to a mile in breadth, the West channel from a quarter to three-quarters of a mile. Between the Southwest mud flat and Isle West flows a shallow boat-channel about a mile in breadth at its widest part, but narrowing and shallowing considerably as it runs to the North. The average depth in the East channel is about 40 feet—ranging from 5 to 8 fathoms; in the West channel the mean depth is about 33 feet, whilst the depth of the shoal water to the Southwest is from 13 to 3 feet, and on the center shoal it is some 10 feet. The rise and fall of tide is about 4 feet, and flows at the rate of from 2 to 4 knots per hour. The nature of the bottom is mud.

In planning the submarine defense of any harbor the points to be taken into consideration are—the object and importance of such defense, the assistance afforded by its natural formation and land defenses, and the question of expense. And, going further into details, there would have to be considered—the number, position, formation, and class of the dependent electrical and self-acting fixed mines; also the question of the employment of locomotive submarine torpedo-boats.

In the particular case under review, the river, of which a sketch of the entrance is shown, leads to a most important town, possessing an arsenal and dockyard; and therefore the object of

Fig. 4

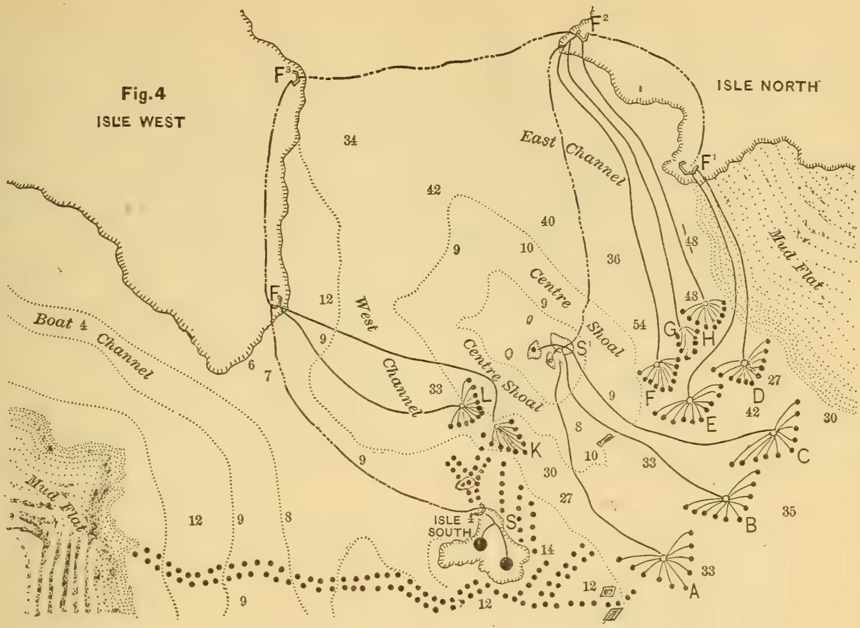
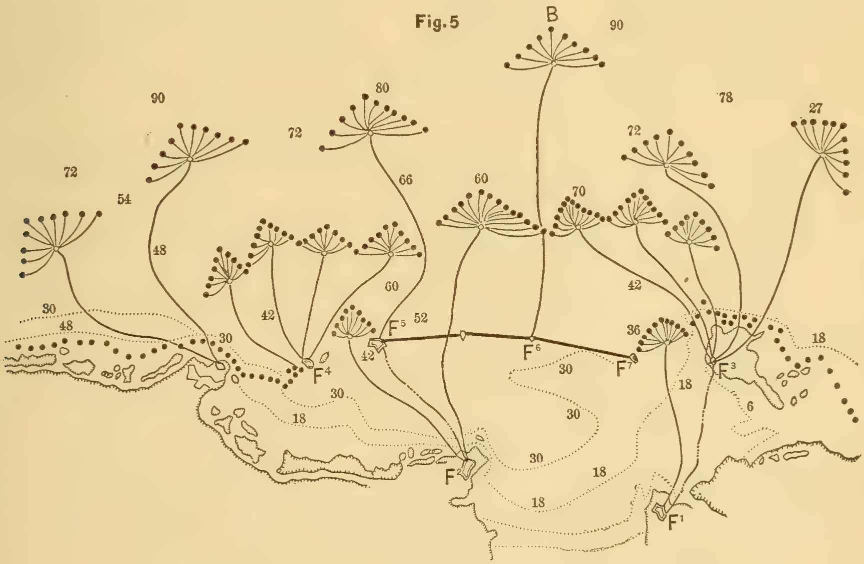


Fig. 5



its defenses, both land and submarine, would be the complete prevention of capture or of an effective blockade being established. Its natural formation lends great assistance to a work of defense, as the plan shows; and this is still further strengthened by the fact that a similar formation of narrow channels and high land is found about 10 miles further up the river, so that really the defense here treated of would only be considered as an advanced work.

First, as to the "fixed" mines themselves. The system of manipulating the dependent electrical fixed mines in this case, and also in that of the open harbor,

would be the "McEvoy's single main system," Fig. 7; and, taking into consideration the depth of water, rise and flow of tide, "buoyant" fixed mines of maximum power would be used, that is, those containing a charge of 100 lbs. of some explosive compound. The "self-acting" fixed mines would be McEvoy's improved Singer mine, containing only 50-lb. charges; and single moorings would be employed for both the dependeut electrical and self-acting fixed mines.

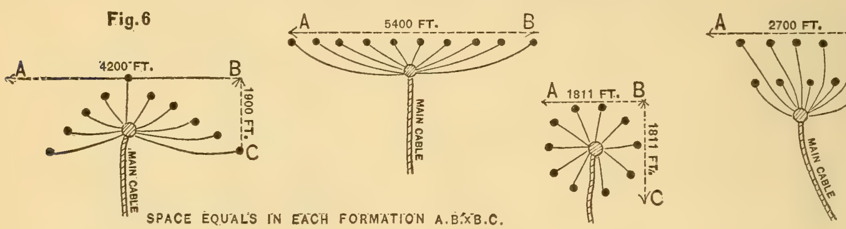
Then as to their formation. By this I mean the shape or form in which the mines composing each group should be planted so as to reap the greatest advantages, such as—spreading over the largest space with greatest probability of success; simplicity in planting them and of laying

For simplicity in planting, &c., the single line is of course the simplest, but next to it comes the triangle form.

Then as to the chances of discovery. Two or three mines of a triangle-shaped group being picked up by dragging would not afford as sure an indication of the position of the remainder as would be the case were either of the other formations resorted to.

Then as regards the number and position of fixed mines. One of the principles of submarine defense is, the prevention of serious damage to the first line of fortifications by the gun-fire of the enemy, and the forcing and retaining as long as possible under a powerful cross-fire the attacking vessels when forcing an entrance. To effect this the outer groups

GROUP FORMATIONS FOR FIXED SUBMARINE MINES



SPACE EQUALS IN EACH FORMATION A.B.×B.C.

out their branch cables; less chance of discovery by dragging on the part of the enemy. Now, considering carefully these points, I have preferred to adopt in the place of either circular, single line, or échelon, a triangular formation, whereby I contend the foregoing advantages are obtained in a high degree. In Fig. 6 I have compared these four methods. The mines here are supposed to be planted at a distance of 600 feet apart, the two lines in the échelon case being 900 feet distant, which is the greatest space allowable. Then calculating the spreading area of each formation to be its length multiplied by its depth, I roughly get, taking s as the area for the triangle formation $\frac{1}{2}$ s for the circular area, $\frac{1}{3}$ s for the échelon area, and $\frac{1}{1000}$ s for the single line area.

Then for the probability of success. A ship steaming on to a group of mines of the triangle formation would be almost sure to strike one or other of them, which would not be so certain in either of the other formations, unless it be the circular one.

are planted as shown at A, B, and C, in Fig. 4. It would have been still more effective had I chosen to fortify Isle South, and consider that as the first line of land defense; but this would necessitate a great increase in the material, and consequently in the expense of the defense, so I have only treated this isle as a look-out station, and as a place capable of affording a refuge for the guard vessels and torpedo-boats of the defense. The station itself at S would be a bombproof, covered earthwork, carrying a few light guns, electric lights, &c., for the purposes of guarding and watching the outer groups of mines. And station S would be connected with fort F by the submarine telegraph cable C′, and by this cable its mines would also be fired when requisite, to render the place useless to the enemy.

Next comes the question of the defense of the different channels. The east channel, I calculate, would require at least 50 dependent electrical fixed mines, those in each group being placed 300 feet

apart. The west channel might of course be effectually and simply blocked by planting it with self-acting fixed mines, but I have preferred to utilize this passage as a mode of egress and ingress for the vessels and boats of the defense whilst carrying out their special duties. The boat channel is rendered inaccessible by self-acting fixed mines being laid down in the shoal water to the south-west. A boom obstruction, combined with some few self-acting mines, would be prepared in readiness to further block the channel at its narrowest part, on the outer defenses being forced.

The main and telegraph submarine cables, of which there are thirteen, are so laid that no crossing of them occurs, and with due regard to the economy of the defense.

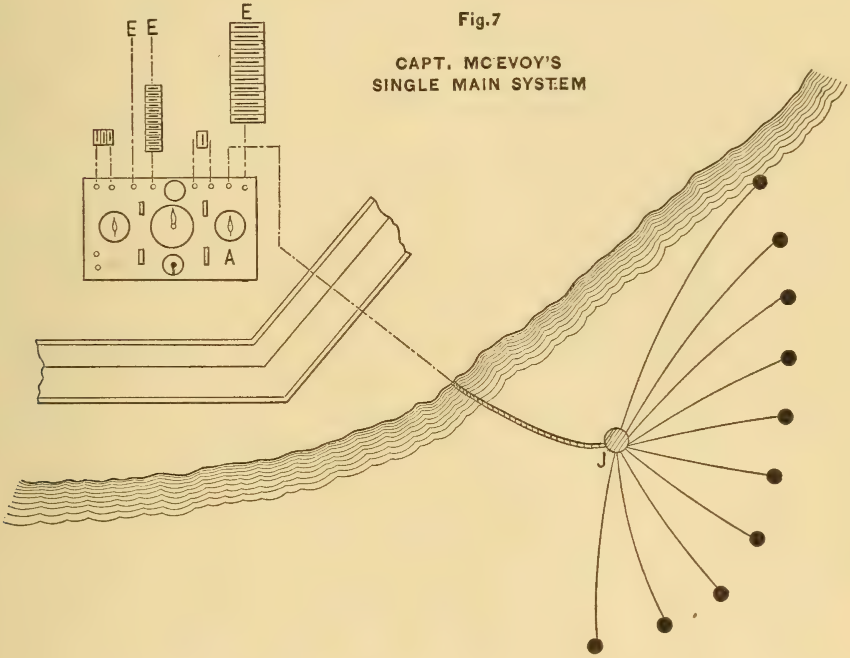
The shoal water round about Isle South is planted with self-acting fixed mines. These might with advantage be to a great extent supplanted by the dependent electrical system of fixed mines with 50-lb. charges, and controlled from station S; but such would no doubt be considered too expensive.

Then we come to the subject of subaqueous batteries for the use of locomotive submarine torpedo-boats.

One battery might be placed to the south of Isle South, as at L, and its torpedoes manipulated from station S; another one, L′, off the work at S′, and controlled therefrom. The Germans are now experimenting with sunken batteries, containing six Whitehead uncontrollable torpedo-boats, starting them by some application of electricity; but the practical knowledge as regards the best mode of operating these subaqueous weapons has yet to be acquired, so I will not venture to further discuss them.

Fig. 7

CAPT. MC EVOY'S SINGLE MAIN SYSTEM



Having thus somewhat meagrely gone into the matter of the submarine defense of what I term a "close" harbor, I will now proceed to discuss the question of a similar mode of defense in the case of an "open" harbor, a rough sketch of which is shown in Fig. 5.

The actual harbor is comprised between forts F^4 and F, and is formed by means of a breakwater; the depth of water off the breakwater entrances, and at the outer (B) group of mines, ranges from 6 to 13 fathoms. The 5, 3, and 1 fathom lines are shown by the dotted lines. The

strength of the current varies from 2 to 4 knots, and in this case a considerable rise and fall of water is met with, namely, some 20 feet at spring, and 13 feet at neap tide.

The nature of the dependent electrical fixed mines used would be buoyant ones of maximum power, and they would be specially moored somewhat after the manner shown in Fig. 3. The self-acting fixed mines would be similar to those used in the defense of the close harbor.

The distance extended by the outer line of submarine defense is upwards of 6 miles, and the groups of mines composing it are laid at some two miles' distance from the first line of fortifications. By the disposition of the dependent fixed mines, as shown in my plan, I hope to obtain what is the *sine quâ non* of such work, namely,—a maximum power of defense with a minimum of cost and of complication; and I cannot believe the capture of any harbor known to be so strongly fortified and supported by so powerful a submarine defense would ever be attempted, and thus the grand object of such a combination would be obtained, which is, the rendering of a port actually impregnable. The bombardment of the town and arsenals would have to be performed at a distance of 3 miles, and from seawards.

The dependent mines in this case are all planted as it were in the open sea, and thus any attempts made to damage or destroy them must be depended on the state of the weather, and so would not be of so frequent occurrence, or stand so much chance of success, as would be the case in connection with a close harbor, where such attempts would possess an important element of success, namely, "smooth water."

The coast about this port is an extremely rocky and dangerous one, and therefore would not require a large number of self-acting mines as a flanking defense; also the fact of their having to be moored on a sea coast would militate to some extent against their use, owing to the possibility of some of the mines breaking adrift in heavy weather. I have shown in the plan a few of these mines planted along the coast, but the actual position and number of them would have to be determined by those more thoroughly acquainted with the peculiar exigencies of the situation.

This will conclude my necessarily very few brief remarks on the subject of a submarine defense of a close and open harbor, such as I have exemplified, and the only excuse I can offer for such a concise treatment of an important subject is the vastness of the field I have attempted to cover in one short paper.

Until within the last few years, and even I believe at the present time, torpedo warfare has been looked on as something uncanny, and too terrible. Now, the primary object of submarine defense is the safeguard of a harbor against the attack of a hostile fleet, and I maintain, that in the event of an enemy's ship being sunk in any attempt to force a harbor so protected, those responsible for such service being carried out should be held accountable for any loss of life ensuing from such a catastrophe, and not those responsible for such defensive means being resorted to. As a rule, it will be generally known whether any particular harbor is studded with fixed submarine mines or not; and should the slightest doubt whatever exist as to that fact, it would in my opinion be a most foolhardy and reprehensible act on the part of any one to attempt to enter such a port.

The popular delusion as to the resultant effect of an explosion of a submarine mine in contact with a vessel-of-war still, I believe, exists, which is, that the ship is blown into the air into small pieces, with the total loss of her crew, while the actual effect with ships as at present constructed would be at the most the gradual sinking of the ship caused by a large hole being made in her double bottom, and with but few casualities to the crew, they having ample time to get clear off in her boats.

Surely this cannot be considered as uncanny, nor yet one iota as terrible as those constant and dreadful bombardments between forts and ships usually ending so seriously for the crews, which have hitherto been so common a feature of maritime warfare; but which the general adoption of submarine defense will to a great extent lessen, and thus the uncanny and too terrible torpedo may under such circumstances be looked upon as really a *life-saving* weapon.

Notwithstanding the imperfection of the systems of submarine defense used during the American Civil War, yet even then we find Admiral Dahlgreen writing: "I believe torpedoes to constitute the most formidable of the difficulties in the way to Charleston." But though some twenty years have elapsed since those words were written, and during this period submarine defensive material has been vastly and generally improved, and that in actual war submarine defense has been proved to be of immense value, yet we find Major Parnell writing: "Submarine mines do not, however, form material obstacles, and it is probable that their value is chiefly of a moral nature."

And yet another officer, Captain Barrington in his work entitled "England on the Defensive," though the author to some extent appreciates the value of submarine defense, says: "But torpedoes can only destroy or cripple the first line of attack, and when they have acted, the enemy can advance without further hindrance." Also Commander Barber, U. S. N., in an article on the progress of torpedo warfare, though he looks on fixed submarine mines as of immense utility as auxiliaries to other systems of defense, yet distrusts them to a great extent, on the belief that countermining and cable cutting will often prove successful; and also the general tenor of the remarks made *á propos* of the employment of torpedoes in the discussions here and elsewhere, is to the effect that torpedo warfare cannot yet be safely relied on, and therefore need not be seriously considered in matters of attack and defense. Now all this is, I venture to say, a great and radical misconception of the real value of submarine warfare, which should most certainly be eradicated at once from the minds of all naval and military men —especially of ours—who will, in the event of war, find the at present slighted and ignored torpedo of infinite value for defensive purposes, and I fear a terrible bugbear in all naval operations.

In the event of an organized invasion of England, which to judge from the numerous articles and letters published of late from the pens of some of the ablest of our naval and military officers, would seem to be looming in the immediate future, we should have to depend for the safeguard of our numerous harbors, ports, inlets, &c., (which number, I believe, some 280), almost entirely on submarine defense—at any rate at the outset—that being the cheapest and most ready to hand of any defensive means; and this applies not only to England, but to all countries possessing any extent of sea coast.

One of the principal objects of an invading force would be an immediate descent on some known unprotected part of our coast, and there effect a landing; but if all the numerous places that must exist in a more or less unprotected state, when land defenses and the presence of a fleet only are relied upon, possess a means of submarine defense, and a trained body of men to manipulate it, then the enemy would first have to wholly or partially destroy this submarine defense, before proceeding to the work of landing, thus occasioning at least a considerable delay, and affording time for our ships to arrive, with the probable result of the failure of the invading force at that particular attempt, and possibly the utter collapse of the expedition.

I trust no misconception may arise from anything I have said as to the value and importance of our Navy; but what I have intended to express is the absolute necessity of submarine defense to enable our splendid Navy being fully utilized, and affording it full opportunity to carry out the multifarious and onerous duties assigned to our ships at the present day, and this with perfect immunity to us from invasion.

Then, again, the vast economy of submarine defense entitles it, on that ground alone, to far greater importance and attention being paid to it than is usually the case; and, as a proof of this economy, I will mention that employing the "McEvoy single main system," Fig. 7, for the dependent electrical mines, a sum of £1,500,000 would allow of a submarine defense of each of those 280 vulnerable inlets on our coasts, consisting of one-half of the whole material considered necessary for the defense of the close harbor. Now this quantity may I think be taken as affording an adequate mean submarine defense for those 280 inlets.

In the method I have adopted of only employing small buoyant fixed mines in a system of submarine defense, I do not see any necessity for complicating such a

system by the addition of special fixed mines, or other means for the prevention of interference with the real defense by the enemy's boats; for if, whilst attempting to damage the defensive fixed mines, one of them be struck and short circuited, I should most decidedly not hesitate to fire that particular one, with the probable effect of destroying the boats carrying out this work of destruction, or at any rate deterring a repetition of similar work. Of course by so doing it may be said that a mine is wasted, and a gap caused in the line of defense; but by the employment of the single main system and small buoyant mines I consider it quite feasible and practicable to reinforce the submarine defense at any time, even in the presence of an enemy; provided that everything requisite for the accomplishment of such has been carefully and thoroughly prepared beforehand, and the submarine defense corps have been practically trained for this special service. This brings me to the question of the organization of a submarine defense corps. With us the work is undertaken by the Royal Engineer Corps, but all the principal Continental Powers intrust the carrying out of this work to the naval Service.

Now the whole defense of any harbor must be a homogeneous work, if it be ever expected to successfully stand the crucial test of actual war, and therefore the old saying, "that those who plan the forts should also plan the torpedo defenses," must be considered as only partially correct, for the resultant plan of defense of any port should be the outcome of a joint committee of naval, artillery, and engineer officers; and the actual execution of the submarine portion of the defense should be placed entirely in the hands of the naval department; and this, I venture to say, must appear self-evident, when it is duly considered what the details of such work consists of; and how infinitely more adapted for lighter, boat, and submarine work those are who, as it may be said, are born to it, than men of the shore, as to all intents and purposes military men are. I trust it may not be conceived that I undertake the value and efficiency of the Royal Engineers, than which a finer or more important corps does not exist; but I do think that the work of the sea should be performed by seamen.

Then coming to the subject of the coast defense of the British Empire—a subject of vital import, involving as it does our immunity from invasion, the power of upholding our foreign prestige, and thus the preservation of peace to the Empire now and always,—submarine defense, if carried out in a thoroughly practical and real manner, and suiting the different means adopted to the peculiar exigencies of each particular place, will assuredly prove of immense benefit by enabling us to utilize to the full the power of our Navy. Of course, the principal naval ports, such as Portsmouth, Plymouth, &c., and strategic points such as Gibraltar, Malta, Hong Kong, and others, are, or will soon be, amply provided with a means of submarine defense; but it is to the innumerable commercial ports, harbors of refuge, coaling ports, inlets, and open beaches which are to be found around our home and colonial coasts, many of which it might be absolutely requisite to securely protect, that I more particularly refer here, and in the safeguard of which submarine defense would prove of paramount importance. Captain Colomb in his Prize Essay of 1878 says—

"If the enemy is to be kept at bay by a home defense of any kind, I should think that a cordon of coast torpedoes, fixed and locomotive, in the hands of a British volunteer force will do it," in which I most cordially agree with him.

A volunteer torpedo defense corps should consist principally of the seafaring population of the different ports—men thoroughly accustomed to boat work, and possessing an accurate knowledge of the characteristics of the particular place to which they belong—and thus essentially adapted to the performance of the work of submarine defense. Little or no training would be needed for the greater portion of the force, as these men would have only the comparatively simple work of laying out the main and branch cables and planting the dependent electrical and self-acting mechanical mines. Then, having ascertained the number of men at each port it is determined to defend that would be capable and available for such service, a muster of them from time

to time would suffice to secure their attendance when called on. The number, condition, and power of the steam tugs, launches, and lighters available at the different ports for the work of laying down the systems of submarine defense and guarding the same would, of course, have also to be ascertained and considered. At each port a few specially instructed members of the force would be needed for the work of manipulating and fitting up the system; no highly scientific training would be needed, but actual practical knowledge of the mode of working the instruments and batteries of the particular system that it is decided to adopt would have to be thoroughly taught. The plan and mode of defense should be determined on beforehand, and all the material prepared in readiness for carrying out the same. The foregoing no doubt seems a very offhand and rough scheme of organization of a volunteer torpedo corps, but with the experience I have gained from carrying out similar work with Turkish and Chinese sailors who had had no previous training whatever, and not one of whom had ever heard of, much less seen, a torpedo, and whose language I was ignorant of, I venture to say that a scheme based on those lines would prove a practical and efficient one.

The submarine defense I would propose to adopt with such a scheme would be the simple buoyant dependent electrical fixed mine, in connection with a simple method of single main cable or rheotome system, and the McEvoy improved self-acting mechanical fixed mines. Locomotive submarine torpedo-boats, if used, should be entrusted to the regular force, and I believe a naval volunteer torpedo defense corps, carefully, and above all practically, matured, would—as an auxiliary—be to the Navy what the Volunteers are to the Army—than which no higher praise could be accorded it.

This concludes my paper, which, though it be terribly incomplete and far too concise, yet may, I hope, prove of some value in raising a discussion on the many points I have been forced to leave untouched, or only cursorily glanced at, in which, I hope, those gentlemen far more able and capable of dealing with them may join and give us the benefit of their vast knowledge and experience. I have only to beg to offer you, Admiral Horton, my most grateful thanks for your kindness in presiding, and to express the deep obligation I feel I owe to my audience for the patient endurance displayed in listening to what has unavoidably been an incomplete and dry paper, and one full of dogmatic opinions. As a result I can only hope that some of you may become imbued with the same strong opinion as myself as to the essential value and power of submarine defense.

I now proceed to describe Captain McEvoy's inventions.

I will first explain the mode of working the invention known as "The McEvoy Single Main System."

The title is intended to convey a system of submarine defense by which a number of mines are controlled and fired through a main cable of any length, but containing only one core or path for the electric current to traverse, which current enables all the various modes of firing and tests connected with any system of submarine mines to be accomplished. Hitherto, for this work, it has been necessary to use a separate core or path for each mine.

The principal object of this invention is the combination of simplicity of manipulation with great economy.

The single main and multiple main systems are shown and compared where Fig. 7 represents the former, A being the shore instrument and J the junction box, inside of which is placed the sea or rheotome instrument; and Fig. 8 represents the latter, T being the test-table, S the shutter arrangement, and J′ the junction box.

The advantage of the former method over the latter in point of simplicity and paucity of connections is obvious from the plate, being as 1 to 2; and this advantage is still more patent when we take the case of the connections in a torpedo-room from which a number of mines —say fifty—are controlled. In this instance, using the McEvoy system, there would be only 5 main cable and 35 other connections, giving a total of 40. While using the multiple cable system there would be 7 main cables, necessitating 49 connections, which, with the 63 other wires, makes a total of 112, or as 3 to 1 in favor of the McEvoy system.

Then another advantageous feature of this method is that due to the lightness per mile of its main cable, weighing as it does, one-third less than the multiple main cable per mile, thus making the laying out of such a work far more handy, requiring less time, and less expense; further, the gain in economy by the use of the McEvoy system owing to the extreme comparative cheapness, namely, as as 1 to 4, of its main cable, which constitutes the chief item of expense in submarine defense, is yet another very important feature to be considered; and in the matter of the foregoing points of vantage on the side of the McEvoy invention, no one, however biased or prejudiced, can take exception thereto.

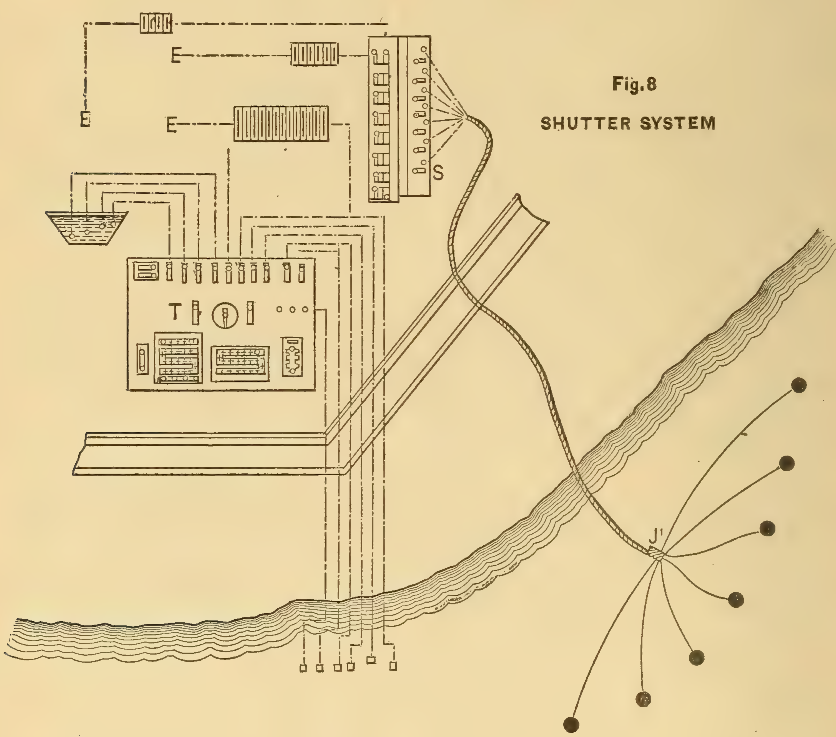


Fig. 8
SHUTTER SYSTEM

Before explaining the mode of manipulating this invention, it will be necessary to state, in general terms, the requirements of a perfect single main cable system:

1st. Its manipulation must be simple and practical, and not requiring the services of a skilled electrician.

2d. It must be certain in all its actions.

3d. It must be capable of firing either at will or by contact.

4th. It must be able to denote the electrical state of each mine by comprehensible and easy tests at any time.

5th. It must afford certain indications of any severing or damage of either main or branch cables.

6th. It must denote the number of any mine exploded or struck.

7th. It must afford indication of the sinking of any mine.

8th. There must be no possibility of a switch or key being left in its wrong position.

[*Note.*—Then followed a general explanation of the system.]

Having thus shown you the capabilities of this invention, I will mention, and I hope overrule, the few objections that might be preferred against it. The sea instrument is of course the main objective point:

1st. It may be thought to be too deli-

cate an instrument to be placed under water, and in a position difficult of access, and on which the working of ten submarine mines depends.

Twenty or even ten years ago, these objections might have been made with some show of reason; but nowadays, with the practical knowledge that we have of the satisfactory working of intricate pieces of mechanism under an excessive strain—such as telegraph and other instruments—no one would think of entertaining any objection to the adoption of an electrical instrument solely on account of its apparent delicacy of construction. I say apparent, for it can be only so to those who are unaccustomed to manipulate such instruments. Now, this sea or rheotome instrument of Captain McEvoy's, which is a very much simplified adaptation of the Wheatstone A B C telegraph instrument, would, on actual service, have to stand comparatively little work—probably not more than four series of tests each day. This instrument, and a similar one I had with me in China, have been often subjected in one day to far more work than would be the case with one of them on actual service in the course of a month's ordinary work; and yet we have never had occasion to remove the lid of the instrument case, for repairs or other matters, though this and the one used in China have been hard at work for many months.

Next, as to its being placed under water. This is, I venture to say, a very simple matter, involving merely the construction of two separate brass boxes, or more if it be considered necessary, one inside the other, both watertight, and capable of resisting a pressure of water, varying according to the depth at which it would beplaced, a work, with the present perfection of all mechanical details, easily and practically attainable.

2d. It might be thought that owing to electrical leakage the balance may not work satisfactorily, and therefore uncertain tests afforded, after a group of submarine mines has been planted for any considerable length of time. Now, this fault caused by leakage, which I have greatly exaggerated, would be easily remedied by merely increasing the power of the signal battery, by the addition of a cell or two: for the failure of the signal current, which is supposed to be kept at a normal strength, to correctly swing the test needle, is due to its current, or a portion of it short circuiting through the points of leakage.

This completes the explanatian of this clever invention, which has been adopted by several foreign governments, and I trust I have most favorably impressed you with its power and practical usefulness for submarine defense; any way, I believe you will concur with me in declaring that it deserves a fair and honest trial.

Captain McEvoy supplied one of his original sets of these instruments to the Chatham torpedo authorities in 1879, but so far, he has not had the satisfaction of receiving an expression of opinion as to the merits or demerits of his invention; the Chatham set does not embrace all the late improvements.

This particular set of Captain McEvoy's system is adapted for a group of 10 fixed mines. Now, it has been often urged that this is too great a number of mines to be manipulated by one set of instruments and dependent on one main cable, and that also a far too large gap would be formed by the loss of one such group through the discovery of its main cable by the enemy.

Take the case of the close harbor in Fig. 4—here 100 dependent fixed mines are estimated for its defense, necessitating 10 sets of instruments, and 10 main cables. If only 7 mines compose each group, then to obtain a similar defense there would be required 14 sets of instruments and 14 main cables; and in the case of 5 mine groups, 20 sets of instruments and 20 main cables would have to be employed, and I do not think the exponents of the reduced group system would consider the advantages claimed for it to be attained by the use of a greater number than 5 mines for each group: then by substituting the reduced or 5 mine group system for the 10 mine system, it is evident that far greater time is required for the work of laying out the defense, the cost is nearly double, and the simplicity of the work much impaired by the increased number of main cables and instruments; also the chances in favor of a main cable being discovered by the enemy are exactly doubled, therefore, weighing carefully the advantages and disadvantages of the two systems, I

think it will be generally agreed that the 10 mine group system is by far the preferable one

In conclusion, I will proceed to explain the very latest of Captain McEvoy's inventions. It is termed a "submarine detector," Fig. 9. It is constructed on

MC EVOY'S SUBMARINE DETECTOR.

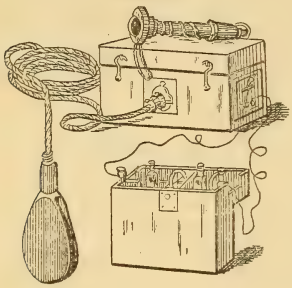


Fig. 9.

the principle of the induction balance, and consists of an instrument box, small battery (which may be either a magneto or ordinary chemical battery), and the detector with its cable and box; this detector cable is composed of four insulated cores, and by an ingenious contrivance, the connection of these four cores can be made or broken by one movement. Within the instrument box is placed a set of induction coils, and a vibratory magneto-electric apparatus, the telephone in connection with which can also be laid in the box; the battery poles are always connected up, but the circuit can be broken or closed at will, by means of a small key; within the detector itself is placed a set of induction coils. The action of this invention is as follows: the circuit being closed, and the telephone placed to the ear, a very distinct and regular increase of a humming sound will be heard on the near aproach of the detector to any piece of metal however small, culminating on actual contact therewith. This invention, as may be easily understood, will prove of great value in an infinite variety of ways; as specially connected with submarine defense, it will be found of great use in discovering the position of submarine fixed mines—both dependent electrical and self-acting—also junction boxes, cables, etc., when it is required to cover the same.

THE DEVELOPMENT OF ELECTRIC LIGHTING.

By J. E. H. GORDON, B. A.

From the "Journal of the Society of Arts."

Among all the great industries which in this wonderful last fifty years have been developed, there is none, perhaps, that has had so stormy a birth as the industry which we have met to speak of to-night, namely, that of electric lighting. We must remember that the public know very little indeed about electricity, in fact, nothing. Even in the early days of railways, although there was much ignorance about steam, still everybody had seen a kettle boiling, and had some vague idea of what steam might be, even if he did not know what it could do. But the public do not know what electricity is, nor have they any knowledge of its laws; therefore, at one time, they were ready to believe any statements made to them about electricity. Nothing was too extravagant, too startling, or too ridiculous for the public to believe in the early days of electrical enterprise. We all remember that panic in gas shares, which occurred about three years ago, when numbers of the public rushed to their brokers, and directed them to sell their gas shares at any sacrifice, to throw them away if there was any liability on them even, but to get rid of them. Why? Because a telegram had come across the Atlantic that Mr. Edison said he had made a discovery which would supersede gas. Few remember that, even with the most perfect discovery, it would at least take a little time to provide for an industry involving a re-investment of capital in England alone of about £100,000,000 sterling, for that is about the amount invested in gas. It was imagined that this great change was coming instantly. How-

ever, this change did not come instantly. Then they said Mr. Edison was an impostor, and that the electric light was a failure, because it was found that we could not undertake to supply one hundred lights, when the whole plant was made for the supply of one hundred lights only, at the same rate as one hundred gas lights, when they form part of a system supplying, perhaps, one million.

You must not for a moment think that Mr. Edison was to blame for this. Mr. Edison is a genius, and one of the greatest inventors of the age, and he has, in a very high degree, that poetic and prophetic insight which is able to see beyond the trammels of difficulty, and to know in his own mind, for certain, what will come in the future. He saw that the thing could be done, and said so. And, further—it is, perhaps, a failing of inventors, and being an inventor myself I may say it—Mr. Edison, like all inventors, has that enthusiastic imaginative temperament which is necessary for the development of inventive skill, and possibly he may sometimes have slightly confused what he was certain shortly would be done with what had already been done.

Now, this development of electric lighting has suffered to some extent from its enemies, but of course it is understood that gas shareholders are its natural enemies, and will do all they can to retard it; it is perfectly fair and right they should do so. We regard them as fair and open enemies; we are quite willing to fight them on their own ground, and hope the best man will win. But they have not done us much harm—in fact they have done us good. If there had been no other light to fight against, electric lighting would not have developed as fast as it has done. But electric lighting has suffered terribly, not so much from its enemies as from pretended friends. It has suffered much from the operations of persons who, while professing to be interested in electric lighting, and to desire to hasten its adoption, have only been interested in the progress of Stock Exchange operations, and the formation of bubble companies.

Whenever any industry is started which is at all new, this will always occur, and the extent to which it occurs is measured to a certain degree by the credulity, and more by the ignorance, of the public. If companies are brought out in connection with a subject the public know all about, there is not much chance for the public to be deceived; but if it is a subject they know little about, they are easily deceived; and when you come to a subject like electricity, about which the public know nothing, there are special opportunities for the speculator. I hope the ground is now cleared, and that these bubble companies, having satisfied their promoters, are disappearing, and making way for honest, legitimate enterprise to go on.

Now to turn to the more practical and the more important part of our subject, we have to think what is the problem which has to be solved in the establishment of electric lighting; it is, first, to provide a perfectly steady, perfectly safe and reliable light; and what is very important still, but which is second, to provide that light cheaply. It is no use getting a light a little cheaper than another, if it is to go out or to blink. Our first consideration must be to make the light perfect; the second, following closely, but still second, must be to make it cheap. This problem must be regarded as a whole, I mean the problem of making it perfect. It has been attacked too much in detail, and the reason of that is that, for success in electric lighting, the knowledge of several branches of science is required, which are not often united in the same person; an electrician, for instance, is very often not a first-class engineer. It is no use getting a little more out of your dynamo if that dynamo will not fit the engine; if you have a certain dynamo that works conveniently with your engine, it is no use making the dynamo one or two per cent. better, if the alteration causes you to lose ten per cent. more in transmitting power to it. You must regard the engines, dynamos, mains, &c., all as one complete system, and you must remember that if anything goes wrong, the public will not make excuses for the lamps like we may make in the laboratory. They will not say it was not the dynamo's fault, but it was the engine's fault, or the boiler's fault. All that the public would be concerned with was that on this or that occasion the light went out. Therefore we must not despise anything, howener foreign it seems to our subject, if it

will help to make the system, regarded as a whole, perfect. The electrician knows very well that the principle of a dynamo is to move a coil of wire through a magnetic field, and he is very apt, when he is a pure electrician, to think that he has invented a wonderful dynamo if he gets a coil of wire that will go through a magnetic field; he says here is a magnet, and here is a coil of wire, the coil is to be thin and the magnet strong, and the coil is to be moved fast through that field. Then some outsider says, "How are you going to secure it, and attach it, and support it?" "Oh," he says, "any practical man can tell you that;" but I am afraid that when "any practical man" has found out that, he is apt to patent it and keep it to himself, and sometimes he says that what the electrician has given him is the very smallest part of the invention. Perfecting dynamos is an engineering problem, quite as much or more than an electrical problem.

We must not spend time in going into the details of electric machines, but there are one or two points I may be pardoned for alluding to. It is bad policy to cheapen your system by endeavoring to greatly cheapen the dynamo. The contract price to be paid by a company for electric lighting plant will be one sum for engines, boilers, dynamos, mains, buildings, &c., and it does not matter if the dynamos are a little cheaper, and the engines dearer, so long as the total sum remains the same. But whatever else we economize in, we must not economize in the dynamo, for this reason that the cost of the dynamo is comparatively a small fraction of the cost of the plant, but the dynamo is the very vital heart and lungs of the whole affair. If anything is wrong with the dynamo, it is no use having the most perfect engines and the most perfect lamps, the lights will go out. Therefore, the dynamos must be perfect, and any economy, any extreme cheapness that is required, must be achieved in some other part of the plant.

Now, this question brings us to a controversy which has been going on, and has been universally discussed ever since electric lighting was first heard of, and which I think will continue to go on for some time, that is, the controversy between the relative merits of high-speed and low-speed dynamos. It is necessary in a dynamo that the coil of armature wire should pass at a certain speed through the magnetic field, and that speed can be obtained in two ways; the wire is wound upon a wheel, and you can either have a small wheel which revolves many times a minute, ar a big wheel revolving few times a minute, and you get the same speed of the rim in each case. The controversy between high and low-speed dynamos means this: will you have a small wheel revolving very fast, or a big wheel revolving very slowly. The first and natural opinion is have a small wheel revolving fast, for you can make the dynamo a good deal cheaper that way; it takes up less space, and altogether seems more convenient. I think everyone, when he begins electric lighting work, approves of high-speed machines. I did, myself. at one time, very strongly; in fact, less than two years ago, I wrote an article in the *Quarterly Review*, in which, if I recollect rightly, I said the dynamos of the future "will, we believe, revolve much more rapidly than at present; their speed will only be limited by their tendency to fly to pieces." Then my friends and I set to work to build a dynamo on the principle I had laid down in the *Quarterly Review* article. We soon found that in a large high-speed machine there was a great deal of difficulty in getting the power into it at all. You had to put belts to it, in fact a number of belts and counter-shafts, and there was always a good deal of slipping. Then the belts going at high speed used to make a most terrible noise; there was a deafening roar in the dynamo-room, and the whole building used to shake. All these things were bad in themselves, but they were worse in this respect, that all these vibrations, whether of air forming sound, or of the building itself, required mechanical force to produce them. and every bit of that force was stolen from the steam-engine, and taken away from the force that ought to be used in producing light. I suppose this ought to have convinced me that high-speed machines were wrong, but it did not. We went on, we tried a machine, it worked very nicely, indeed, for an hour, and we got excellent electric results, and were very well pleased. Then certain vibrations began to get worse, and I thought it advisable to send the workmen

away. There was one safe spot where the person attending the machine could stand, from which I was able to watch what happened. The experiment was conducted in a cellar at Wharf-road, all vaulted with brickwork backed by the earth of the street, and it was so hard that when, on some occasion, we wanted to put a spike into it, we were unable to penetrate it at all. After a few hours' work the dynamo flew to pieces with a loud explosion, and I have here some of the few fragments that were left of it. Here you can see the fracture of a massive piece of iron that was caught by it; here are portions of the holders of the revolving magnets. I have not much of it here, the rest of it was picked up in a shovel. The only large piece which was found was a piece which weighed 1½ cwt., which struck the hard wall of the cellar (the distance from the dynamo being about tweuty-five feet), and cut a hole two feet in diameter and two feet deep. Another piece skimmed along the vaulted roof, and made a series of grooves in which you could put your two fists. After that experiment we thought we had had nearly enough of high-speed dynamos —in fact, we thought we had nearly enough of dynamos altogether. Here, perhaps, although it hardly belongs properly to the subject of the paper, I must ask your indulgence if I turn aside for a moment, to say a few words of tribute to those friends who, both by their moral and material support, helped me through that very difficult time. After this explosion, when it might have been fairly thought the whole attempt was at an end, my friends met, almost among the ruins of the dynamo, and in a few days they subscribed a large sum of money, and said to me, "Go on; build us another dynamo ten times as large as the first, only," they said, "make it strong enough this time." We set to work and built a second machine, and this time we went on very different lines. We made the wheel enormous, and made it run very slowly, taking good care that the whole thing was strong enough. The first machines had taken two to two and a-half years to make, the second machine took about eight months, and this was last year, and the experiments cost a great deal of money; one thousand pounds went after another, and still there was no result. Things seemed far from completion, and the money which had been handed to me to build the machine was spent three times over before the machine was finished; still my friends said, "go on, if it costs three times as much as you expected, you must make it do three times as much work." At last we were all ready to try the experiment, and for the last week or ten days I may say we had given up going to bed; it was of no use, for we could not sleep, we used to go to bed for two or three hours, but we spent nearly all night in the factory. At last my friend, Mr. Clifford, the Telegraph Construction Company's chief engineer, and myself, began the final trial; we began on a Saturday morning, and thought we should be ready to start the engines on Saturday night, we got the steam up, but first one hitch occurred, and then another, before we could start, and at last it was three o'clock on Sunday morning before we made our first trial. That failed completely, the machine did nothing; we looked over it, and saw what was possibly the cause of error, and set to work to change the connections. Now, if you are changing connections in little dynamos, it is simply a matter of unscrewing two or three screws, and changing the wires, but with this large machine, it was not so simple; it took as many men as could cluster over it until six o'clock in the morning; then we got a little better result, and went home for a few hours' sleep. We started again early on Monday morning, and at nine o'clock on the Monday evening we were ready for the trial. The experiments were made at the Telegraph Construction Company's Works, at Greenwich, works which are about a third of a mile long, and cover fourteen acres; there are 1,200 or 1,400 lights all over the works; and as the cable department was then busy, there were about 1,000 men at work, and a little glimmering gaslight over each lathe. When I turned the valve, the whole place burst into a blaze of daylight at once, and we felt at last rewarded for that three years of anxiety we had had before.

Now, what are the chief advantages of a big machine as compared with a small one? I think the chief advantage of all is that we can dispense with belts. With a big machine we get the necessary veloc-

ity by the largeness of the wheel, and therefore, can cause it to revolve only the number of times in a minute which is not too great for the steam-engine to revolve, and can connect direct to the steam-engine. Mr Edison was the first to point out the importance of this, and his was the first very large machine which was made. He has constructed a very large dynamo, which works 1,300 of his lamps, equal to about 1,000 20-candle lamps, his being 16-candle. It has been worked extremely successfully in New York, and also in Holborn; and I will now throw on the screen a picture of it, then I will show you some of the important points, not of dynamos generally, but of a large machine as compared to a small one. You know that a dynamo machine consists of some apparatus for causing a coil of wire to revolve opposite a magnetic pole, or else magnetic poles to revolve past a coil of wire; in the one you see before you, the shaft is geared direct to the steam-engine, no belting at all being required; it is driven at the speed of 350 revolutions a minute.

I now come to a very important point, which is where the current is taken off. Of course, if a current has to be taken from a moving coil to fixed wires such as the conductors that go to the streets, there must be sliding or rubbing contact of some sort, and you have all seen in ordinary dynamos the contact brush or commutator, and there is always a little sparking. As the current gets greater, that sparking gets greater, not proportionately, but still it is greater when you get a large machine. I must say myself I think the current for 1,000 or 2,000 lights is about the biggest that can be taken off by rubbing contact, but I never heard that with this 1,300-light machine there has been any difficulty with this. Still, as a matter of prudence, I should not consider it advisable to take enormous currents through rubbing contacts. I am speaking of currents which will supply as many lamps as come from our big gas works. Therefore when we designed this machine I have been speaking of, we arranged it somewhat differently. We used an arrangement which is not new, but was never applied on a large scale before, namely, causing the magnets to revolve, and the coils to be fixed. That enables the current, however great, to be taken off without sparking at all, there being a metallic connection the whole way from the fixed coils to the mains.

Our machine has been worked up to nearly 2,000 lights, and we were able to get this off a fraction of the coils, and we have no doubt, it will give about 5,000 lights with adequate engine power. We call these 5,000 lights the baby ones; there will be much bigger ones made some day, we hope. There are 32 magnets, which revolve between fixed coils whence the current is taken off. I must be pardoned in my illustration of big machines for going so much into the details of my own, and so little into the details of the excellent machines of Mr. Edison. My excuse must be, first of all, I am not so well acquainted with the details of Mr. Edison's as with my own; and, secondly, from the natural feeling a parent has for his own child, for he always believes it to be the finest child in the world.

We now come to a requirement which is common to all large machines, namely, a method of adjusting and regulating them while they are going. You see we have to keep the pressure constant, and we have a varying demand. We put on a number of lights when we start, and then somebody turns out a number of lights; later on, somebody else turns a number on, and we have to keep the pressure absolutely constant. Now, with any machine, such as this, if a thousand lights, say, are on, and another 500 are put on, the pressure will drop, and the 1,500 lights would not be in proportion so bright as the former 1,000. Similarly, if 500 are taken off, the remaining ones would get brighter, and break the lamps; and, therefore, means for compensating for these changes have to be provided. The way in which the regulation is done is by varying the strength of the field magnets. These magnets which are fixed on the big revolving wheel, are not permanent magnets, but are magnetized by the current of a small direct current machine. At present we use a very good little machine made by Mr. Crompton. This small direct current machine is driven by a small separate steam-engine, and by varying the speed of this small steam-engine, we can vary the strength of the current in the magnets, and so we can alter the pressure just as we want it. Of

course we have to alter the pressure according to the requirements of the district, and this is the way the regulation is at present done. The picture on the screen represents the regulating room of our works at Greenwich. First of all, we have to see what pressure is right, and for that purpose we employ a photometer, an iron rod which casts two shadows on a screen, one the shadow from a candle and the other the shadow from an electric light further back, the relative distances being so adjusted that the candle power of the lamp is correct at the moment that the two shadows are equal. By shifting the position of the candle we can work to any candle power we prefer. The lamp employed being a fair sample lamp, we know that if that lamp is right all the others are right, but we have one or two connected from different parts of the factory, which can be turned on by means of switches. Now, suppose some one at the other end of the factory turns of 100 lights, when there are a large number on, that would not be perceptible in the factory; but the man here can see the change of brightness much more readily than anyone can in the open room or yard, and he at once sees that the pressure has run up a little bit. He then turns this valve a little, which partly shuts the tap, and diminishes the supply of steam to the small engine, that diminishes the speed and the strength of the magnets, and so takes the pressure down as required. Similarly, if 100 lamps are put on, he finds the brightness drops, and he has to open the valve, and make his small engine run faster. There is one very important feature which it has been necessary to put into this regulating apparatus, and that is due to the property of the human eye to accommodate itself gradually to changes of light. The pupil of the eye expands and contracts, and takes in pretty nearly the same quantity of light, if the illumination is within certain limits. Although the eye is very sensitive to sudden changes of light, yet if you make the change sufficiently slowly, you allow the pupil time to expand or contract, and the change is not noticed very well. Therefore, instead of turning this valve rapidly, it is turned by a slow screw, so that it takes at least a minute to make a change of light equal to one-candle power. The result is that, even if a considerable change has to be made, such a change is never noticed at all. At the back of the room there is a steam-pressure gauge, showing the pressure on the boiler, and an indicator showing the speed of the large engine, and two men stand here on duty, in turns, all night, to work the valve.

You may say that this is an expensive way of regulation, on account of the cost of men's wages; but what we have to consider in the commercial aspect of electric lighting is not the price of working each machine; but the price of working each lamp. It would have been ruinously expensive to have one or two men to attend to a small machine, but when their wages are distributed between 5,000 or 15,000 lamps, it becomes a very small item of expense indeed, although those same wages distributed between 500 or 1,000 would be very extravagent.

There is one small point about this machine, before we leave the technical part of the subject, which I may be pardoned for mentioning; although it is a purely scientific point, I think it is of some general interest, as showing the kind of difficulties which the designer of any new engineering work has to encounter. In our first model machine we made the same number of coils as there were magnets; that is to say, there were a series of round bobbins in that disc of the machine, and a similar number on the revolving wheel, and currents were induced in the way usual in that clsss of machine. On the first experiment we put one coil on the small experimental machine, and got a good result, but when we put on the next coil alongside, we found the mutual action of the two coils pulled down the amount of light produced nearly 50 per cent., in fact diminished the power of the machine nearly one-half. A little study of the laws of induction will show you how that occurred. It is a well-known law that a varying current, under certain circumstances, will induce a current in an opposite direction to itself. Here was a current traveling in one direction, which induced a current in the opposite direction in a neighboring coil, so that we got two opposite currents, and we only got the difference between the actual current induced by the magnet, and the back current induced by the next coil. The

practical result was, that the efficient lighting we got out of the machines was reduced to about one-half. To get over the difficulty we made a number of fixed coils, twice the number of the magnetic poles, flattened them, and squeezed them together, so that each pair of coils was acted on alternately, and between each there was a coil which was not in use at the moment; therefore, although there was a tendency to induce back currents, yet each pair of active coils being separated by an intervening coil, the action was so small as not to be perceptible, and thus the quantity of electricity we could get out of the machines was nearly doubled. We hoped we had now got the dynamo perfect, and that we had solved the problem and got to the end of our troubles. We had got the dynamo as perfect as we knew how, but we found our troubles only beginning. It is a small portion of the problem of electric lighting to get the dynamo perfect, because there are so many details to be attended to. In putting up a plant for 10,000 lights in the middle of a populous town—a town of wealthy houses, where the inhabitants do not at all like smoke and waste steam — there are a great many matters to be attended to. There is the dynamo house with three dynamos, each working five thousand lights, two to be always at work and and one in reserve, the whole plant being designed for 10,000. Then we have an arrangement so that the mains are laid double. That is to say, each house has half its lights from one dynamo, and half from another, so that, supposing an earthquake upset one of the dynamos and put it wrong, then only half the lights would go out; the town would not be put in darkness. There is one class of accident which may occur to any machine, and it is, I think, about the only difficulty which large dynamos are liable to, that is, getting a hot bearing. But an engine driver is not worth his wages if he cannot keep an engine with a hot bearing going for a quarter of an hour, and supposing he finds he has a hot bearing, all he has to do is to start the spare dynamo, get the speed of that the same as the speed on the other, start the exciting engines, get the pressures constant, then pull over the switch, and all that will happen will be a slight jump in the light, and then the engineer may instantly stop the heated dynamo and attend to the bearing. The engines are condensing engines, each dynamo to have two, and they will require about 510 indicated horse-power when at full work. Each engine is calculated to have a power of 275 indicated horse-power, so as to give about 550 horse-power available, which is sufficient reserve. Again, there are boilers in reserve; 4½ boilers would give sufficient power, so that there will be always two boilers standing quite idle, which can be cleaned. There is a large crane running along the roof, which we used to put the machinery into its place, and to lift any part of it as may be required for repairs.

I have spoken of condensing-engines, and I think we must have condensing-engines, at least, in all important and wealthy towns. There are certain advantages of economy which are matters quite open to discussion, but I think they are more economical even in London, where water is expensive. We have seen lots of engines for small electric light plants which are not condensing, and they do very well, but they have all been small. If you only want 10 or 20, 50 or even 100 horse-power, you may send the waste steam up the chimney, it disappears in the air, and you hear no more about it; but if you have over 1,000 horse-power, even in this station, which is regarded as a small experimental one, and send the waste steam into the air, two or three unpleasant things will happen. First, you will choke the chimney, because the effect of the steam will be to cake the soot, and so spoil the draught, and some day you would find the furnace would not draw; secondly, that steam would attract all the London blacks, and form a fog to which our present fogs would be as nothing, and that would be a bad introduction of the electric light, one of the great arguments in favor of which has been that it is to keep our houses clean. With a condensing-engine the waste steam is all condensed, and goes away in the form of water. It is a hard saying to say there must be condensing-engines, because condensing-engines very greatly increase the first cost, but in spite of this I say that we must make up our minds that, if we have the electric light at all, we must be pre-

pared to pay for condensing-engines. We must remember, too, that there is a special clause in the Electric Lighting Act, and in the draft provisional orders, pointing out that no Parliamentary powers, conferred on a corporate or other body, are in any way to be regarded as a protection to them againt being indicted for a nuisance if they should cause one. Therefore, we must arrange the plant so as not to cause a nuisance. With electricity there is more loss in transmitting it to a long distance than with gas, so that we cannot place our electric lighting machines at a great distance from the town. About half a mile radius is a convenient distance, and therefore, the stations must be tolerably central.

As to the space required, these engine-houses, for a 10,000 light plant, require 72 feet square; that is to say, two strips 72 feet by 36 feet. If it were more convenient for the site we might set the boiler-house end to end with the engine-house. Then we have to get a supply of coals. We cannot put the coals on with a shovel when working on this scale. We have to think of bringing the coals in through a populous neighborhood, storing them so as not to cause a nuisance, and feeding them so as not to cause smoke, and also so as not to have an enormous staff of firemen, whose wages would run away with our profits. We may assume at once that we must have what are called mechanical stokers to the boilers; that is to say, that each furnace is fed, not by shovelling in coals, but by a little steam apparatus, by which the coal is discharged into a hopper, and slowly screwed into the furnace. That has the advantage that it enables us to burn much cheaper coal than we could otherpise do. We have here a coal store which will hold about three weeks' winter supply, so that we need only coal about once a fortnight, and we can do it at night. The floor of this coal store is higher than the level of the boilers. The coal wagons will stop outside; there is a small crane by which the sacks are all taken up, the coal is emptied into the store until it is full, and that is the only time in which it is lifted. Near the bottom of the coal store there is a trap-door, and the coal comes out, and is run into trucks running on a raised tramway, and is discharged by its own weight into the hoppers of the stokers. There is, again, a second underground tramway for removing the ashes, so that the coal requires no handling at all.

You may say this is all very pretty, it is a very nice dream for the future, but what is it going to cost? People are not going to buy electric light at two or three times the cost of gas. That is a very important question, and he would be a very bold man who would speak too strongly on the subject, but yet I think we may say, not that electric lighting will be dearer than gas, but that it will be almost immediately cheaper. I believe that in the future it will be about two-thirds the price of gas; and I am quite certain that at present it can be supplied in London at the same price as gas, when supplied on a moderately large scale. I have worked some estimates (see next page) of the capital expenditure, and the working cost for an electric lighting plant, and there is not much difficulty in getting out the total expenditure accurately. The doubtful point is what the revenue will be. The estimates show the expenditure of capital and the annual expenses for electric lighting plant of two sizes, one for the supply of electricity to 60,000 lamps of 20 candle-power, and the other to 10,000 lamps. In each case we can always count upon putting up half as many more lamps as we should supply, for never more than two-thirds of the total number would be alight at the same instant. First comes the capital expenditure, viz., the price which would be charged by the manufacturers for the whole plant, that is, for machines, dynamos, mains, chimneys, and, in fact, bringing a pair of poles into every house, and starting the concern. Those are not estimated sums; they are taken from tenders which my company has actually furnished to corporations, so we know that the expenditure would not exceed these prices, because we have actually offered to contract to do it for the terms mentioned. The annual expenditure is another matter. It depends to a great extent on the number of hours per annum the lights are burning. I have spoken here of 2,000 hours, but I find there has been a good deal of misunderstanding about what is meant by that. I do not mean that all the lights will burn for 2,000 hours, but what I

PLANT TO SUPPLY ELECTRICITY SIMULTANEOUSLY TO FEED 60,000 LAMPS OF 20 CANDLE-POWER EACH, EQUAL TO 85,000 OF 14 CANDLES. CAPITAL EXPENDITURE, £220,000.

Annual Expenditure, lamps burning 2,000 hours per annum.

Depreciation and repairs	£8,000
Slack coal, at 11s	7,100
Water, at 6d. per 1,000 gallons	7,100
Oil, &c	850
Wages and superintendence (63 persons)	5,390
Rent, rates, and taxes	1,000
Office expenses	500
Directors' fees	1,000
Renewal of incandescent lamps	12,000
10 per cent. of dividend on capital	22,000
Total required revenue	£64,940

85,000 14-candle gas burners burn, in 2,000 hours, 850,000,000 cubic feet, which would produce £65,000 at 1s. 6¼d. per 1,000 cubic feet.

PLANT TO SUPPLY ELECTRICITY SIMULTANEOUSLY TO 10,000 LAMPS OF 20 CANDLE-POWER EACH, EQUAL TO 14,000 OF 14 CANDLES. CAPITAL EXPENDITURE, £50,000.

Annual Expenditure, lamps burning 2,000 hours per annum.

Depreciation and repairs	£1,500
Slack coal at 11s	1,230
Water, at 6d. per 1,000 gallons,	1,230
Oil, &c	150
Wages and superintendence (30 persons)	2,968
Rent, rates, and taxes	250
Office expenses	250
Directors' fees	350
Renewal of incandescent lamps	2,000
10 per cent. dividend on capital	5,000
Total required revenue	£14,928

14,000 14-candle gas burners burn, in 2,000 hours, 140,000,000 cubic feet, which would produce £15,000 at 2s. 1¾d. per 1,000 cubic feet.

Gas in London is 3s. 2d. per 1,000 feet.

N.B.—It is not expected that each lamp erected will average 2,000 hours per annum, but that 2,000 hours is the average consumption of the maximum number alight at one time. Thus, if a house has thirty lamps, but not more than eighteen alight at once, the average consumption of that house will be $18 \times 2{,}000 = 36{,}000$ lamp-hours per annum.

mean is, that the average of the maximum number of lights burning at once will be about 2,000 hours. For instance, suppose in any particular house we put up 30 lamps: there are not, as a rule, more than 18 lamps burning at once, and I should not call the expenditure of that house 30 times 2,000 hours, but only 18 times 2,000 hours. We need not provide, in plant, more than sufficient for the total number of lamps likely to be burning at once. I was talking to a gas engineer the other day, of very great experience, and he thought I might count on never having more than half the lamps alight at once, but, to be on the safe side I have taken it at two-thirds. The number of hours lamps burn depends where they are. Street lamps burn nearly 4,000 hours; then come clubs, which burn over 2,000, but in private houses they would burn less. I think, with that reservation, that when we are speaking of the maximum number alight at once, and not of the maximum number put up, we may say that 2,000 hours is not very far wrong. Here is the way the expenditure is calculated. Every year put aside something for depreciation. Of course, the first year there would be nothing spent, and perhaps not for a few years, but some day new boilers will be required, and for that we should go on the reserve fund; therefore each year a certain sum must be put aside for repairs. I have taken that at about £1,500—a certain percentage on one part of the plant, and a different percentage on another. It is a large percentage on the boilers, a smaller percentage on the dynamos, a smaller still on the mains, and a smaller still on the buildings. When the dynamos are run at a small speed, you can diminish the depreciation on dynamos and buildings; high-speed dynamos shake buildings to pieces before very long. Then comes slack coal at 11s. The cousumption of coal has not been guessed at, but is taken by me from figures furnished by Mr. Hill, chief engineer at the Royal Mint. They have lately had a pair of engines put up by Messrs. Maudsley & Field, of 500 horse-power, about the type of engine we think of using. They are not put up for electric lighting purposes, but for driving the machinery of the Mint; they have been working seven or eight months, and the figures for the consumption of coal are from the actual bills. By using mechanical stokers we can use slack coal at 11s., instead of steam coal at 18s. Mechanical stockers do not give us quite an advantage in the ratio of 11 to 18, because 1 lb. of slack coal does not produce so much steam as 1 lb. of good coal, but still there is a very great saving in using slack—about 13 to 18. The actual con-

sumption at the Mint has been $2\frac{1}{4}$ lbs. of slack coal per indicated horse-power per hour. From that 11s. that item is taken of £1,230. Then there is the water for condensing purposes. Where we get waterside premises, that will cost nothing; but in central stations we must reckon 6d. per 1,000 gallons, and a rough and ready rule is, that the water costs about the same as the coal when you have to pay for it, and, therefore, I have put it at the same price. Then oil is a small item; wages and superintendence includes the chief electrician, chief engineer, and various workmen required for small repairs. It does not include any large repairs, which are included in the first item. To be in the dark room, and attend to each engine and dynamo, requires three men always in attendance, *i. e.*, one engine-driver, one laborer, and one man in the dark room; each of those work eight hours, so that you require three shifts of men. That makes a man's work 56 hours a week, 54 being the ordinary workman's time. They work, of course, seven days a week. Then there is rent and taxes, interest, directors' fees, and renewal of lamps. These must be renewed free of expense, that is, the expense of renewing them must be included in the charge for electricity, whatever it is. I do not mean that if the glass of the lamp is broken it is to be renewed; but when a lamp has been destroyed by the current, it is to be renewed. When the lamp is broken by the current, the carbon alone breaks, not the glass, and the rule will be when the lamp has broken down, but the glass is intact, it can be exchanged without charge for a new one; but if the glass is broken, it follows it is broken by violence, and must be paid for. The total required revenue, including 10 per cent. dividend, you see, is £14,828.

The light obtained from 10,000 20-candle lamps is the same as obtained from 14,000 14-candle lamps burning 5ft. of gas an hour, and they, burning for 2,000 hours, burn 140 million cubic feet of gas, which produces £15,000, which is the required revenue, if gas is charged at 2s. $1\frac{1}{4}$d., the price in London being 3s. 2d. On the larger scale the economy is great. The larger scale we work on the cheaper per lamp will the light be. The light, of course, will have to be supplied by meter, but about that I must postpone speaking to another time.

The introduction of electric lighting has, of course, been attended with many difficulties, but I do not think it has been attended with much more difficulty than must necessarily attend the introduction of any new industry requiring a great re-investment of capital. People who have capital invested in something paying well are shy of taking it out and putting it into something new. If you go to a corporation and say, "If you will spend £100,000 in electric lighting plant you will make a good profit," they will naturally say, "That is all very well, but here is the money we are making from gas, and we prefer that;" and then they say, "Why do you not go in yourselves for this? If it is a good investment, invest your own capital in it, and show us practically it is so." I will not say that is not somewhat hard upon us, but, on the whole, it is a fair enough challenge, and we have made up our minds to accept it. I am not at liberty to say what arrangements are being made, but I hope I shall shortly be able to commence putting up in the West-end of London such a plant as we have already erected; and we made an offer to a vestry, in a district in which we propose to put up the lighting, that we will guarantee that the maximum charge for a quantity of electricity equal to 1,000 feet of gas shall never exceed 3s., and shall be reduced if we can do it. I will not enlarge any more on this, as it would savor much of a very vague scheme, for you to listen to-day to what we propose to do, and it would savor somewhat of boasting for me to say it; but if this Society would do me the honor, in about a year's time, to ask me to read a paper before them again, then, if our work turns out as I hope and expect, I may be able to tell them not what we hope to do, but what we have actually accomplished.

Shipbuilding trade on the Clyde is on the increase, and the returns show a great extension. The output from the stocks in one month was 33,202 tons, against 30,304 tons in the previous year, 21,754 tons in 1881, and 15,874 tons in 1880.

SOURCES OF ERROR IN SPIRIT LEVELING.

By J. B. JOHNSON, Member of the Engineers' Club of St. Louis.

From the "Journal of the Association of Engineering Societies."

I. General Remarks.

It is proposed in this paper to call attention to the ordinary sources of error in spirit leveling, with some effort to arrive at the relative magnitude and probability of such errors and to consider the best means of avoiding or eliminating them. It is only in the last ten or fifteen years that spirit leveling has been brought to such a degree of perfection that it would compare favorably with other geodetic operations. Astronomical determinations, and the measurement of distance on the earth's surface, both directly and by triangulation, have been far in advance of methods for determining the third co-ordinate, viz., elevations above sea level. The latter, however, are essential to a proper reduction of the former, and are becoming more necessary annually in connection with the various problems relating to the weather, the improvement of rivers, the water power of streams, drainage, navigation, railroad economy, and many scientific problems. These interests being so intimately connected with the highest development of a nation, it becomes the duty of the State to make the determination of elevations above the world's common datum plane a vital part of every general scheme of geodetic operations. For so large a country as ours, this involves lines of spirit levels thousands of miles long, and if great accuracy is not attainable, the final errors in the elevations of points in the interior are likely to be material.

Much attention has therefore been given, in the last few years, by various governments in their national surveys to this subject. Switzerland may, perhaps, be said to have taken the lead, but England, France, Germany and America have not been slow to follow her example.

Three departments of our government surveys may be credited with doing what is called precise leveling, viz., The U. S. Lake Survey, recently completed, the Coast and Geodetic Survey, and the Mississippi River Survey under the Commission. The elevation of all the great lakes was obtained with a probable error of less than one foot. The Coast Survey is carrying a line of accurate levels across the continent from Chesapeake Bay to San Francisco, passing through St. Louis. The Mississippi River Commission has a line from the Gulf as far North as Central Iowa, along the Mississippi River. This is soon to be connected with Lake Michigan at Chicago, and a check thus obtained *via* the Great Lakes to sea level at New York City. The writer has been directly connected with some 900 miles of this work and that on the Great Lakes, extending over a period of eight years. In the course of this work every conceivable source of error has been examined, its magnitude investigated, and methods of preventing or eliminating the error advanced. The result is that a remarkable degree of precision has been reached, and much valuable data as to the sources of error obtained.

Before taking up the various sources of error in leveling in detail, it is necessary to distinguish between two classes of errors, viz., compensating errors, or those which tend to balance each other, and cumulative errors, or those which always have the same sign. This distinction is very important. An apparently inappreciable error, if of the latter class, may, in the course of a hundred miles, amount to more than all the larger compensating errors combined.

Take an example: If ten settings are made to the mile, in 100 miles there will be 1,000 settings. If the mean algebraic sum of all the compensating errors for one setting be 0.01 foot the theory of probabilities indicates that the final probable error would be $\sqrt{1000} \times 0.01$ ft. $=$ 0.32 ft.

Whereas, if the mean algebraic sum of the cumulative errors for one setting be 0.001 ft., the final *actual* error from this cause is 1000×0.001 ft. $= 1.00$ ft.

It may be further remarked, that in

the prosecution of any work where a certain degree of accuracy is required, it is desirable, yes, even necessary, that the director of the work should be cognizant of the nature and importance of all sources of error down to a limit much smaller than that to which he is working. The more thorough and complete his knowledge is in this matter, the more readily and accurately will he be able to decide what sources of error may be wholly neglected, what may be provided against partially, and what must be carefully avoided or eliminated. In other words, his work is planned by an intelligent judgment, instead of by blind guessing. This larger knowledge of the sources of error will be conducive to economy of time and cost for a given degree of precision. The observer comes to see that what before had been carefully attended to at considerable expense may now be neglected, and he can rigidly proportion his pains and labor to the degree of precision required. The most successful and valuable director of any work is always he who does his work just well enough for the needs of the case, making therefore the cost directly proportional to the degree of precision and security sought.

With this preface let us proceed to consider in detail some of the most common sources of error in leveling.

II.—Discussion of Errors.

All possible sources of error in a line of levels may be classified under the following heads: 1. The observers. 2. The instruments. 3. The ground. 4. The atmosphere.

We accordingly have four general classes of errors:

1. Errors of observation. 2. Instrumental errors. 3. Errors from unstable supports. 4. Atmospheric errors.

They will be considered in this order.

1. *Errors of Observation.*—Since no observer is infallible, we may say that these errors are unavoidable, and that our only safety lies in a sure means of detecting and correcting them.

A single observation in leveling consists of two readings, reading the bubble and reading the rod. If the bubble is kept in the middle of the scale, it is no less read. If the instrument has a delicate milled head screw under one wye, the bubble may always be read in the middle, even with a very delicate bubble. If the wye adjustment is made by capstan screws, and the bubble can only be conveniently moved by the lower leveling screws, it is difficult to keep the bubble exactly in the middle. It is then advisable to bring it nearly to the center and read the two ends and correct the reading by the amount it is out. A table of bubble corrections should be provided for this purpose, for various distances of rod and readings of bubble. I believe this to be the largest source of errors of observation, that the bubble is not carefully centered, or that it is not carefully read and the correction applied. Every leveler should know what the error of rod reading is for an inaccuracy of bubble reading of 1 division. This is readily done by taking a known base, running the bubble, say 10 divisions, and noting the corresponding change of rod reading. The observer thus learns how accurately he must read his bubble for a given degree of accuracy in results.

The fact that 1 second of arc gives a taugent of 0.3 inch (0.025 foot) in a mile is a very convenient piece of information. Thus a 25 second bubble gives a movement of 0.05 ft. for a run of 1 division of 400 feet distance. This is the mean value for the two level bubbles in the level in use for students in the Washington University. Delicate levels have bubble tubes that give a run of one division for 2 or 3 seconds of arc.

If a target rod is used, errors of one foot and one-tenth are not uncommon. Such errors are less common with speaking rods. These, and in fact all other errors, are usually sought by duplicating the line. Since, however, the discrepancy in the two lines is the algebraic sum of all the errors committed, it furnishes an unsatisfactory check on any one class of errors. It is advisable, therefore, to obtain a check on each source of error independently.

The best check on errors of reading a target rod is, perhaps, for both rodman and observer to read it independently and compare notes. Let the rodman read it and make a record of it. The observer, on passing the rod, or *vice versa*, reads it also and records it in the note-book. The rodman then calls off his reading and checks.

To avoid errors in reading a speaking rod two or three horizontal wires may be used, a reading taken on each of them, and the mean used as the rod reading. This method is practiced in the levels of precision of Switzerland and it has also been used on the U. S. Lake Survey and under the Mississippi River Commission. It gives excellent results. A speaking rod is much more satisfactory for the observer, the observation is made in less time, and I believe, where the rod is properly graduated, it gives better results than a target rod. I think the spaces in the rod should not be less than 0.02 ft. in width. The precise leveling rods that I have used are graduated to centimeters, and the reading is made to millimeters by estimating the tenths of this space. Three horizontal wires are read. One centimeter is almost exactly $\frac{1}{3}$ of a tenth of a foot. If the graduation is to be in feet, very good results could be obtained by making the smallest graduation $\frac{1}{2}$ of a tenth, and then estimating tenths of this space, bringing the smallest reading to five thousandths. One should not be alarmed at this apparently large limit to our reading error. Because a target reads to thousandths is no evidence that it was set to that limit. Besides, if the reading on the speaking rod is made to the *nearest* tenth of the above space, it is true to $2\frac{1}{2}$ thousandths, and the target is not usually set within this limit.

This reading of three horizontal wires also furnishes an accurate stadia measurement of the length of sight, and thus enables the observer to keep the sum of his back sights equal to the sum of his fore sights, and thus to thoroughly eliminate all instrumental errors.

Not only so, but since these two wire intervals bear a known relation to each other—usually they are nearly equal—if the partial intervals be at once taken out they will show whether or not one wire has been read wrong, and if so, it can be corrected before the instrument has been disturbed. If but two wires are read, this check cannot be obtained and the check is a very valuable one. When so great care is taken, the observer should have a recorder to keep the note-book. If also the bubble is not read in the middle, but correction is made for its displacement, we have a true measure of the length of sight, so that this correction can be applied with great accuracy.

For greater convenience in reading the bubble, it is sometimes set on the top of the telescope as a striding level and provided with a mirror so that the observer can read it with one eye without removing the other from the eye-piece. With this arrangement, and a milled head screw under the wye, the observer can continually watch his bubble and hold it, by touching up the wye adjustment, to any desired reading, preferably to the center.

2. *Instrumental Errors.*—By instrumental errors we mean all errors that enter the work on account of any want of adjustment in the instrument. There are but two sources for this class of errors, viz.: (1) From line of sight not being parallel to the axis of the bubble, and (2) from the rod not being vertical. For, if the line of sight is horizontal, and the rod is vertical, a true difference of elevation may be obtained.

The parallelism of the line of sight and the bubble axis may be examined directly, or through an intermediate plane; directly by means of the "peg adjustment," and indirectly by means of the collimation and inclination. In the former, the instrument is usually set midway between two points and their difference of elevation determined. It is then moved near to one of them, and their difference of elevation again determined. If it is the same as before, the line of sight is horizontal and the adjustment is perfect. If the new difference of elevation is different from the first, the adjustment is made either on the bubble tube or on the collimating screws; that is to say, the bubble is brought to be parallel to the line of collimation, or *vice versa*. A simpler modification of this method is to set the level nearly over the first stake, and, by holding the rod upon it, read height of eye-piece. Then hold rod on distant stake and read. Now move the instrument nearly over the second stake and repeat the operation. If a is the difference of elevation by first set, and b is the difference by second set of readings, the true difference is $\frac{a \times b}{2}$ and the target can be set and line of collimation brought parallel to bubble accordingly. It will be seen that this is on the same

principle as the usual method, only that one of the sights in this case is reduced to zero, and the remaining two are equal.

The more common method, perhaps, for bringing the line of sight to a horizontal position is by making it first coincide with the axis of telescope (centers of rings) by revolving the latter about its axis (collimation), and then making the bubble parallel to the lower side of the rings by reversing the telescope in the wyes (inclination). This is on the principle that two lines that are parallel to a third are parallel to each other. This method does not involve any readings on the rod, and is more rapidly made than the other. All this on the supposition that the adjustment is to be made as nearly as possible, and then called correct, and no further account taken of it. Since it is impossible, however, to do anything exactly, in the best work the values of the residual errors in these adjustments are determined in divisions of bubble (which is seconds of arc) and a final correction made for them.

One marked peculiarity of these errors in leveling is the fact that, provided they are constant, they are wholly eliminated by taking equal back and fore sights. When the greatest possible accuracy is sought, a correction is applied to the difference between the sum of the distances on back sights and the sum of the distances on fore sights. This difference in distance is the residual length of back or fore sight for which the instrumental errors of adjustment remain uncompensated. One adds a great deal to the accuracy of a line of levels by carefully attending to making the back and fore sights on turning-points as nearly exact as possible.

The stability of the instrumental adjustments is greatly increased by keeping the instrument in the shade. A heavy canvas umbrella should be provided, which would thoroughly intercept the sun's rays, which the ordinary alpaca umbrella is by no means able to do.

The value of one division of bubble is also not a constant ordinarily. If the bubble is confined by metallic fastenings, a change of temperature of these will increase or diminish the strain on the bubble, and so change its curvature. If, therefore, the bubble is read out of the center and corrections applied to the rod readings, the value of the bubble should be tested under various temperatures, and if its value changes its fastenings should be examined and so arranged as to relieve it from variable pressure. If the bubble is always read in the middle of the scale, a change in the tube's curvature is of no consequence. I have used rubber bands for fastening bubble tubes in their cases, and they answered very well but need to be renewed every few months. I have known of bubbles changing nearly fifty per cent. of their value from a change of temperature of their metallic fastenings.

A small source of error arises from the sluggishness of the bubble, and the amount it lacks of coming to its true point of equilibrium. If it is very sluggish it is apt to be read before it has stopped moving, and if it stops short of its true center, a small angular error is committed. From theoretical considerations and also from experiments I have made, I conclude, that for a given tube, and within the limits of uniform curvature, the air bubble is sensitive directly in proportion to the square root of its length, and also, that the longer the air bubble the nearer it finally comes to its true center. By sensitiveness, I here mean readiness or quickness in responding to small changes of angle and rapidity in settling to its final position.

The bubble tube should therefore always have an air chamber, so that the length of the air bubble could be adjusted. Then a short bubble need never be used, and there would be a saving of both time and accuracy, inasmuch as the long bubble settles more quickly and more accurately than the short one.

2. The inclination of the rod may be called an instrumental error, or an error of observation. Without some special device, the rodman cannot hold his rod exactly plumb. It is an easy matter to attach a watch level to a rod, which can be adjusted daily by means of a plumb line. The verticality of the rod is sometimes tested by having it waved back and forth, after the target is set, to see if the wire corresponds to its highest position. This is well enough, perhaps, except when the target comes less than about three feet from the bottom of the rod. The error, at this height, in a New York rod used in this way is two thousandths

of a foot, and this error increases fifteen thousandths for a reading of 0.5 ft. on the rod. The error is caused by the front face of the rod being lifted when the rod is revolved backward and rests on its back corner at bottom.

Another small instrumental error is the error in the graduated length of rod. This is a slightly variable quantity, but only affects the total difference of elevation between initial and final points.

In the Coast Survey Precise Levels,* the instrumental constants of collimation and inclination are eliminated by reversing bubble and telescope on each back sight and fore sight, thus making four pointings for each complete reading. Each pointing is of a single wire on a target. The target is set once for the entire set of four readings, and the differential quantities read on a micrometer head attached to the elevating screw under the eye and wye. There are two objections to this method, aside from the labor. First, the target being set but once, there is but one reading for it, and opportunities for error in making this reading do not seem to be sufficiently checked. Second, when the final bisection of target occurs, the bubble is not readable, since this bisection has been made by moving the telescope out of the horizontal by the micrometer screw. The screw was previously read for a central position of bubble, and the bisection made and the screw read again. This is good provided the perfect stability of the instrument can be relied on. It is my experience, that with a 24-lb. instrument, on many kinds of ground, this cannot be done. Much less would I look for stability in a 45-lb. instrument, such as the Coast Survey use. The internal evidence of their published results also goes to show that when all corrections are applied, the four pointings of one reading have discrepancies that can only be satisfactorily explained by supposing the instrument changes between the two micrometer readings of a single pointing. I would therefore prefer a method which would enable me to know the exact direction of the line of sight at the instant when the reading is made.

For a delicate bubble, the changes of temperature in the different parts of the instrument, even when it is in the shade, and the instability of the instrument supports, usually cause the bubble to move almost continually, so that it must be constantly touched up by the wye adjustment while making the rod reading. For this purpose, the great advantage of being able to watch the bubble in a mirror while making the wire readings, is apparent. The accuracy of a pointing depends directly on our ability to know accurately the direction of the line of sight at the instant the reading is taken. This the observer is unable to do in the Coast Survey method.

3. *Errors from Unstable Supports.*—This is a much more important source of error than is generally supposed, inasmuch as it is usually cumulative. If the rod and instrument settle slightly the effect of each is the same and makes the final elevation too high by the total amount of the settling. The converse, of course, is true for a recovery or springing up of supports. If the rod settles between the reading of fore and back sights upon it the back sight readings are too large. If the instrument settles between the reading of a back and fore sight the fore sight reading is too small. The effect of each is the same, viz., to make the final elevation too great. Whether the supports rise or settle depends on the character of the ground and method of setting the tripod and kind of rod support used. In soft ground both rod and instrument may settle. In spongy and clayey grounds, if the tripod legs are forced in too hard they will recover somewhat when the pressure is removed, and if the rod is supported on a peg or pin it may spring up some also. I think, however, that the discrepancies caused by a rising of supports come mainly from the instrument rather than the rod. In very sandy ground or in pure sand a pin used for rod support is apt to settle or be lowered by the slight impact of setting the rod upon it if it is not of considerable size. A foot-plate of some 18 or 20 square inches is a better rod support in sand and in hard solid roadways than a pin, while the latter is preferable in clayey grounds.

If we assume a settling of supports the final result is too high by the total amount of the settling. If the line be run in the opposite direction, what is

* See Report of U. S. Coast and Geodetic Survey 1879.

now the initial point, as computed from what is now the final point, is too low by the total amount of settling. The mean of the two lines has this error eliminated, and the discrepancy between the two measures the total settling on the two lines. If, in a long series of stretches, duplicated in the opposite direction, the discrepancies are mostly of one sign, we are forced to conclude that some such action has occurred. If the line had been duplicated in the same direction this error would not show in the discrepancy, so that the check would be closer, but the actual error in the mean much greater than if duplicated in the opposite direction. The only method of eliminating this set of errors is, therefore, to duplicate the line in the opposite direction and take the mean.

Although these errors are very small if considered singly, they are cumulative, and become important if long distances are run without eliminating them.

The fact that when lines were duplicated in opposite directions the discrepancies tended to one sign has been often discussed, and various reasons assigned,* such as personal equation, illumination of target, etc., but I think it is all explained by the instability of supports. In so far as different observers would more or less effectually overcome these errors by their methods of setting rods and instruments, in just so far may it be called personal. It is not personal in the sense that it is in the reading of the rods or in the setting of the targets. If it were it would affect back and fore sights alike and would not appear in the difference between these readings, which is really the observed quantity.

4. *Atmospheric Errors.*—Errors from this source may be classified as coming from (1) Wind, (2) Tremulousness, (3) Variable Refraction.

(1.) Wind generally shakes the instrument and makes the holding of the rod difficult or impossible. For two seasons I have used a tent on windy days to protect the instrument, and with great success. Good work can be done in this way so long as the rod can be held. We also have large square canvas umbrellas that can be set on the ground to the windward of the instrument, and these effectually shield them in ordinary windy weather.

The tents used were wall-tents, 5 × 6 feet, and one 8-foot center pole. A square iron frame, 3 × 3½ feet, sewed into the canvas near the top, formed the lateral support there. It was held down by six or eight steel pins, 18 inches long and ½ inch diameter, with flat heads. These passed through iron rings sewed into the bottom. There were openings for the line of sight and a flap for the observer to enter and pass out with the instrument. These tents were made to be used on Gulf coast at a very windy season, when one-half the time would have been lost from high winds without them. The rodmen supported their rods by sticks held in the hand and braced against the rod at an angle, resting on the ground. Care had to be exercised that the rods were not thereby lifted from their sockets in the foot-plates.

(2.) Tremulousness is caused by a difference of temperature between air and ground, and always occurs in clear weather after the sun is a few hours high. This causes the target, or figures on a speaking rod, to appear to move up and down, giving rise to what is known as "dancing" or "boiling." This simply causes an uncertainty in the reading, depending directly on the degree of unsteadiness. It is a compensating error, and the observer must be his own judge as to when he must stop work in order to obtain the required degree of precision. The only remedy is to shorten the length of sight, but as there are some errors that multiply directly with the number of sights taken in a given distance, there is also a limit to which this remedy may be profitably carried. I do not think it advisable to use sights less than 100 feet if the highest accuracy is sought, and perhaps never more than 400 feet, even when the atmosphere is perfectly clear and steady. In clear weather not more than 3 or 4 hours a day can be utilized for the best work.

(3.) Variable refraction occurs when the sunshine suddenly comes upon or leaves the line. This happens along the edge of timber or under the brow of a hill, as when the line rapidly emerges from or comes into the shade, from the sun's movement, or on partially cloudy

* Acknowledged in Coast Survey Report 1879, p. 208, to be not, as yet, satisfactorily explained.

days, when the sun is alternately covered and clear. When from the first source, it occurs about 8 A. M. and 4 P. M. It is a peculiar phenomenon, and is more common in winter than in summer. The atmosphere is apparently steady and the sight well taken, but, upon checking it, the reading has changed, and may be observed to change gradually or suddenly, and sometimes to recover a part or all of its original movement, when the instruments were known to be stable. I have seen these changes of reading amount to 5 millimeters, or $\frac{1}{5}$ of an inch in a distance of 100 meters, or 328 feet. If the atmosphere is found to be in this condition, the work should be stopped for a while, as this state of affairs is not likely to continue long.

III.—Conclusion.

We have now considered about all the legitimate sources of error in leveling. Such errors as arise from setting on the wrong turning point, disturbing of turning point, slipping of target, holding rod wrong end up, etc., are due to accident or carelessness, and need not be mentioned.

With good instruments and with proper care and attention to all sources of error, a single instrument ought to be able to duplicate 30 miles of line a month with a wye level and a target rod, and bring all discrepancies within five hundredths of a foot into the square root of the distance in miles; or with the U. S. Precise Levels and speaking rods, reading three horizontal wires, one instrument should do the same work, bringing all discrepancies within two hundredths of a foot into the square root of the distance in miles. For the last 400 miles of Mississippi River Precise Levels with which I have been connected, about nine-tenths of the work has checked within a limit of 12 thousandths of a foot into the square root of the distance in miles, and the last 200 miles was done at the rate of 30 miles of completed duplicate line a month for a single instrument. The cost of this field-work, including one complete field reduction and the setting of permanent bench marks every 3 miles, was about $18 per mile of completed line.

The limit for the discrepancies between duplicate lines of levels under the Commission is 21 thousandths of a foot into the square root of distance in miles. On the Coast Survey it is 30 thousandths of a foot into the square root of the distance in miles, being respectively expressed by the following formula in terms of millimeters and kilometers:

Commission's limit, $d = 5^{mm} \sqrt{k}$

Coast Survey's " $d = 5^{mm} \sqrt{2k}.$

Magnetic Elements for 1882.—From the report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, the principal results for magnetic elements for 1882 were as follows:

Approximate mean Westerly declination (or variation) 18° 22′.

Mean horizontal force $\begin{cases} 3.913 \text{ (in English units).} \\ 1.804 \text{ (in metric units).} \end{cases}$

Mean dip $\begin{cases} 67° \ 33′ \ 33″ \text{ (by 9-in. needles).} \\ 67 \ \ 34 \ \ 34 \text{ (by 6-in. needles).} \\ 67 \ \ 34 \ \ 14 \text{ (by 3-in. needles).} \end{cases}$

—*Nautical Magazine.*

In their monthly report on the London water supplied by the companies during March, Mr. William Crookes, F. R. S., Dr. Wm. Odling and Dr. C. Meymott Tidy, say: "In respect to the proportion of organic matter, as indicated by the determinations of organic carbon, it is interesting to compare the results afforded by the past months analyses with those obtained before and during the recent period of quite exceptional river floods. Confining attention to the five companies taking their supply exclusively from the Thames, the average amount of organic carbon during the past month was 0.152 part in 100,000 parts of water; the average during the month of October, when the influence of the floods began to be felt, being 0.158 part. The average for the four months, preceding October and the floods, was 0.117 part; while the average for the four succeeding months, when the floods were at their highest, was 0.244 part—this last and highest average, corresponding to less than half a grain of organic matter per gallon of water, with a maximum in any individual sample, out of the sixty-eight averaged, falling short of three-quarters of a grain."

THE GASKILL PUMPING ENGINE AT SARATOGA, N. Y.

By JOHN W. HILL, M. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE many inquiries from engineers and others interested in pumping machinery for public water supply, upon points not included in the official report upon the performance of the Gaskill Compound Pumping Engine at Saratoga, is the principal apology for this paper.

The duty developed upon contract trial, charging the engine with all coal, burned, with no allowance for unburnt coal, ash, or clinker, or water entrained in the steam, was nearly 113,000,000 foot pounds. A duty which stands without a parallel, capacity and cost of engine considered.

The engine is horizontal, of the rotative beam, non-receiver, compound type, and involves several novel features of construction, whereby a large capacity and a high economy is obtained by a simple machine in a small compass.

The contractor's specification describes an engine containing four steam cylinders arranged in pairs, one high-pressure and one low-pressure cylinder to each pair, with the high-pressure cylinder mounted axially over the low-pressure cylinder.

Each pair of steam pistons drives one pump, the rod of which is connected direct to the cross-head of the low-pressure piston, and by means of the beam and system of short links with the high-pressure piston.

The main shaft common to both pairs of steam pistons turns in two heavy pillow blocks, mounted one on each pump-discharge chamber, between which the fly-wheel revolves. The ends of the shaft are provided with overhung cranks and pins set at quarters to receive the outer ends of the two shackle bars, the inner ends of which are strapped to the upper pins of the beam.

The pumps are of the double-acting plunger variety, packed with an internal gland which is adjusted by means of bolts passing through the rear pump heads. Each plunger is driven by a single rod.

The steam cylinders are all jacketed sides and heads, the condensation from which is trapped back to the boilers.

The air pumps are worked by a double arm, keyed to the inner end of each of the two beam shafts.

The steam valves are of the double-beat poppet style, the intermediate valves and exhaust valves to the low-pressure cylinders are gridiron slides.

The steam valves are adjustable at the will of the engineer, but the intermediate and exhaust valves have a fixed action relative to motion of piston. All the valves are driven by eccentrics, mounted upon two longitudinal shafts turning in bearings sprung from the inner sides of high-pressure cylinders.

These shafts are driven by miter wheels from the main shaft.

The receiving and discharge valves of the pumps are of rubber, working on metal stems and seats, small in diameter and numerous in quantity.

The entire engine is compactly massed upon a heavy horizontal bed-plate, to which the beam housings and steam and water cylinders are neatly and firmly attached.

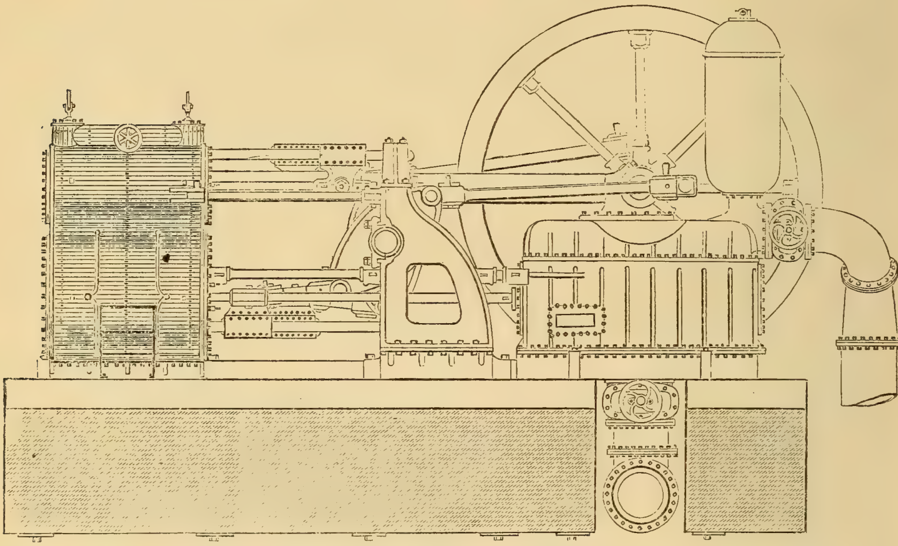
The engravings of the engine, which are kindly furnished by the Holly Manufacturing Company to illustrate this article, are drawn to scale, and correctly represent all the principal details of this celebrated machine.

The following dimensions of engine are taken from the contractor's specification, excepting measurements of pump plungers and strokes, which were made by the writer directly the contract trial was completed:

High-pressure cylinders (2), diam.	21 inches
High-pressure piston rods (2), diam.	3 "
High-pressure pistons, stroke....	36 "
Low " cylinders (2), diam.	42 "
" " piston rods (4), diam	3.5 "
Low-pressure pistons, stroke....	36 "
Clearance high-pressure cylinder.	2.35 per cent.
" low " "	2.50 " "

Pump plungers (2), diam	20 inches
" plunger rods (2), diam....	4 "
" plungers, stroke.........	36 "
Fly-wheel, diam	12.33 feet
" " weight..............	12,000 lbs.
Main shaft, diam...............	10 inches

The boilers furnishing steam to the engine are two in number, of the horizon-



SIDE ELEVATION OF ENGINE.

tal return tubular variety, and are each of the following dimensions:

Diameter of shell...............	65 inches
Length of shell.................	18 feet
Number of tubes................	87
Diameter of tubes, outside......	3 inches
Grate length....................	57 "
" width....................	72 "
Heating surface, both boilers....	2957.5 sq. ft.
Grate " " "	57 " "
Ratio heating to grate surface...	51.89
" grate surface to cross section of tubes................	8.717

The contract requirements of engine were a "pumping capacity of four millions United States gallons in twenty-foui hours, working at eighteen revolutions per minute against a pressure of eighty pounds per square inch, and the plant to develop a duty the equivalent of eighty million pounds of water raised one foot high, with a consumption of one hundred pounds of best coal."

By the terms of the contract under which the machinery was furnished, the builder had the privilege of selecting the coal to be burned, and the election of the pressures by steam and water gauges, and revolutions of engine, for the duty trial.

The Water Board of Saratoga had the option of the time and duration of duty trial.

The Water Board selected the writer, and the contractor selected Professor D. M. Greene, of Troy, N. Y., to conduct and report the trial, the results of which are recorded in this paper.

The town of Saratoga Springs is supplied with water for all purposes upon the direct service system, the water being pumped from a collecting reservoir, known as Loughberry Lake, directly into the distributing mains. After canvassing several doubtful methods of measuring the delivery of the pumps without cutting off the force main (a proposition which, for diplomatic reasons, could not be entertained by the Water Board), it was finally deemed sufficient to carefully measure the diameter and stroke of plungers, and from this data and the revolutions of engine during the trial, with such an allowance for slip or loss of action as was justified by precedent, to estimate the delivery of pumps.

The displacement of two plungers, each 21 inches diameter and 36 inches stroke, with a single rod 4 inches diameter for each revolution of the engine, in United States standard gallons, is 191.923.

Which quantity, reduced by a proper allowance for loss of action (with packing rings and valves tight under pressure), represents the approximate actual delivery of pumps.

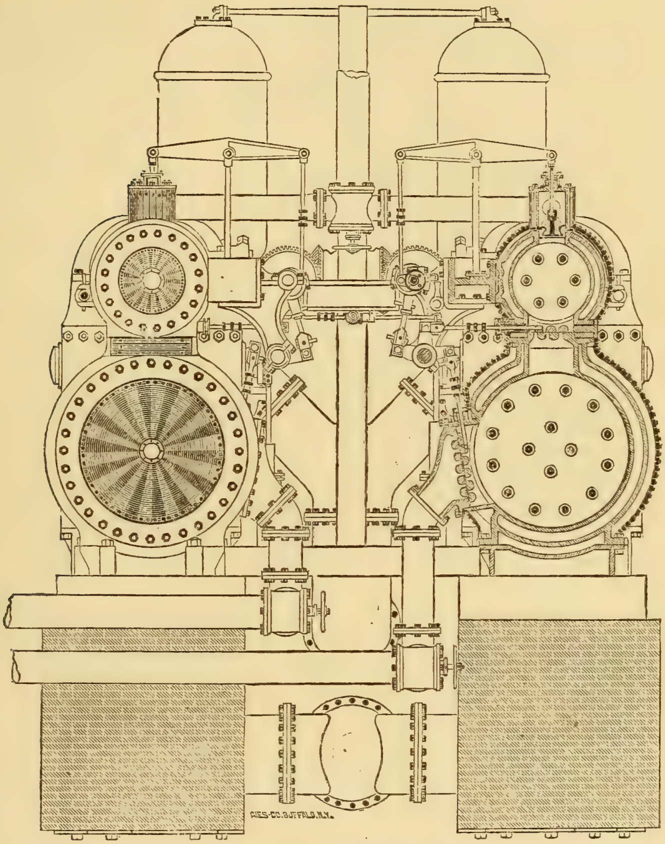
The pumps, four in number, of the Gaskill Compound Pumping Engine, built by the Holly Manufacturing Company for the water-supply of Memphis, Tenn., were substantially similar to the pumps of the Saratoga engine, and showed upon measurement by reservoir a delivery of 97.57 and 97.56 per cent. of the plunger displacement.

Adopting 97.5 per cent. as the coefficient of delivery for the pumps of this engine, then the actual daily discharge for eighteen revolutions per minute is $191.923 \times .975 \times 18 \times 60 \times 24 = 4,850,280$ gallons per diem of 24 hours.

The contract calls for a delivery of 4,000,000 gallons, whence the probable actual delivery is

$$\frac{4,850,280-4,000,000}{4,000,000} \times 100 = 21.257$$

per cent. in excess of contractor's guarantee.



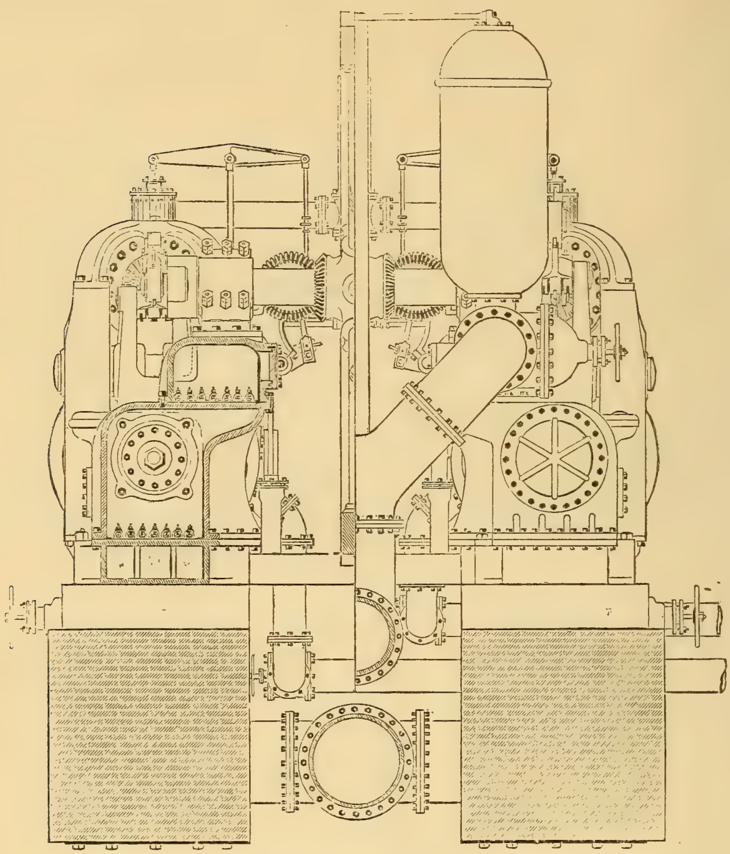
ELEVATION STEAM END.

Assuming, however, the very liberal slip of *five* per cent., then the excess of delivery over contractor's guarantee was nearly 726,000 gallons per diem—a margin sufficient to cover any possible error of judgment in estimating the capacity from pump dimensions and speed.

The contract provided for a delivery of "4,000,000 gallons per diem, against a domestic pressure of 80 pounds per square inch." During the duty trial of twenty hours, the engine made an average of 17.8742 revolutions per minute, pumping against an average pressure of

95.07 pounds by gauge, which was equivalent to a delivery per diem, under contract pressure, with an allowance of 2.5 per cent. for loss of action, of

$$\frac{17.8742 \times 95.07}{80} \times 187.125 \times 60 \times 24 =$$



ELEVATION WATER END.

5,723,667 gallons, or an excess of capacity above contractor's guarantee of

$$\frac{1,723,667}{4,000,000} \times 100 = 43.09 \text{ per cent.}$$

And yet it seems there are people in Saratoga who do not believe that this engine is capable of pumping 4,000,000 gallons of water against 80 pounds pressure at any speed.

The duty trial began at 8 A. M., Nov. 1, 1883, and terminated at 4 A. M., Nov. 2, covering an unbroken run of twenty hours, during which time, at regular intervals, observations were made of the engine counter, steam and water pressure gauges, water levels in boilers, temperatures of injection, overflow, water to heater and feed water to boilers, meter in feed pipe, and vacuum gauge.

The coal burned was "Lackawana" of excellent quality, as shown by the remarkably small percentage of refuse during the trial. This was weighed in double charges of 150 pounds each, and dumped into the coal wagon in front of the boilers; each charge to the boilers of 300 pounds was exhausted before the next charge was permitted in the wagon.

Indicator diagrams from all the steam cylinders were taken at irregular intervals during the trial.

From the log of trial are obtained the following averages and totals as affecting the duty:

Data from Duty Trial.

Engine counter at 8.00 A.M., Nov. 1st.	286,053
" " " 4.00 " " 2d.	307,502
Revolutions for 20 hours	21,449
Average pressure by water gauge	95.06875
" " " engine steam gauge	74.25
Average vacuum by gauge	27.295
" temperature of injection	56.225
" " " overflow	71.125
Total coal burned (pounds)	6,750

From which data we deduce the duty as follows: First, by the generally adopted method of

$$D=\frac{A\times P\times F\times 100}{C}$$

Where A represents the mean area of one plunger at right angles to its axis, P represents the total pressure per square inch of plunger, and consists, first, of the observed pressure by gauge, + or — difference of levels (center of water pressure gauge and source of supply) + an allowance for extra frictional resistances of water passages into and out of pumps, usually taken at one pound; and F represents the total plunger travel during trial, in feet. C represents the coal burned during trial. The condition of fires being alike at beginning and end of trial, with minimum fluctuations of steam pressure and water levels in boilers; and second, as a net or absolute duty by the method

$$D=\frac{G\times W\times H\times 100}{C}$$

Where G represents the actual delivery of water during trial in United States standard gallons, W the weight of water at observed temperature per gallon, and H the head in feet through which the water is pumped, consisting of the algebraic sum of the head due pressure by gauge, and the difference of levels (center of water pressure gauge and source of supply), C, as before, represents the total coal burned during trial.

Calculating duty by first method,

$$A=\frac{20^2\times .7854+(20^2\times .7854-4^2\times .7854)}{2}$$

$$=307.8768 \text{ sq. ins.}$$

P=pressure by gauge mean of 81 readings	95.06875
Difference of levels, center of water pressure gauge and source of supply $\frac{.23}{2.308}$	0.09965
Allowance for extra frictional resistance of water passage into and out of pumps	1.0000
	96.1684 lbs.
F=revolutions during trial	21,449
Piston travel per revolution	12
	257.388 feet
C=total coal burned	6,750 lbs.

Then by formula,

$$\frac{307.8768\times 96.1684\times 257{,}388\times 100}{6{,}750}$$

$$=112{,}899{,}983.104$$

foot pounds duty, or the work of raising nearly *one hundred and thirteen million pounds of water one foot high*, without frictional resistance or loss of effect.

The contract provides for a duty of eighty millions, which is exceeded by the duty based upon the conventional formula by 41.125 *per cent.*

Calculating the duty by second method:

G=191.923 x .975 x 21,449=	4,013,642.516 gals.
W=weight per gallon taken for temp. 56	8.33 lbs.
H=pressure by gauge (95.06875 x 2.308)	219.418
Difference of levels, center of gauge and source of supply	0.230
	219.648 feet.
C=total coal burned	6,750 lbs.

Then by formula,

$$\frac{4{,}013{,}642.516\times 8.33\times 219.648\times 100}{6{,}750}$$

$$=108{,}793{,}535.3$$

million pounds of water raised one foot high as the equivalent of absolute duty.

The contract provides for a duty of eighty millions, which is exceeded by the net duty, *nearly* 36 *per cent.*

In the following table are recapitulated the averages of all data and totals for duty trial:

Duration of trial hours	20
Average pressure by engine gauge, pounds	74.25
Average vacuum, inches	27.295
" temperature of injection, Fahr.	56.225
Average temperature of overflow, Fahr.	71.125
Average temperature of feed-water to boilers	169.175
Average pressure by gauge on force-main, pounds	95.06875
Difference of levels, center of pressure gauge and source of supply, pounds	0.09965

Revolutions in 20 hours.......	21,449
Revolutions per minute.......	17.8742
Piston speed per minute, feet..	107.2452
Total coal burned, pounds....	6,750
Calculated discharge of pumps per revolution, gals.........	191.923
Actual estimated discharge of pumps per revolution, gals..	187.125
Weight of water per gallon at 56 F., pounds..............	8.33
Contract delivery at 18 revolutions, gals...	4,000,000
Actual estimated delivery at 18 revolutions, gals............	4,850,280
Excess over contractor's guarantee, per cent............. .	21.257
Contract duty per 100 lbs., coal	80,000,000
Conventional duty as per trial..	112,899,983.104
Excess over contractor's guarantee, per cent.............	41.125
Net absolute duty as per trial..	108,793,535.3
Excess over contractor's guarantee, per cent..............	36

As a matter of interest to the Water Board and others, arrangements were made to determine the economy of the boilers during the duty trial, with the following result:

Duration of trial, hours............	20
Average pressure by gauge, pounds	76.644
Average temperature of feed-water.	169.175
Average temperature of water to meter..........................	85.575
Average percentage of water entrained.......	6.273
Water by meter record, cubic feet..	1181.8
Error of meter record, percentage..	2.8457
Weight of water per cubic foot, (temp. 85.575) pounds.... ...	62.135
Weight of water passed through meter, pounds....	75,520.773
Weight of water drawn off for temperature of feed, pounds.........	214.0
Weight of water to boilers, pounds.	75,306.773
Weight of water entrained, pounds.	4,723.994
Weight of net steam, pounds.......	70,582.779
Weight of coal burned, pounds.....	6,750
Steam per pound of coal from temperature of feed, pounds	10.4567
Steam per pound of coal from and at 212, pounds...........	11.286
Weight of ash and clinker returned, pounds	216.0
Percentage of non-combustible . . .	3.2
Coal burned per square foot of grate surface per hour, pounds........	5.833
Steam per square foot of heating surface per hour, pounds........	1.1933

The specification for the engine provides for "air pumps" to remove the air, water of condensation, and condensing water from "jet condensers," to be worked from the engine shaft. In place of which the contractor has substituted the now well-known Bulkley condensing apparatus. The merit of this change must be measured by the results. The vacuum obtained ranges from 27 to 28 inches, and the engine power, which, with air pumps, would be absorbed in discharging the contents of the condensers, is now utilized in forcing water into the mains.

Whatever gain in economy of performance was obtained by the change in condensing apparatus is a benefit alike to the contractor in his increased duty upon trial, and a continuous benefit to the water service to the extent of the power which otherwise would be required to work the air pumps.

Barring an unevenness of some bearings (which a limited use and proper attention to working joints will soon remedy), the performance of the engine was very satisfactory.

There was a defect in the cut-off valve motion of the left engine, to which the contractor's attention was called, and which he promised to correct. When corrected, the present hesitation of the engine in turning the right inboard center will disappear.

The following schedule contains the rate at which the fuel was consumed during the trial:

Date.	Time.	Coal charged.	Coal burned.	Pound per min.
Nov. 1.	800 A. M.	300	——	——
	8:56 "	300	300	5.357
	10:03 "	300	600	4.478
	10:51 "	300	900	6.250
	11:35 "	300	1200	6.818
	12:48 P. M.	300	1500	4.110
	1:59 "	300	1800	4.225
	2:47 "	300	2100	6.250
	3:20 "	300	2400	9.091
	4:13 "	300	2700	5.263
	5:11 "	300	3000	5.085
	6:03 "	300	3300	5.769
	6:43 "	300	3600	7.500
	7:53 "	300	3900	4.286
	8:48 "	300	4200	5.454
	9:33 "	300	4500	6.666
	10:31 "	300	4800	5.172
	11;32 "	300	5100	4.918
Nov. 2.	12:23 A. M.	300	5400	5.882
	1:17 "	300	5700	5.555
	2:02 "	300	6000	6.666
	2:51 "	300	6300	6.122
	3:23 "	150	6600	9.375
	4:00 "	——	6750	4.054
Average..........................				5.625

Coal burned first five hours......	1550.7 lbs.
" " second " "	1732.0 "
" " third " "	1659.93 "
" " last " "	1807.37 "
	6750.00

The mean water pressure, or head pumped against, including difference of levels and allowance of one pound for friction, for the several intervals of five hours each, was,

For first five hours	95.874
" second " "	96.900
" third " "	97.075
" last " "	94 824

And the corresponding revolutions of engine was,

For first five hours	5305
" second " "	5587
" third " "	5396
" last " "	5161

The duty varies directly as the head pumped against, as the revolutions of engine, and inversely as the coal burned, or by equation

$$D'=D\frac{p'r'c}{prc'}$$

where D=duty for whole trial.

p=mean head pumped against for whole trial.

r=revolutions for whole trial.

c=coal burned for whole trial.

p'=head pumped against for any interval of trial.

r'=revolutions for same interval.

c'=coal burned " " "

and D′=duty for same interval.

Whence the duties have been calculated approximately for the several intervals of trial as follows:

For first five hours	121 millions
" second " "	115 "
" third " "	116 "
" last " "	100 "
For whole trial approximately	113 "

While the duties for the several intervals are not as reliable as the duty calculated for whole trial, the method adopted in handling the coal was such as to furnish fair approximations at any stage of the test.

The writer gauged the fires carefully at beginning of trial, at end of trial, and frequently during the trial, to determine how faithfully the injunction was observed by the fireman and assistant in charge of the coal, to so employ the fuel that as each barrow was exhausted the original depth of fire should be restored. In brief, the fires were not to be diminished from the condition subsisting when trial began.

The writer and Professor Greene sat in the boiler room during the intervals between observations, with an assistant in constant attendance upon the coal scale and barrow, and every charge of coal was reported for time and quantity directly it was weighed up.

The final charge of 150 pounds of coal at 3:23 A. M. was recorded against the protest of the contractor, upon demand of the writer, in order that any error in restoring the original depth of fire should count against, rather than in favor of, the engine, and an examination of the schedule of coal fired will demonstrate that the final charge of 150 pounds might have been omitted with an increase in the duty, and without disturbing the previously established rate of coal consumption.

The mean rate of coal consumption per hour, for the first eighteen hours and fifty-one minutes, by the record was 334.2 pounds, and for entire trial, omitting final charge of 150 pounds, 330 pounds per hour, including final charge of 150 pounds, 337.50 pounds, and for last hour and nine minutes of trial, at the rate of 391.3 pounds per hour.

The mean steam pressure by boiler gauge was, at commencement of trial, 65 pounds, at end of trial 80 pounds, and as a mean for whole trial, 76.644 pounds; and mean water pressure upon pumps exclusive of difference of levels (source of supply and pressure gauge) and allowance for frictional resistance, was, at commencement of trial, 90 pounds, at end of trial 94.5 pounds, and as a mean for whole trial 95.068 pounds, from which it appears that the steam pressure was raised during 20 hours of trial from 65 to 80 pounds, whilst operating against a mean resistance of 95.068 pounds upon water pressure gauge, or an accumulating resistance from 90 to 94.5 pounds, with an increase in the revolutions of engine of last hour over first hour of trial, as follows:

Revolutions first hour	1026
" last "	1040
" mean for 20 hours	1072.45

The furnaces were in excellent shape, and the boilers cleaned a few days prior to the trial, and no known change in any portion of the plant calculated to affect the duty occurred during the trial, and the diminution of duty during the last interval of five hours can only be accounted for by a more lavish expenditure

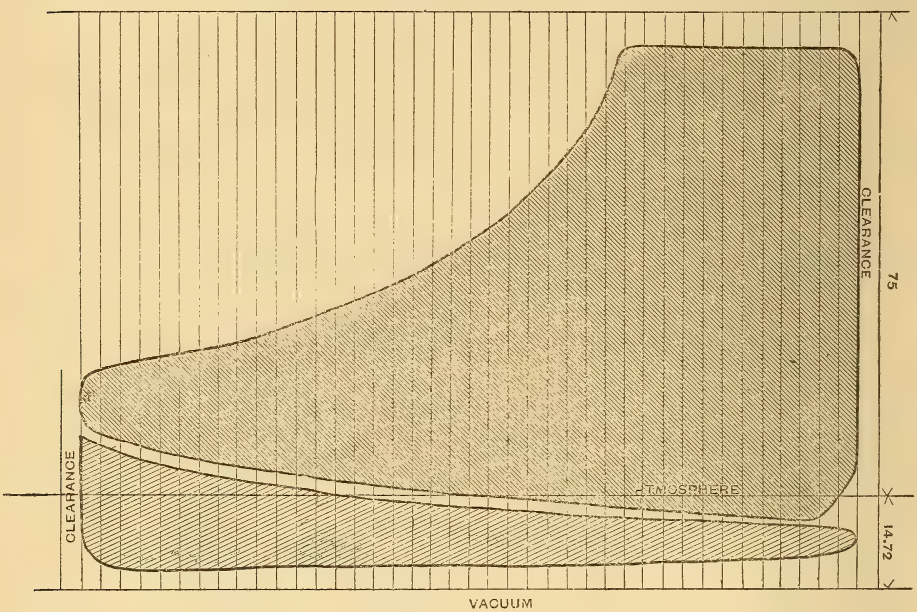
of coal upon the part of the contractor, and the special charge of 150 pounds during last hour of trial, to restore the original thickness of fires, which the writer thought at the time and still believes *might* have been omitted without prejudice to the interests of the Water Board, but which was added upon his demand in order that the error, if any, in restoring the fires should be calculated to diminish rather than increase the duty.

Omitting the last charge of 150 pounds of coal the duty for the 20 hours was 115½ millions, which more nearly represents the scientific duty of the engine than the one officially reported.

BACK END, RIGHT ENGINE.

Piston displacement, high-pressure cylinder	7.2158 cu. ft.
Clearance	0.1696 "
Volume	7.3854 "
Cut-off in decimal of stroke including clearance	0.3039
Exhaust closure in decimal of return stroke	0.9282
Piston displacement, low-pressure cylinder	28.8633 cu. ft.
Clearance	0.7215 "
Volume	29.5849 "
Release in decimal of stroke, including clearance	0.9927
Expansion by volumes	13.333
Mean expansion by volumes	13.119
" " " pressures	10.161*

FRONT END RIGHT ENGINE.



FRONT END, RIGHT ENGINE.

Piston displacement, high-pressure cylinder	7.0686 cu. ft.
Clearance	0.1661 "
Volume	7.2347 "
Cut-off in decimal of stroke, including clearance	0.3136
Exhaust closure in decimal of return stroke, including clearance	0.9282
Piston displacement, low-pressure cylinder	28.4625 cu. ft.
Clearance	0.7115 "
Volume	29.1740 "
Release in decimal of stroke, including clearance	0.9854
Expansion by volumes	12.906

The cuts are true copies of the diagrams taken at 12:10 P. M., Nov. 1st, from right engine reduced from 40 spring for high-pressure cylinder, and 12 spring for low-pressure cylinder, to a scale of 30 pounds per vertical inch for both cylinders. The upper diagrams are from the back end of cylinders, and the lower diagrams from the front or stuffing box end of cylinders, the condensing effect of the two low-pressure piston rods being

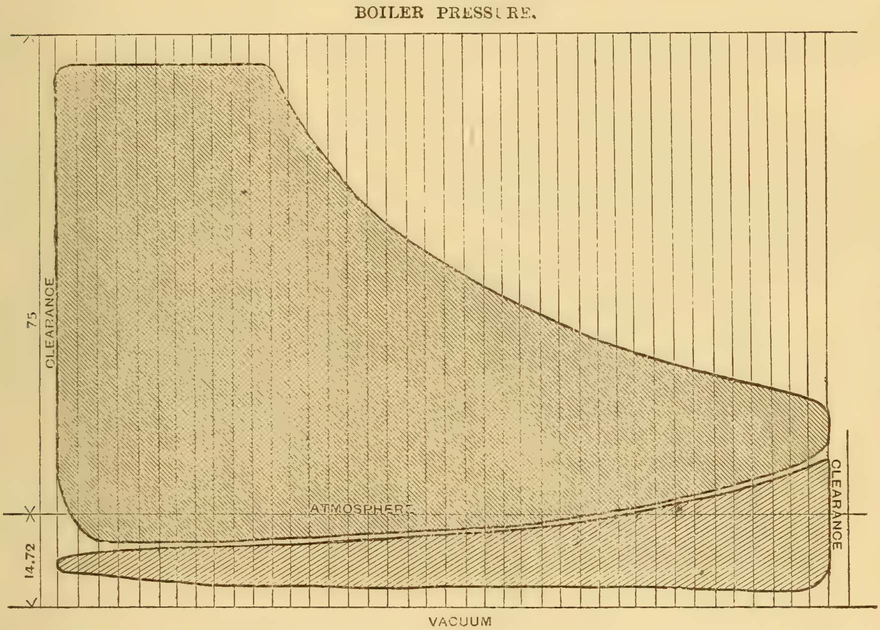
* The cylinders were jacketed, and the steam contained at least, by weight, *six* per cent. of water entrained, the evaporation of which, after cut-off in the high-pressure cylinders, and during expansion in the low-pressure cylinders, accounts for the smaller grade of expansion by pressures. With dry or slightly superheated steam the approximation of expansions by volumes and pressures would have been much closer.

plainly shown by the increased drop of pressure between the two cylinders.

The diagrams have not been criticised for defects of port and valve design, but it is safe to assert that no serious defects could be found in the light of the exalted duty obtained.

The diagrams were taken irregularly, and are too few in number to be accepted as exponents of power and steam consumption, but for what they are worth the data from them are given in the following table:

BACK END RIGHT ENGINE.



DATA FROM DIAGRAMS.

High-pressure cylinder mean effective pressure, front	47.38
High-pressure cylinder mean effective pressure, back	52.211
High-pressure cylinder mean inititial pressure	72.146
High-pressure cylinder mean terminal pressure	21.389
High-pressure cylinder mean counter-pressure at commencement of return stroke, absolute	25.31
High-pressure cylinder mean counter-pressure at end of return stroke, absolute	9.692
Low-pressure cylinder mean effective pressure, front	9.4025
Low-pressure cylinder mean effective pressure, back	9.9988
Low-pressure cylinder mean initial pressure, absolute	23.942
Low-pressure cylinder mean terminal pressure, absolute	8.574
Low-pressure cylinder mean counter-pressure at commencement of return stroke, absolute	6.574
Low-pressure cylinder mean counter-pressure at end of return stroke, absolute	2.338
Vacuum realized in cylinders, pounds	12.382
Vacuum realized in cylinders, inches	25.122
Factor of horse-power high-pressure cylinder, front	1.1027
Factor of horse-power high-pressure cylinder, back	1.1256
Factor of horse-power low-pressure cylinder, front	4.4400
Factor of horse-power low-pressure cylinder, back	4.5025
Total horse-power developed	197.7823
Caal per indicated horse-power per hour	1.7064
Duty of engine based on total work	116,033,755

Comparing the duties of steam and water ends of engine, it appears that of the total power developed, 97.3 per cent. was utilized in forcing water into the mains. The thermal value of a pound of pure carbon is 14,500 British units, and the percentage of carbon in the coal burned was fairly 96.8, from which the mechanical equivalent of heat per H. P. of engine was, $1.7064 \times .968 \times 14,500 \times 772 = 18,490,195$, and the efficiency of engine

$$\frac{1,980,000}{18,490,195}=.10708.$$

Had the writer, previous to the contract trial, suspected such a remarkable duty as was developed, the arrangements for indicator diagrams and other data upon which to discuss the exact scientific performance of the engine would have been more elaborate and complete; as it is, he can only rest with the hope that other engines of this type may come under his observation in the future, when an effort will be made to omit no inquiry bearing upon the scientific value of the machine.

CORROSION OF IRON AND STEEL.

By M. L. GRUNER.

Translated from Annales des Mines for VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

III.—*Action of Acidulated Water.*

We will pass now to the consideration of the experiments with acidulated water without exposure to the air.

In the first experiment, which was of three days' duration, the bath contained one half per cent. of concentrated acid, and was renewed every morning because the action ceased after six hours' time; at least there was no visible disengagement of hydrogen. At the end of the experiment one quarter per cent. of acid was added to favor the cleansing.

The results obtained from the 5th to the 8th of April at Saint Montan are exhibited in Table F, next column.

It is seen that the tempered steel is more strongly attacked than the untempered. Comparing Nos. 5, 6 and 7 of the Verdié steels with the first four of the same series, we find the acidulated water acts upon the high steels more energetically than upon the low ones. The ordinary manganese steel of Saint Montan, especially the hardened plate, is more corroded than the purer steels of Firminy.

Finally the compact and pure cast iron of Ruelle is oxidized much less than Nos. 1 and 3. The manganese specimen proving an exception.

It would seem that the crystalline condition, or rather the intimate combination with carbon, protects the manganese iron against the action of the acid. This is remarkable since sea water attacks manganese iron more strongly than the other cast irons.

TABLE F.

Origin of plates.	No.	Original weight.	Final weight.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Verdié forge.	1	242.3	242.0	0.3	All the steel covered with a thin, black coating.
	2	263.8	263.7	0.1	
	3	265.8	265.4	0.4	
	4	280.6	280.0	0.6	
	5	262.5	261.8	0.7	
	6	294.6	293.5	1.1	
	7	270.7	269.9	0.8	
Steel from St. Montan containing manganese and silicon.	*	363.6	362.0	1.6	Thin, black coating.
	†	367.0	362.9	4.1	Thick coating with a strong odor.
Black pig iron from St. Montan.	1	311.6	295.7	15.9	No. 1 was vigorously attacked & was covered with a graphitic coating, thick and odorous.
Charcoal iron.	2	336.0	335.2	0.8	
Gray cast iron.	3	391.3	382.4	8.9	
Manganese iron of St. Montan.	‡	723.4	721.9	1.5	

* Soft. † Tempered. ‡ White crystalline.

The following experiments exhibit still further the resistance of the white crystalline iron to the action of acidulated water. From the 17th to the 27th of July the same plates were exposed to water containing ½ per cent. of concentrated acid which was renewed twice; first on the 19th and again on the 21st of July.

TABLE G.

Origin of plates.	No.	Original weight.	Final weight.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Verdié forge	1	236.0	234 8	1.2	All the plates were well cleaned.
	2	257.4	256.8	0 6	
	3	258.8	258.0	0.8	
	4	273.2	272.5	0.7	
	5	255.1	253 9	1.2	
	6	286.3	285.3	1.0	
	7	263.2	262.0	1.2	
St. Montan steel containing manganese and silicon.	*	356.2	354 2	2.0	Covered with a black coating.
	†	356.1	350.8	5 3	
Black pig iron from St. Montan.	1	289.6	280.4	9 2	No. 1 briskly attacked.
Charcoal pig from Ruelle.	2	329.9	326.9	3.0	
Gray pot metal.	3	377.5	371 7	5.8	
Specular iron of St. Montan.	‡	720.0	719.0	1.0	

* Soft. † Tempered. ‡ White crystalline.

This experiment like the preceding shows that the ordinary steel of Saint Montan is more strongly affected than the pure steels of Firminy (Verdié), and that among the latter the higher carbonized steels are the more sensitive to the action of acid.

We see also that tempering favors the action of the acid. It seems at first singular when comparing this experiment with the preceding, that the irons are relatively less attacked and the steels more. This is explained by the slightly different conditions of the experiments.

Although the second experiment lasted ten days and the first but three, the proportion of acid was made a little higher in the first experiment by the additional ¼ per cent. put in to facilitate cleaning the plates. In the twelve liters employed in the two experiments there were 70 grams of acid consumed in the first against 60 grams in the second trial. By reason of the longer time in the second experiment, the air became an oxidizing agent as well as the acid. Now as moist air oxidizes steel nearly as well as iron, we can understand how in the former experiments (Table C) there was a less difference between the irons and steels than in the experiment F where the action was nearly all due to the acid.

The steels of Unieux (Holtzer) were also subjected to the action of acidulated water without exposure to the air. The experiment continued seven days. The proportion of acid was 2-5 grams for 4 liters (a little more than ½ per cent.) and the bath was renewed after the third day. Table H exhibits the results:

TABLE H.

Origin of plates.	No.	Original wt.	Final wt.	Loss.	Remarks.
		gr.	gr.	gr.	
Annealed steel from Holtzer works	1	301.9	300.5	1.4	Thin covering of black rust.
	2	326.1	324.4	1.7	
	2a	334.7	331.9	2.8	
	3	305.8	304.2	1.6	
	4	322 8	320.8	2.0	
	5	341.8	338.5	3.3	The chrome steel had a thick gray coating.
	6	340.3	339.0	1.3	
Tempered steel. Holtzer.	1	315.7	343.9	1.8	The black deposit is thicker on the tempered plates.
	3	293.3	291.1	2.1	
	4	329.7	326.5	3.2	
	5	335.9	333.2	2.7	
Mild steel of Montluçon	1	148.9	148.0	0.9	
	2	161.9	161.4	0.7	
High steel of St. Montan	1	284.5	282.2	2.3	
	2	360.0	357.5	2.5	

This experiment confirms the results exhibited by tables E and G. We see that the soft steels of Montluçon are as susceptible as the hard and manganese steels of Saint Montan; that the soft chrome steel oxidizes more readily in the air than the others. Furthermore, that in the air the simple carbon steels are more oxidizable when tempered, but Chrome steel is somewhat protected by tempering.

By comparing this experiment with Table B, we see that although acidulated water oxidized soft steels less than hard ones, the action of moist air is only slightly modified by the degree of purity

or percentage of carbon; excepting always chrome steel.

From these tables and from comparisons made with the results obtained by English engineers, we conclude that the action of sea water or acidulated water upon iron differs from that of moist air; and that consequently experiments made with acidulated water teach us nothing of the prolonged action of moist air or salt water. To avoid error we will reconsider separately the results furnished by the experiments.

1st. *Action of Moist Air.*—Tables A, B, C and D prove that all the steels, high or low, except chrome steel, are corroded to the same extent by moist air; that the cast irons resist better than the steels, and the white crystalline *spiegel* iron better than the gray irons.

Chromium favors the corrosion of annealed steel while tungsten produces a contrary effect.

Tables B and D seem to establish the fact that tempering invites rust. In order to test this question a longer test is necessary, for in the two cases referred to, acidulated water played too important a part to enable us to state definitely that tempering favors rusting in moist air alone.

The experiments of M. R. Mallet, in which the specimens were exposed two years to the moist air of Dublin, confirm the above conclusions. The cast iron specimens resisting rust better than wrought iron or steel; the gray cast iron two or three times, and the white cast iron five or six times as well.

Between the irons and the steels on the whole there was not much difference; the latter when tempered resisting a little better than the wrought iron.

M. Parker subjected several kinds of wrought-iron plate and bar to the smoky atmosphere of London for 455 consecutive days. These experiments also proved that all the varieties rust to about the same degree, the commonest bar iron resisting a little better than the best Lowmoor, and the latter a little better than soft cast iron holding $\frac{1}{4}$ to $\frac{1}{2}$ per cent. of manganese.

The English Admiralty and Mr. Phillips have continued the experiments with pure water and with sea water.

2d. *Action of Sea Water.* — The Tables E and E′ exhibit the effect of sea water. These effects are to some extent the reverse of those of moist air. Thus the irons are more strongly attacked than the steels, and among the irons the manganese specimen is most corroded—twice as much as the gray or black iron from the same work. The common iron for pots, and containing phosphorus, is rusted twice as much as the purer charcoal iron from Ruelle. (Table E.)

Tables E and E′ prove also that sea water attacks tempered steel less strongly than untempered, and that the presence of manganese favors the corrosion of steel plates. Apart from this, the action of sea water is about the same for all degrees of carbonization.

The results of M. Mallet's experiments are quite in accord with the above; proving also that wrought iron and steel are affected to about the same degree by salt water, but the steel contrary to the writer's observation was corroded more than cast iron. This difference is probably due partly to the greater amount of manganese in our specimens, and partly also to the circumstance mentioned by Mr. Parker, that M. Mallet's cast-iron plates were used in rough condition, as they came from the foundry, while the wrought iron and steel specimens were previously prepared as in our experiments.

With regard to the amount of oxidation of wrought iron and steel in salt water, it appears, according to Mallet, a little less than that due to moist air during the same length of time, and on the other hand it is nine times as great as that of fresh water.

It is well to observe, however, that the action of the air varies with the season and the climate, and the action of water varies with the depth below the surface. It is observed by iron ship builders that the iron plates are most corroded along the water line; that the parts that are alternately wet by the sea and exposed to the air corrode much faster than those constantly immersed. An analogous fact is observed in steam boilers. The corrosion is most rapid where the feed water is constantly brought in contact, holding air freshly dissolved.

Messrs. Parker and Phillips have studied the action of sea water upon ships' plates and the action of distilled water upon steam boilers. The experi-

ments were chiefly upon wrought iron and mild cast steel. Their results accord with ours and with the observation of the English constructors, proving that there is no sensible difference between wrought iron and low steel in resisting corrosion when used for hulls of ships or in steam boilers using distilled water. In the meantime it seems that water, like moist air, acts upon mild steel a little more readily than upon Lowmoor iron or common wrought iron, but the difference is not more than 5 or 6 per cent. at the most. Notwithstanding this slight inferiority, steel is preferred for purposes of construction by reason of its greater strength and superior homogeneousness. One precaution is indispensable. It is important to scour the plates before using them, otherwise the isolated patches of oxide on the surface tend by setting up a galvanic action to excite corrosion on places not protected by oxide. The action of such spots of rust is nearly as deleterious as a copper plate riveted to the iron. The strip of zinc sometimes employed as a galvanic protector will not entirely neutralize the deleterious influence of patches of oxide.

3d. *Action of Acidulated Water.* — This is chiefly remarkable from the fact that unlike sea water it attacks high steels and impure steels more strongly than the low steels, and tempered more than the untempered steels. It is important therefore to avoid the use of acid even for cleaning metal that is designed for experiments in moist atmosphere. It is liable to vitiate the results as was before remarked in regard to the experiments reported in Tables B, C and D.

The presence of chromium favors corrosion by acidulated water as it also does by moist air and sea water. As to the action of acidulated water upon cast iron, it varies with the compactness of the metal and elements combined with it. The graphitic gray irons are speedily acted upon, while the white crystalline castings resist much better than steel. In a general way the acidulated water acts differently from moist air or sea water. No conclusion about the oxidizing action of moist air or sea water can be fairly reached by experiments made with acidulated water. A conclusion has been wrongly based on M. Adamson's experiments to the effect that moist air tended to corrode hard steel rails more rapidly. In fact these experiments teach us nothing about the wear of rails. Prolonged trials are necessary of exposure to a moist atmosphere before we can pronounce upon the results.

While waiting for such experiences, it seems necessary to admit that the experiments of Messrs. Mallet and Parker teach that the hardness of steel seems to have little influence upon its corrosiveness under the action of moist air. Notwithstanding this it is quite evident that if rails are found to wear faster in tunnels than outside, it is especially due to the moist heat which favors rusting.

4th. *Experiments of M. Brustlein, Director of the Unieux Forges.*—There remains to be given as complementary to the writer's experiments, a report of some interesting analogous studies by M. Brustlein, Director of the Unieux Works.

This engineer sought to determine the influence of manganese upon the properties of steel. He cast three ingots of steel holding six thousandths of carbon, and respectively 1.737—1.188 and 0.434 per cent. of manganese.

It was at once established that the density and rigidity increased with the amount of manganese. Some small bars tried under a hammer took a deflection not quite in proportion to the percentage of manganese. The bending was relatively least for the higher proportions. Some other bars of 13 millimeters diameter and 100 millimeters length between the heads, when tried under a tensile strain, exhibited the following results:

Per ct. of manganese	Elastic limit.	Breaking weight.	Elongation.	Diam. at fracture.	Remarks.
p. 100.	kilog.	kilog.	p. 100.	milm.	
1.737	48.9	88.2	17.0	9.6	Black fibrous, but clear at center.
1.188	44.8	78.2	18.6	9.5	Black fibrous, but clear at center.
0.434	39.4	66.8	22.4	9.6	Gray fibrous.

Two sets of bars from the same three ingots were suspended, one set in the flue of a Galloway boiler, and the other in the water of the same boiler at a distance from the feed-water inlet. The experiment continued 112 days. The weight in both sets remained sensibly the same; but the set from the flue were slightly coated with magnetic oxide, while those

from the boiler were covered with a brown muddy deposit of 1 to 2 millimeters thickness, under which appeared a black carbonaceous coating similar to that which appears on steel when attacked by acidulated water. The bars were then subjected to tensile strain with the annexed results.

Bars in flue.	Elastic limit.	Breaking weight.	Elongation.	Diam. at fracture.	Remarks.
Per ct. mn.	kilog.	kilog.	p. 100.	milm.	
1.737	41.8	69.4	16.2	8.8	Dark gray; fibrous.
1.188	38.0	64.7	21.3	8.7	
0.434	30.7	58.7	13.7	9.3	Gray; fibrous.

Bars in water.	Elastic limit.	Breaking weight.	Elongation.	Diam. at fracture.	Remarks.
Per ct. mn.	kilog.	kilog.	p. 100.	milm.	
1.737	44.8	80.2	15.3	9.4	Dark gray; fibrous.
1.188	40.1	73.5	15.7	10.8	
0.434	32.1	63.4	14.0	10.7	Gray; granular.

We see that the metal was acted upon in the water as well as in the smoke flue, and the steel suffered more than the manganese; it has become less ductile and granular.

This is a point that boiler makers would do well to note; that steel plates lose in time a part of their tenacity and flexibility.

M. Brustlein has also determined that acidulated water dissolves most actively the metal that contains most manganese. But we cannot conclude from what we have previously determined that fresh water in boilers would act in the same way. Meanwhile, since manganese, according to the preceding experiments, favors the action of either fresh or salt water upon steel, it may be considered as not the less established that as little manganese as possible should be permitted in plates for steam boilers or hulls of ships.

VARIOUS METHODS OF DETERMINING DIMENSIONS.

By DR. JAMES WEYRAUCH, Professor at the Polytechnic of Stuttgart.

Selected Papers of the Institution of Civil Engineers.

I.

In a former paper, the author promised subsequently to give a short demonstration and comparison of those methods of determining dimensions, based on the assumption of a variable strength, which have recently been proposed. This review it is intended to give in the present paper. The development of each method must of course be confined to the essential ideas involved. For purposes of ready comparison this will be followed by the determination of the limiting stresses per unit of sectional area allowed by each, and, in conclusion, a number of examples and tables will be given. Some of the methods shown have obtained a footing in practice, others contain valuable ideas for further development, and all are of interest as steps forward on the road towards a rational method of determining dimensions. When it is once recognized that the strength of materials has not the same value in all the various circumstances in which the loads are applied, a method different to that hitherto used must gain ground. Those who cannot approve any of the proposed methods will, perhaps, by a knowledge of them, be stimulated to achieve something better.

The general points of view explained in I. to IV. of the author's former paper will be assumed as known, and the nomenclature and notation there introduced maintained. Hence t denotes the *statical breaking strength* per unit of area (developed by a static load once applied), u the *primitive strength* (breaking strength for stress of one sense alternating with zero), s the *vibration strength* (strength developed by oscillations, where there is an alternation of stresses of equal intensity in opposite senses), a the *ultimate working strength* (the breaking strength under the particular circumstances of loading), d the differ-

ence of the limiting stresses, and b the admissible stress; the word stress being an abbreviation for "intensity of stress," unless otherwise indicated.

For the sectional area F, let φ denote the ratio of the numerically smaller to the numerically greater limit of stress (φ being positive when both are tensile, negative when one is tensile and the other compressive, φ_0 the ratio of the stress produced by the fixed or "dead" load, if any, to the numerically greater limit of stress, and B_0, B_1 and B_2 the numerical values (without signs positive or negative) of the fixed load,* and the two limiting values of the variable or "live" load.

Then there follows for tension only or compression only, if max. B and min. B denote the numerical values of the upper and lower limits of total load,

$$\varphi = \frac{\min B}{\max B} \qquad \varphi_0 = \frac{B_0}{\max B} \quad . \quad . \quad . \quad (1)$$

and for alterations of tension and compression, when max B and max B′ denote the values of the numerically greater and smaller limiting total loads respectively,

$$\varphi = -\frac{\max B'}{\max B} \quad \varphi_0 = \pm \frac{B_0}{\max B} \quad . \quad . \quad . \quad (2)$$

In the latter expression the upper or lower sign is to be taken according as the dead load produces stress of the same or opposite sense as the upper limit of live load.

If the admissible stress b be known, then

$$F = \frac{\max B}{b}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

as in equation (2) of the former paper.

Where in the sequel numerical values occur, the stresses b are given in kilograms per square centimeter, the values of F in square centimeters.

I. Gerber's Method.

The first work on the admissible stresses for iron and steel based on Wöhler's experiments, was written by Gerber, the manager of the South German Bridge-building Establishment, in 1872, adopted by the Bavarian Government, and published in 1874.†

* "Load," as in the former paper, means the total amount of the external force, tensile or compressive as the case may be, applied to the single bar or piece under consideration.

† Gerber, "Bestimmung der zulässigen Spannungen in Eisenconstructionen."

Every piece of a square unit of sectional area would be destroyed by a static load producing stress of intensity $=t$. The same result may be attained by a load constant only as to one portion c, and as to the other d acting by numerous temporary repetitions; hence the difference of stress d is equivalent to a certain static load τd, and there follows

$$(a) \qquad c + \tau d = t = \delta d,$$

where δ denotes a coefficient, determined by the preceding equation.

If for a piece of any section F, the static calculation give a fixed load, B_c and a limiting value B_v of the live load, then these loads might by means of the equation

$$(b) \qquad B_c + \tau B_v = B_r = \delta B_v$$

be reduced to a static load, if only τ or δ were known, and the requisite section would then be found by

$$(c) \qquad F = \frac{B_r}{b_r}$$

where b_r is the admissible stress per unit of area for a static load.

The hypothetical loads B_r Gerber calls "reduced forces."

As Wöhler has determined the statical breaking strength t for certain materials, and also the possible differences of stress d for various initial stresses c, by substituting these special values c, d, t, in the above equation, the corresponding values of the coefficients τ, δ may be at once ascertained. τ and δ will of course vary, not irregularly, but continuously with the ratio

$$(d) \qquad \varphi = \frac{c}{d} = \frac{B_c}{B_v}.$$

In order to express the law according to which this variation takes place, Gerber makes $x = \frac{c}{t}$, $y = \frac{d}{t}$, assumes the curve determined by x and y, having regard to the numerical values obtained by Wöhler, to be a parabola, and thus gets relations, by means of which δ, and therewith also

$$(e) \qquad \tau = \delta - \varphi$$

may be determined. In general, if δ denote a constant depending on the nature of the material,

$$(f) \quad \delta = \tfrac{1}{2}\left(\delta + \sqrt{\delta^2 + 4\varphi^2 + 4\varphi + 1}\right) \; . \; (4)$$

The values of δ can be previously calculated for regularly progressive values of φ and tabulated, as done by Gerber.

In all formulas B_c and B_v are to be substituted with their signs (tension positive, compression negative), so that Φ becomes positive or negative according as B_c, B_v, have similar or opposite signs. If the straining force B_v due to the "live" load* act in the contrary sense to that due to the "dead" load, the total force may become $B_c + B_v = 0$, in which case $\varphi = -1$. The values of B_r always have the same sign as $B_c + B_v$.

The practical application of the preceding method Gerber make as follows:

In order to take into account vibrations and impact, for the live load B_v, is substituted n times that quantity, and then

$$\varphi = \frac{B_c}{nB_v} \quad . \quad . \quad . \quad . \quad (5)$$

For this value of φ, δ is determined by (4), and there results

$$B_r = n\delta B_v \quad . \quad . \quad . \quad . \quad (6)$$

$$F = \frac{B_r}{b_r} = \frac{n\delta B_v}{b_r} \quad . \quad . \quad . \quad . \quad (7)$$

Gerber chooses for iron $\delta = 1.5$, $n = 1.5$, for structures in which lightness is the principal requisite and small alterations of form are no disadvantage, $b_r = 2{,}400$, and for structures in which the greatest possible durability is required, $b_r = 1{,}600$ kilograms per square centimeter. Hence, for the latter

$$\varphi = \frac{B_c}{1.5\,B_v} \quad . \quad . \quad . \quad (8)$$

$$\delta = \tfrac{1}{4}(3 + \sqrt{16\,\varphi^2 + 16\,\varphi + 13}) \quad . \quad . \quad . \quad (9)$$

$$F = \frac{1.5\,\delta B_v}{1{,}600} \text{ square centimeters} \quad . \quad . \quad (10)$$

If the moving load may become positive as well as negative, φ, δ, F must be calculated for both limiting values B_v separately, and the sum of the numerical values $F = F_1 + F_2$ gives the actual sectional area.

The object now is to express the stress per unit of area allowed by Gerber.

If a piece be subjected to tension only or compression only, then minimum $B \leqq B_0$.

$$\left.\begin{aligned} \varphi_1 &= \frac{B_0}{n(\max B - B_0)} F_1 \\ &= \frac{n\,\delta_1}{b_r}\,(\max B - B_0) \quad . \quad . \\ \varphi_2 &= \frac{B_0}{n(B_0 - \min B)} F_2 \\ &= \frac{n\,\delta_2}{b_r}\,(B_0 - \min B) \quad . \quad . \end{aligned}\right\} (11)$$

From this follows the resultant maximum stress per unit of area

$$\left.\begin{aligned} b &= \frac{\max B}{F_1 + F_2} = \frac{1}{\delta_1(1-\varphi_0) + \delta_2(\varphi_0 - \varphi)}\,\frac{b_r}{n} \\ &\text{where with (1)} \\ &\delta_1 \text{ from (4) corresponds to} \\ &\qquad \varphi_1 = \frac{\varphi_0}{n\,(1-\varphi_0)} \quad . \quad . \quad . \\ &\delta_2 \text{ from (4) corresponds to} \\ &\qquad \varphi_2 = -\frac{\varphi_0}{n\,(\varphi_0 - \varphi)} \quad . \quad . \quad . \end{aligned}\right\} (12)$$

If in a special case the fixed load coincides with one of the limiting loads, then either $\varphi_0 = \varphi$, $F_2 = 0$, or $\varphi_0 = 1$, $F_1 = 0$, and from (12)

$$\left.\begin{aligned} b &= \frac{1}{\delta\,(1-\varphi)}\,\frac{b_r}{n} \quad . \quad . \quad . \quad . \\ &\text{where with help of (1)} \\ &\text{for } B_0 = \min B, \text{ from (4) corresponds to } \varphi = \frac{\varphi}{n(1-\varphi)} \\ &\text{for } B_0 = \max B,\ \delta \text{ from (4) corresponds to } \varphi = -\frac{1}{n(1-\varphi)} \end{aligned}\right\} (13)$$

If, on the other hand, a piece has to sustain alterations of tension and compression, there follows, according as the fixed load (numerical value B_0) has the same or the opposite sign as the higher limiting load (numerical value max. B) with the upper or lower sign,

$$\varphi_1 = \frac{\pm B_0}{n\,(\max B \mp B_0)};\ F_1 = \frac{n\,\delta_1}{b_r}(\max B \mp B_0)$$

$$\varphi_2 = \frac{\mp B_0}{n\,(\max B' \pm B_0)};\ F_2 = \frac{n\,\delta_2}{b_r}(\max B' \pm B_0)$$

The greatest stress per unit of sectional area now amounts to

* In the sequel the straining force due to the live load will be called simply the "live load," and that produced by the "dead" load the "fixed load," these terms referring to the forces applied to a single piece, and not to the *weight* on the structure of which the piece forms a part.

$$\left.\begin{array}{l} b=\dfrac{\max B}{F_1+F_2}= \\ \dfrac{1}{\delta_1(1-\varphi_0)+\delta_2(\varphi_0-\varphi)} \cdot \dfrac{b_r}{n} \\ \text{where with (2)} \\ \quad \delta_1 \text{ from (4) corresponds to} \\ \qquad \varphi_1=\dfrac{\varphi_0}{n\,(1-\varphi_0)} \\ \quad \delta_2 \text{ from (4) corresponds to} \\ \qquad \varphi_2=\dfrac{\varphi_0}{n\,(\varphi-\varphi_0)} \end{array}\right\} (14)$$

When in a special case the permanent load coincides with one of the limiting loads, either $\varphi_0=\varphi$, $F_2=0$, or $\varphi_0=1$, $F_1=0$, but by (14)

$$\left.\begin{array}{l} b=\dfrac{1}{\delta(1-\Phi)}\,\dfrac{b_r}{n} \quad . \quad . \quad . \quad . \\ \text{where by (2)} \\ \quad \text{for } B_0=\max B',\ \delta \text{ from (4)} \\ \qquad \text{corresponds to } \varphi=\dfrac{\varphi}{n(1-\varphi)} \\ \quad \text{for } B_0=\max B,\ \delta \text{ from (4)} \\ \qquad \text{corresponds to } \varphi=-\dfrac{1}{n(1-\varphi)} \end{array}\right\} (15)$$

For iron structures of the greatest possible durability the values $n=1.5$, $b_r=$ 1,600 would have to be substituted, in which case equation (4) merges into (9).

II. Schaffer's Modification.

In Gerber's method, the way in which he proceeds in the case of the live load having different signs, provokes criticism, particularly with regard to such structural parts as are subjected alternately to tension and compression. The calculation is made as though there were two pieces, instead of taking into account the whole difference of stress at once in accordance with Wöhler's law. This drawback Professor Schaffer, of the Darmstadt Polytechnic, has avoided by a modification of Gerber's method. His first work on this subject appeared in 1874, and was supplemented by subsequent papers.*

Schaffer, in developing his theory, reversing Gerber's method, makes $x=\frac{d}{t}$, $y=\frac{c}{t}$; and recognizing Gerber's conclusions, with regard to the relation between x and y, puts the latter into a somewhat different form by introducing

$$(a) \qquad \xi=\frac{d}{a},$$

a denoting the ultimate working strength. For this latter the following general expression results

$$a=\frac{-\delta\xi+\sqrt{\delta^2\xi^2+(2-\xi)^2}}{(2-\xi)^2}\,2t \quad . \quad . \quad (16)$$

where δ is the same constant, dependent on the nature of the material, used by Gerber.

The numerical values of the limiting straining forces resulting from the moving load only, producing tension solely or compression solely (min $B \underline{\underline{<}} B_0$), are expressed as follows:

$$B_1=\max B-B_0 \quad . \quad . \quad . \quad . \quad (17)$$
$$B_2=B_0-\min B \quad . \quad . \quad . \quad . \quad (18)$$

and for alternate tension and compression (max $B' \underline{\underline{\leq}} \max B$)

$$B_1=\max B\mp B_0 \quad . \quad . \quad . \quad . \quad (19)$$
$$B_2=\max B'\pm B_0 \quad . \quad . \quad . \quad . \quad (20)$$

In the latter case the upper or lower sign is to be used according as the fixed load (numerical value B_0) is of the same or opposite sense as the higher limiting load (numerical value max B).

As the difference of load for the whole section with tension or compression only is max B — min B, and for alternate tension and compression max B + max B′, there follows with reference to (a) generally

$$(b) \qquad \xi=\frac{d}{a}=\frac{B_1+B_2}{B_1\pm B_0};$$

this value must of course always be positive.

If the ultimate working strength a corresponding to the preceding value of ξ is to be determined, and the unit of area of the section subjected to this stress, the limit of destruction would just be attained without the action of impact and vibrations. In order to take the latter into account, Schaffer introduces all forces resulting from the moving load into the calculation multiplied with a factor n, even in determining ξ, and for further safety only the m^{th} part of the hypothetical working strength corresponding to this value of ξ is used in the calcula-

* Schäffer. "Bestimmung der zulässigen Beanspruckung für Eisenconstructionen." Zeitschrift für Bauwesen, 1874. Deutsche Bauzeitung, 1875, 1876.

tions. The determination of the sectional area then takes the following form:

From the static calculation is determined

$$\xi = \frac{n(B_1 + B_2)}{nB_1 \pm B_0} \quad . \quad . \quad (21)$$

where B_0 has the upper or lower sign prefixed according as it corresponds to a load in a similar or an opposite sense to max B. Then equation (16) gives the hypothetical working strength a, and the sectional area becomes

$$F = \frac{m}{a}(nB_1 \pm B_0) \quad . \quad . \quad . \quad (22)$$

Like Gerber, Schaffer adopts for iron the values $\delta = 1.5$, $n = 1.5$, and for permanent structures $\frac{t}{m} = b_r = 1{,}600$, so that

$$\xi = \frac{1.5\,(B_1 + B_2)}{1.5\,B_1 \pm B_0} \quad . \quad . \quad . \quad (23)$$

$$\frac{a}{m} = \frac{-3\xi + \sqrt{13\,\xi^2 - 16\xi + 16}}{(2-\xi)^2} 1{,}600 \quad . \quad (24)$$

$$F = \frac{m}{a}(1.5\,B_1 \pm B_0) \quad . \quad . \quad . \quad (25)$$

For $\xi = 2$, formula (24) gives $\frac{a}{m} = \frac{0}{0}$; the determination of the value of this expression leads to the result $\frac{a}{m} = 534$.

Schaffer's equations must give the same results as Gerber's when the fixed load coincides with one of the limiting loads, because in that case Gerber's division into two pieces does not enter into the calculation. Schaffer has also demonstrated how by his method liability to buckling may be taken into account.

With the help of equations (17)–(20), and (1) (2), there follows from (21) (22)

$$\xi = \frac{n(1-\varphi)}{n-(n-1)\varphi_0} \quad . \quad . \quad . \quad (26)$$

$$F = \frac{m}{a}\,(n \max B \mp (n-1)B_0),$$

and hence the admissible stress per unit of area

$$b = \frac{\max B}{F} = \frac{1}{n-(n-1)\,\varphi_0}\,\frac{a}{m} \quad . \quad . \quad (27)$$

Here—in accordance with (26) $\frac{a}{m}$ by (16) —φ, and φ_0 are determined in the case of tension or compression only, from equation (1), and for alternate tension and compression by (2)

In particular for permanent iron structures

$$\xi = \frac{3(1-\varphi)}{3-\varphi_0} \quad . \quad . \quad . \quad . \quad (28)$$

$$b = \frac{2}{3-\varphi_0}\,\frac{a}{m} \quad . \quad . \quad . \quad . \quad (29)$$

III.—Muller's Suggestion.

Mr. Müller, a Viennese engineer, published in 1873 an essay on the "Determination of Dimensions." He starts from the assumption that every stress exceeding the limit of elasticity, that is, accompanied by a permanent alteration of form (set), must, if repeated sufficiently often, produce fracture. The primitive strength u, as the smallest stress in one sense which can practically cause fracture, is identical with the usual limit of elasticity. With smaller differences of stress, or larger values of $\frac{c}{d}$—notation as under I.—Fracture can only be brought about by the greater working stress a; hence there exist, according to the ratio $\frac{c}{d}$ an infinite number of limits of elasticity, varying from u to the statical breaking strength t.

If all values of c assumed by Wöhler be represented as abscissæ (tension positive, compression negative), and the corresponding values of a, determined experimentally, as ordinates, a curve is obtained which Müller prolongs until it intersects the c-axis, arriving by means of analogies, which, however, are not precisely defined, at the value of the primitive strength for compression, and thus completing Wöhler's data. From this curve it would be possible for every given value

$$(a) \qquad \varphi = \frac{c}{d} = \frac{B_c}{B_v},$$

to determine the working strength a, and allowing a suitable factor of safety the admissible stress b.

Müller considers a factor of safety of 3 as suitable, but intends when using the value $b = \frac{a}{3}$ to take into account the in-

* G. Müller, "Zulässige Inanspruchnahme des Schmiedeisens bei Brücken constructionen."

fluence of temperature and corrosion separately in previously determining a.

The influence of a rise of temperature is taken as equivalent to the stress which would produce the same extension.

It is stated that the influences of temperature and load are fortunately not altogether cumulative, but that each takes a separate part in the wear and tear, and "this circumstance clearly tends to reduce the absolute maximum stress with a large permanent load, because when stresses due to other causes come into action, the danger of reaching the absolute limit of fracture is increased."

In accordance with this the ratio $\beta = \frac{a}{u}$ is modified in a way not quite clear, and determined for a series of values of φ from the completed curve representing Wöhler's results, whence

$$(b) \qquad b = \tfrac{1}{3}\,\beta\,u.$$

Here Müller makes $u = 1{,}600$, and calculates accordingly two tables of admissible stresses, one for tension alone, and one for alternations of tension and compression.

It is easy to perceive that in the preceding method untenable assumptions are included. It is by no means the case that everything tends to show that a single increase of temperature "exerts exactly the same influence on a member of a bridge as a single application of a load"; but, on the contrary, experience hitherto has been opposed to this. It has been observed that at temperatures of 100° to 200° Centigrade equal and greater loads are carried than at ordinary temperatures, although both influences, which are supposed to act in the same sense, are cumulative. Neither can the choice of a primitive strength for compression, which exceeds that for tension, in the total absence of experiments in this direction, be approved.

IV.—Winkler's Method.

In the year 1877 Dr. Winkler, Professor at the Polytechnic of Berlin, published a method of determining dimensions,* which may be briefly described as follows.

If for given values $\pm\, a'$ of the numerically smaller limiting stress per unit of area as abscissas, the corresponding values of the working strength a be plotted as ordinates in accordance with Wöhler's experiments, Winkler finds that when a represents a tensile stress, the ends of the latter are grouped sufficiently closely about a straight line to justify the equation.

$$(a) \qquad a = u \pm \alpha\, a'.$$

This formula is temporarily assumed to be applicable for compression also, so that generally speaking the upper or lower sign is to be taken before a', according as the latter value represents a stress in a similar or opposite sense to a.

With a static load $a = a' = t$, therefore

$$(b) \qquad t = u + \alpha t \quad u = (1-\alpha)t.$$

$$(c) \qquad a = (1-\alpha)t \pm \alpha\, a'.$$

Assuming a factor of safety m, the admissible stress is $\frac{a}{m}$; hence if temporarily B_g represent the numerical value of the numerically smaller limiting load,

$$(d) \qquad F = \frac{m \,.\, \max B}{a} = \frac{m \,.\, B_g}{a'}.$$

From this equation there follows, with reference to the preceding,

$$(e) \qquad \max B = \frac{1-\alpha}{m} F t \pm \alpha B_g,$$

and if the static stress admissible with a factor of safety m be again denoted by $\frac{t}{m} = b_r$, then

$$(f) \qquad F = \frac{\max B \mp \alpha \,.\, B_g}{(1-\alpha) b_r},$$

here the upper or lower sign must be taken according as the smaller limiting load is of the same kind as (numerical value $B_g = \min B$) or opposite to (numerical value $B_g = \max B'$) the greater. By substitution of the values min B and max B from (17)-(20) there follows generally

$$(g)\; F = \pm \frac{B_0}{b_r} + \frac{B_1}{(1-\alpha) b_r} + \frac{\alpha B_2}{(1-\alpha) b_r}.$$

In order to take account of impact and vibrations, Winkler introduces the live load multiplied by n into the calculation, whence

$$F = \pm \frac{B_0}{b_r} + \left[\frac{B_1}{(1-\alpha) b_r} + \frac{\alpha B_2}{(1-\alpha) b_r} \right] n. \quad (30)$$

* E. Winkler, "Wahl der zulässigen Beanspruchung für Eisenconstructionen."

When max B denotes tension, Winkler makes $b_r = 1{,}400$, $a = 45$, and, after rounding off the results, obtains for structures not subject to impact and with $n=1$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{770} + \frac{B_2}{1{,}700} \quad . \; . \quad (31)$$

for railway bridges, with $n=1.3$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{590} + \frac{B_2}{1{,}300} \quad . \; . \; . \quad (32)$$

for road bridges, with $n=1.2$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{640} + \frac{B_2}{1{,}400} \quad . \; . \; . \quad (33)$$

When, however, max B denotes compression, the values should be $b_r = 1{,}200$, $a=0.40$, so that then, for structures not subject to impact with $n=1$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{720} + \frac{B_2}{1{,}800} \quad . \; . \; . \quad (34)$$

for railway bridges, with $n=1.3$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{550} + \frac{B_2}{1{,}380} \quad . \; . \; . \quad (35)$$

for road bridges, with $n=1.2$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{600} + \frac{B_2}{1{,}500} \quad . \; . \; . \quad (36)$$

In all formulas B_0 has the + or — sign, according as B_0 representing a load in the same sense as or opposite to, max B. Hence for tension only or compression only the + sign must be used.

Winkler recommends that formulas (31)—(33) should be used even when max B denotes compression, provided that, as in the case of tension, under F the nett sectional area (after deducting the rivet-holes) be understood.

If in formula (30) B_1 and B_2 are expressed by (17)—(20) in terms of the limiting loads, there follows with reference to the expressions (1) and (2),

$$b = \frac{\max B}{F} = \frac{(1-a)b_r}{n(1-a\varphi)-(n-1)(1-a)\varphi_0} \; . \; (37)$$

For railway bridges in particular there results with $n=1.3$, where max B denotes tension and $b_r = 1{,}400$, $a=0.45$,

$$b = \frac{770}{1.3-0.585\varphi-0.165\varphi_0} \quad . \; . \; . \; . \quad (38)$$

and if max B denote compression, and $b_r = 1{,}200$, $a=0.40$,

$$b = \frac{720}{1.3-0.52\varphi-0.18\,\varphi_0} \quad . \; . \; . \; . \quad (39)$$

Some objections to the preceding formulas cannot be overlooked.

For alternate tensile and compressive loads of equal magnitude, that is for $\varphi = -1$, either the tensile or compressive load may be regarded as max B, and then both formulas (32) and (35), or (38) and (39), should yield equal values. Instead of this, however, for $\varphi = -1$ and the subjoined values of B_0 and φ_0 the following results are obtained:

B_0	φ_0	by (38)	φ_0	by (39)
Compression..	-1	376	$+1$	439
" ..	$-\frac{1}{2}$	391	$+\frac{1}{2}$	416
0............	0	408	0	396
Tension	$+\frac{1}{2}$	427	$-\frac{1}{2}$	377
"	$+1$	448	-1	360

From this it appears that differences up to 17.5 per cent. of the mean values from both formulæ may occur.

That Winkler should take the admissible stress for compression without liability to buckling as smaller than for tension (1,200 as against 1,400 for b_r) is remarkable, and altogether the expression adopted for a does not appear sufficiently warranted by Wöhler's results. In order to arrive at such expressions, experiments made with the same material, and under the same conditions only should be utilized, because only in this case does the effect of variable loads undisturbed by other influences show itself.

For unhardened Krupp spring steel with $u = 500$ Centner primitive strength, and $t = 1{,}100$ Centner at least per square inch Rhenish, with which the most complete experiments were made, the following are the results obtained by different methods with initial stresses:

	$a'=$	0	250	400	600	1,100
Wöhler's experiments*	$a=$	500	700	800	900	(1,100)
Launhardt's formula	$a=$	500	711	800	900	1,100
Winkler's formula	$a=$	500	612	680	770	995

V.—Formulas of Cain Smith and Seefehlner.

In order to make the well-known empiric formulæ for liability to buckling agree with some older proposals of a committee of the American Society of Civil Engineers, Professor Cain of Charlotte, North Carolina, adopted, as representing the admissible static stress of

* Wöhler, "Die Festigkeitsversuche mit Eisen und Stahl," Berlin, 1870.

iron bars subjected to compression, the expression

$$(a)\quad b_r = \frac{1}{4+\frac{1}{10}\left(\frac{l}{d}-15\right)} \frac{t}{1-\delta\left(\frac{l}{r}\right)^2},$$

when l denotes the length of bar $r^2 = \frac{I}{F}$, the square of the least radius of gyration of the section, d the diameter of the bar at right angles to the axis for θ, t the crushing stress, δ a canstant, having for bars held fast at both ends, or supported on flat surfaces, the value 1 : 36000, for bars attached by pins at both ends 1 : 18000, and for bars held fast at one end and attached by a pin at the other 1 : 24000.

In recognizing Wöhler's experiments Cain has regard principally to lattice girders.* He assumes that the effect of impact diminishes with increasing weight of the members of a structure. That the weight of the web members increases tolerably uniformly from the middle towards the ends of the girders, equally so the ratio φ of the limiting loads on the members. Consequently it may be assumed with sufficient accuracy that the effect of impact increases proportionally with φ. Hence if for iron, by Launhardt and Weyrauch's formula $b = c\left(1+\frac{\varphi}{2}\right)$ then the empirical value to be used expressed in kilograms per square centimeter would be

For rods in tension

$$b=525\ (1+\varphi) \quad . \quad . \quad . \quad . \quad (40)$$

For members in compression

$$b= \frac{1}{4+\frac{1}{10}\frac{l}{d}} \frac{2,700}{1+\delta\left(\frac{l}{r}\right)^2}(1+\varphi) . (41)$$

for alternate tension and compression Cain simply makes $\varphi=0$.

Mr. Seefehlner, engineer of Budapest, in applying Launhardt and Weyrauch's method, wished to take separately into account the effect of impact.† In the first instance, for the ultimate working strength of iron, he sets

$$(b)\quad a=u\left(1+\frac{\varphi}{2}\right)=\frac{2t}{3}\left(1+\frac{\varphi}{2}\right).$$

"If it is desirable to include in the factor of safety m the influence of the impact of the moving load, it may be considered that the influence of the latter is greater for small bridges than for large spans, Wöhler's experiments also show that as the difference in the limiting stresses decreases the working strength increases, hence may be written

$$m=A-B\varphi,"$$

and therefore the admissible stress per unit of area in general for iron

$$b=\frac{a}{m}=\frac{2+\varphi}{A-B\varphi}\frac{t}{3} \quad . \quad . \quad . \quad (42)$$

here Seefehlner chooses the following values, t=3,600, A=4, B=1.6, consequently

$$b=\frac{2+\varphi}{4-1.6\,\varphi}\,1,200 \quad . \quad . \quad (43)$$

$$F=\frac{\max B}{b}=\frac{4-1.6\varphi}{2+\varphi}\frac{\max B}{1,200} \quad . \quad (44)$$

The imperfection of the Launhardt and Weyrauch method induced Professor Smith of Birmingham to construct a new formula based upon Wöhler's experiments.* He makes the difference of stress.

$$(c)\quad d=\frac{4(t_d+e)(t_z-e)u_z}{(2t_d+u_z)(2t_z-u_z)}=\frac{4(t_d+e)(t_z-e)u_d}{(2t_d-u_d)(2t_z+u_d)},$$

where t_z, t_d, are the statical breaking strengths, and u_z, u_d, the primitive strengths for tension and compression respectively, these all being numerical values without + or − signs; e, on the other hand, is the arithmetical mean of the limiting stresses per unit of area, where tension is to be taken as positive and compression negative. As for alternations of equal tensile and compressive loads d is equal to twice the vibration strength s, there follows from (c)

$$(d)\quad s=\frac{2\,t_z t_d u_z}{(2t_d+u_z)(2t_z-u_z)}=\frac{2\,t_d t_z u_d}{(2\,t_d-u_d)(2\,t_z+u_d)},$$

and hence generally

$$(e)\quad d=\frac{(t_d+e)(t_z-e)}{t_d t_z}\,2\,s.$$

* Cain in Van Nostrand's Eclectic Engineering Magazine, 1877.

† Seefehlner in Zeitschrift des oestreichischen, Ing. u. Arch. Vereins, 1878.

* Smith in "Engineering," 1880.

If temporarily, as the values u_d, t_d, are not known, it is assumed that $u_d = u_z = u$, $t_d = t_z = t$, there follows:

$$(f) \qquad d = \frac{4u(t^2-e^2)}{4t^2-u^2} = \frac{t^2-e^2}{t^2} 2s.$$

As to the practical application of these formulas no particulars are given.

With regard to a method of taking into account the effect of impact as desired by Gerber, Seefehlner, Schaffer and Winkler, a difference of opinion may exist on the following grounds: (1) the influence of loads which do not gradually increase is already included in Wöhler's results; (2) the effects of impact are not greatest on those members for which the live load B_v is a maximum, while for those parts which, like the rail bearers, are directly exposed to impact, the admissible stress is in any case taken as smaller than for other portions; (3) the influence of differences of stress on bridges must be less than in the case of Wöhler's experiments, in which all the straining actions followed each other very quickly, and this, even when Wöhler's results are taken into account, may be noticed by dividing the influence of the moving load by n_1; (4) according to Fairbairn, Vicat, Thurston, among others, a continuance of the stress is unfavorable, and this might lead to the practice of introducing the fixed load B_c into the calculation multiplied with the factor n_2 as compared with the live load Bv; (5) by a separate treatment of the fixed and live load, not only the determination of dimensions but also the static calculation becomes more complicated, because then B_c and the two limiting values B_v have to be reckoned separately, while otherwise only the limiting loads are used. Let it be now assumed that the influence of impact alone makes it desirable to multiply B_v by n, then by (3) and (4) there would have to be taken into calculation the quantity

$$(g) \quad B = n_2 B_c + \frac{n}{n_1} B_v = \frac{n}{n_1}\left(\frac{n_1 n_2}{n} B_c + B_v\right).$$

By introducing simply B_c and B_v into the calculation, and taking account of the effects of impact by a general factor of safety, there results $\frac{n_1 n_2}{n} = 1$, which, in the absence of further data, appears to be generally most suitable.

If, however, the influence of impact is to be accounted for in the way assumed by Gerber, Schaffer, and Winkler, namely, by introducing the limiting forces B_v B_v' resulting from the moving load, into the calculation multiplied with the factor n (tension positive, compression negative), this can in every case be done. As hypothetical limiting loads the expressions

$$(h) \qquad B = B_c + nB_v.$$

$$(i) \qquad B = B_c + nB'_v.$$

are taken, the ratio of the numerically smaller to the numerically greater of these values is denoted by φ, and then (even when liability to buckling is considered) the method of proceeding is exactly as though B and B were the actual limiting loads.* Of course, as the influence of impact is already accounted for, a more favorable factor of safety may be chosen.

Krohn's † method deviates somewhat from this, as from the commencement he reckons with 1.5 time the moving load, but then allows the value

$$(k) \qquad b = 1{,}000\left(1 + \frac{\varphi}{2}\right).$$

In the present state of knowledge on the subject, and with regard to the matter generally, excessive refinements appear to the author unsuitable.

VI.—Ritter's Hypothesis.

In a paper of the year 1877 ‡ Mr. Fr. Ritter, an engineer of Vienna, aims at placing the facts relative to the strength of materials proved by Wöhler's experiments upon a deeper foundation.

According to Ritter, the assumption is a plausible one, that the destructiveness of repeated stresses is inversely proportional to their distance from the statical breaking strength t. The resulting destructiveness of every stress varying between the limits a and $\pm$ a' may not exceed a certain value for a given material. "This value is independent of the relative position of the vibration as regards o and t, so that when the position changes, and, according to the preceding,

* For further information, *vide* Crugnola-Weyrauch, "Stabilita delle construzioni in ferro et in acciao," Turin.

† Krohn, "Resultate aus der Theorie des Brückenbaues," Aachen, 1879.

‡ Fr. Ritter, "Die Festigkeitsverhaltnisse nach den Wöhlerschen Versuchen."

each individual oscillation becomes more or less destructive, not the sum of the individual destructiveness is altered, but on the other hand the admissible limits of the resulting vibration become narrower or wider." Ritter claims to express this hypothesis by the formula

$$(a) \qquad \int_{\pm a'}^{a} \frac{t}{t-a}\, d\,a=\text{const},$$

whence when k is a constant for tension or compression only

$$(b) \qquad \frac{t-a'}{t-a}=k,$$

and for alternate tension and compression on account of the expression

$$\int_{-a_1}^{a} = \int_{o}^{a} + \int_{o}^{a'}$$

$$(c) \qquad \frac{t}{t-a} \cdot \frac{t}{t-a'}=k.$$

For a factor of safety m, the admissible stress per unit of area $b=\frac{a}{m}$ and $a=mb$. As by (1) for a piece subjected always to tension or always to compression $a'=\varphi a$, there follows from (b)

$$(d) \qquad \frac{t-m\varphi b}{t-mb}=k$$

$$b=\frac{k-1}{k-\varphi}\frac{t}{m} \quad . \quad . \quad . \quad (45)$$

As further, with alternate tension and compression, by (2), $a'=-\varphi\, a$ there results from (c)

$$(e) \qquad \frac{t}{t-mb}\ \frac{t}{t+m\varphi b}=k$$

$$b=\left\{-\frac{1-\varphi}{2\varphi}-\sqrt{\left(\frac{1-\varphi}{2\varphi}\right)^2+\frac{k-1}{k\varphi}}\right\}\frac{t}{m} (46)$$

For iron in particular Ritter makes $k=2$, $m=3$, $t=3{,}600$, whence for tension only or compression only

$$b=\frac{1{,}200}{2-\varphi} \quad . \quad . \quad . \quad (47)$$

$$F=\frac{\max B}{b}=\frac{2-\varphi}{1{,}200}\max B \quad . \quad (48)$$

and for alternate tension and compression

$$b=\frac{\varphi-1+\sqrt{1+\varphi^2}}{\varphi}600 \quad . \quad . \quad (49)$$

$$F=\frac{\varphi}{\varphi-1+\sqrt{1+\varphi^2}} \cdot \frac{\max B}{600} \quad . \quad (50)$$

For $\varphi=0$ the last equation gives $\frac{0}{0}$, then however (43) is applicable and gives $b=600$.

ON THE MEASUREMENT OF ELECTRICITY FOR COMMERCIAL PURPOSES.*

By JAMES N. SHOOLBRED, B. A., Mem. Inst. C. E.

From "The Engineer."

THE present remarks are intended to deal more especially with the commercial question, raised by what is termed the "supply of electricity," in the Electric Lighting Act, 1882, the objects of such supply being more particularly lighting for public and private purposes, and the transmission of power. All these require electric currents of such magnitude as are generally produced by dynamo machines. To such branches of the subject as telegraphy and telephony, excepting in the general principles common to electric currents, the scope of the paper, and especially the special form of the various apparatus described, will but indirectly apply. The remarks must, in fact, be taken as mainly referring to an electric supply by mechanically-generated induction currents. The basis and principle upon which rests this mode of producing electricity is the conversion of mechanical into electrical energy. Any exact financial evaluation of the commodity produced and offered for sale, involves a knowledge, by measurement, of the amount of this energy, not merely of the electrical energy as offered to the customer, but also of the mechanical which is expended and absorbed in such offer.

* Read before the Society of Telegraph Engineers and Electricians.

The former is a question mainly for the consumer, the latter for the producer, who must, however, check the two together to enable him to arrive at the fair marketable value of the supply which is offered for sale. In most cases the evaluation of the mechanical energy expended resolves itself into coal, wages, engine-room expenses, outside working and collection charges, interest on capital expended, and depreciation on plant employed, but chiefly on the two first-named items--coal and wages. It is not intended in these remarks to deal with this portion of the subject, which is one of statistics, but with the more pertinent one, to this society at least, of how to measure and value the electrical product resulting from the above expenditure. Before dismissing the former, the mechanical branch of the subject, it may be well to advert to the similarity, in most cases, and taken broadly, in the nature of the conditions of manufacture and of supply of electricity and of gas, both for illumination and for power. Each starts with coal, the energy of which it transforms into a commodity for the purposes just named; and it is in the process of transformation, in the nature and costliness of the intermediate operations, and in the amount and value of the plant and working expenses involved in each system respectively, that the main commercial difference between the two must exist. Iron forms a large part of the material of the plant necessary in both, and coal and wages, subject to the same conditions and variations, form the main bulk of the working expenses of the two systems. This similarity in objects and in operations is here referred to, in order to point out that, as it is by the careful experience of more than half a century, the conditions, both legal and social, have been ascertained for the supply of gas—and also, it should be added of water—it need not, therefore, be surprising if some of these conditions, stamped with the seal of every-day practice, should be found to adapt themselves, or to be worth while taking into consideration, in the case of electricity, the differences in the processes of production, and of the products themselves, being always remembered. The valuation of the electrical energy of the supply depends upon the exact measurement of two factors—the amount of the supply and the pressure under which it is given, or, in other words, the quantity of the current, and the difference of potential—or electromotive—force which the consumer is able to avail himself of. As there appears to exist a misconception, in some quarters, as to the exact terms in which the various electrical units have been defined, and the precise duty of each, more especially as to the "ampère" and the "coulomb"—for each of which the term "weber" was formerly and almost indiscriminately used—it may be well to give the sense of the resolutions passed at the International Congress of Electricians, held in Paris in 1881, and now universally accepted.

Electrical units.—The fundamental units adopted are those of the centimeter—length—the gramme—mass—and the second—time—or, as it is called for brevity, the C.G.S. system. For practical units the following are adopted: The ohm—10^9 C.G.S. units—as the unit of resistance, is that of a column of mercury having one square millimeter of section, and of a length hereafter to be determined by a commission specially appointed for the purpose. *Note.*—The length is supposed, however, to be between 104 and 105 centimeters. The volt—10^8 C.G.S. units of electro-motive force—as the unit of electro-motive force. *Note.*--This corresponds nearly to that of a Daniell's cell. The ampère—10^{-1} C.G.S. units of current—as the unit of current, which is the current produced by one volt through one ohm. The coulomb—10^{-1} C.G.S. units of quantity—as the unit of quantity of electricity, which is defined by the condition that an ampère yields one coulomb per second. The farad—10^{-9} C.G.S. units of capacity—as the unit of capacity, which is such that one volt in a farad shall give one coulomb. Or, to quote the words of Sir Wm. Thomson—the chairman of the British Association Committee which settled the C.G.S. system of units above referred to—when explaining the practical units to the Congress: "The volt acting through an ohm gives a current of one ampère, that is to say, one coulomb per second; and the farad is the capacity of a condenser, which holds one coulomb, when the difference of potential of its two plates is one volt." The

following were also suggested by Dr. C. W. Siemens, at the British Association Meeting, 1882, to be added to the above units: The watt—10^7 C.G.S. units of power—as the unit of power; being the power conveyed by the current of one ampere in one second through a conductor whose ends differ in potential by one volt. The joule—19^7 C.G.S. units of work—as the unit of work, or heat, being the heat generated by a watt in a second. Although the unit of quantity, as defined, is the coulomb, yet it is sometimes found more convenient to express measurements of quantity in terms of the current or rate of flow in amperes. Thus it is more convenient, sometimes, to say "one ampere hour" than "3,600 coulombs," as the result of work done by a current of one ampere flowing continually, and uniformly, for the space of one hour. The product of the current expended, in ampere seconds, or of this amount of quantity expended in coulombs, by the electrical pressure of the same, expressed in volts, gives the electrical energy expended, or power of the supply expended in watts. The electrical energy is, therefore, represented by the product of volts × amperes × time; or, by the product of volts × coulombs, or, expressed algebraically, $W = E\ C\ t = E\ Q$. The following are the equivalent expressions for the same amount of energy, only expressed in other terms, some of which may, perhaps, be more familiar:

Energy expended per second.

$$\text{Volt} \times \text{ampere} = \begin{cases} 1 \text{ watt.} \\ \frac{1}{1 \cdot 35} \text{ foot-pounds.} \\ \frac{1}{9 \cdot 81} \text{ kilogrammeters.} \\ \frac{1}{736} \text{ force cheval — French horse-power.} \\ \frac{1}{746} \text{ horse-power indicated.} \end{cases}$$

It is suggested in some of the—draft—provisional orders for the supply of electricity, now before the Board of Trade, that the unit of price to be charged should be based "on the energy contained in a current of 1,000 amperes, flowing under an electro-motive force of one volt during one hour;" or, in other words, the unit might be put as 1,000 volt-ampere-hours. Since the ampere hour is another way of saying 3,600 coulombs of quantity of electricity supplied, this above expression may be put thus, as representing the value of the above unit.

Work done.

$$\text{1000 volt-ampere-hours} = \begin{cases} \text{3,600,000 volt-coulombs.} \\ \text{1000 watt's hours.} \\ \text{1.34 horse-power hours.} \\ \text{2,653.200 foot-pounds.} \\ \text{1.36 force cheval heures— French horse-power hours nearly.} \\ \text{100 kilogrammeters.} \end{cases}$$

Put in terms more in accordance with actual practice, the above unit might mean the supply for one hour of a current of ten amperes with an electro-motive force of 100 volts. To arrive at a due evaluation of the supply of electric energy, it is evident that the measurement of each of these two factors—in volts and amperes respectively—should be effected either separately or combined; and also that a continuous and cumulative record should be kept of the supply as it proceeds. Many instruments exist for the measurement of the above elements at any particular time, but without any means of continuously recording such measurements. These evidently, except under certain conditions, cannot comply with the commercial conditions required for ascertaining the amount of supply. It is only, therefore, instruments furnished with means of continuously integrating, or recording the successive progressions of the supply, or instruments to which such recording apparatus can readily be attached, that come properly within the scope of this paper. To measure with completeness for commercial purposes a supply of electricity will entail, therefore, a continuous record of each of the two elements just referred to, current and pressure, either separately or combined. In the supply of towns, however, the question for the consumer, may, and will most probably, be much simplified by causing one of these elements, that of pressure, to remain constant, since it is very likely that a constant standard pressure of supply will be fixed by the Government in granting the several provisional orders. If so, it then becomes the duty of the suppliers to keep up to that pressure under penalty; and instruments for recording such pressure will have to be installed where required, and placed under proper supervision. For the customer, however,

it will then generally suffice to have an exact record of the quantity only of his individual consumption of electric supply. It has been thought advisable to precede the description of the recording or registering instruments, which alone are meters in the commonly-accepted sense of the word, by an enumeration of some of those non-recording instruments which are in more general use, since they suffice for present exigencies. This short descriptive enumeration is even almost necessary, since most of these instruments, by the addition of some recording apparatus or appliance by which the element of time can be integrated, may be made to enter into the class of registering meters. Indeed, some of them already possess their representative in this second class, or else have given rise to some modification, which has complied with the requirements in the latter case. Thus, any current or ampère measurer may be converted into a record of quantity, or a coulomb meter, by the integration of the time during which the current has flowed; and similarly, any power or volt ampere measurer may become a register of work done by means of the addition of the elements of time. Again, volt or pressure measurers, will always be required in any case where a check is required upon the actual difference of potentials, or electro-motive force of the supply, and this may arise from a variety of causes.

Non-registering instruments.—Current meters: Siemens' electro dynamometer;* Professors Ayrton and Perry, ammeter;* Sir Wm. Thomson, current galvanometer; Capt. Cardew, R.E., low resistance galvanometer;* Dr. Obach, tangent galvanometer;* Marcel Deprez, galvanometer. Pressure meters: Professors Ayrton and Perry, volt-meter; Sir Wm. Thomson, potential galvanometer; Siemens' torsion galvanometer; Marcel Deprez, galvanometer. Power, or energy, meters: Professors Ayrton and Perry, power meter; C. V. Boys, power meter.

Registering meters.—Quantity, or coulomb, meters: (1) Electrolytic: T. A. Edison, total deposition; T. A. Edison, alternations of deposition; J. T. Spague, alternations of deposition. (2) Mechanical: Dr. J. Hopkinson, rotating meter; C. V. Boys, vibrating meter; Theos. Varley and Greenwood, disc meter; Professors Ayrton and Perry, fluid friction on magneto-electro motor;* T. A. Edison, fluid friction on magneto-electric motor.† Energy, or work, meters: Professors Ayrton and Perry, erg-meter; C. V. Boys, energy meter; Deprez-Abdank, energy meter.

Edison's current meters.—These are based upon electro-deposition of metal, due to the action of a known fractional part of the total current. The weight of the increments is ascertained periodically, and from it the total quantity of the current which has passed during the interval is deduced. The metal now generally used consists of plates of zinc, immersed in a solution of ninety parts of sulphate of zinc and one hundred parts of pure water. In the form of meter for commercial use, two cells are placed as a check against one another; one, termed the "monthly cell," receiving four times the current of the other, which is known as the "quarterly cell." In order to prevent the temperature of the liquid in the cells falling so low as to freeze, a connection is made by means of a long thin strip of brass and steel rivetted together to an incandescent lamp, which is thereby lighted and raises the temperature as required. It is only when the temperature falls to 42° Fah. that this tongue is sufficiently depressed to form contact, and so to light the lamp. On the temperature rising the tongue rises, and the lamp is extinguished. Experience shows that electro-deposition, to give a true and reliable record, should not be forced or overworked in its action; and that the plates should not in their daily duty be required to do more work, or be longer in action than they are intended for by their superficial area. In practice, about 75 per cent. only of their nominal work should be required of them. Several other forms of meter have been devised by Edison, such as his beam meter, where, when the increments by electro-deposition have accumulated to a certain limit, the current is reversed and the accumulation is re-dissolved, to re-commence again when the normal condition is gained; and, again, electric-motor meters, by fluid pressure, etc. All of these

* See "Journal" Soc. Tel. Engrs.—No. 43, vol. xi. Sept. 1882.

† See "Journal" Soc. Tel. Engrs., Sept. 1882.

have, it is said, in practice been found—in cases where the total current supplied is very small, and this often so—to require so large a proportion of it for these mechanical operations, as to make the record unreliable. The meter first described, by electrolytic action only, is therefore now generally adopted by Edison.

Sprague's meters.—These instruments are based upon electro-deposit up to a certain point; *i. e.*, when the intended quantity of metal, whether copper or zinc, has been deposited on the plate. The current is then reversed, and the metal gradually dissolved again until the primary condition of the plate is reached; when, by another reversal of the current, deposition again commences; each reversal of the current being recorded by a mechanical counter and a train and wheels. Not much practical experience has so far been obtained with these meters; but, what has been done, tends to point out that the mechanical operations involved in the reversals of the current, and in their registration, absorb a large amount of power.

Hopkinson's current meter.—This instrument consists of a thick wire coil, in the form of a solenoid, through which the current passes to be measured. The iron core of this solenoid revolves with its central shaft by the action of the armature of a small dynamo machine, placed at one end of the shaft. The core of the solenoid is in two parts; the lower is fixed to the shaft, while the upper is movable, being attached to a governor ball arrangement, and sliding up and down the shaft in accordance with the variations in the rotation speed of the shaft. A shunt current passes through the dynamo and its armature, then up through the lower or fixed portion of the core, and—by contact only—to the sliding part, and thence to the framework of the apparatus. If the movable core be lifted, owing to the speed of rotation by the action of the governor balls, this circuit is broken, and the shunt current through the dynamo interrupted. Whenever a current to be measured passes through the coil, attraction, by means of its casing, takes place between the fixed and the movable parts of the iron core. This magnetic action, which is proportional to the square of the current, tends to keep the two parts of the core together and in contact, while the centrifugal force of the governor balls, which is proportional to the square of the speed of revolution, tends to break the contact by lifting the movable part. These opposite forces will, in working, balance one another, and the result is that the system revolves with a velocity proportional to the current through the coil. As the revolutions of the shaft are transmitted continuously by a train of wheels to a set of index dials, a record is thus kept of the quantity of the current that has passed. In the construction of the apparatus the weight of the core is taken off by springs. The arrangement of the parts, as now made, is shown on the diagram.

Boys' quantity or vibrating meter.—This instrument is based upon two well-known principles. (1) The force acting on the armature of an electro-magnet, in any position, is proportional to the square of the current. (2) The square of the number of vibrations, say, of a pendulum, is a measure of the controlling force. Therefore, if the controlling force under which a body vibrates is due to the action of an electro-magnet on its armature, the square of the number of vibrations in a given time is a measure of the square of the electric current. In other words, the rate of vibrating is a measure of the strength of the cuarent, and the number of vibrations is a measure of its quantity. The exact form and nature of the meter may vary in many details. The one now shown consists, primarily, of an electro-magnet—the upper one in the diagram—through the coils of which passes a portion of the main current to be measured. This magnet is placed horizontally, and a vertical rocking shaft stands between its poles. This shaft has fixed on it a soft iron armature, rounded at the ends, and free to move in the horizontal plane between the poles of the electro-magnet. The intensity of the attraction between the poles and this armature determines the rate of vibration; which, as above stated, is a measure of the strength of the current. Each vibration is itself recorded by means of an escapement, a train of wheels, and a set of index dials; and the number of vibrations thus registered becomes a measure of the quantity of the current. To add to the momen-

tum of the vibrating body, two long arms, weighted at the end, are attached to the lower part of the vertical shaft. In order, likewise, to prevent the vibrating armature from gradually coming to rest, it is arranged that, when the vibrations fall below a certain limit, by making contact, a portion of the current is sent round the coils of a second or "impulse" electro-magnet—placed underneath the "controlling" magnet—and which has an armature of a suitable form fixed on to the same shaft that carries the armature of the upper magnet. The extra motion thus given to the shaft, by the attraction of the lower armature, affords the necessary impulse to the vibrating armature when required to do so.

Boys' energy meter.—This instrument consists of two parts: (1) the indicator of energy, and (2) the integrating apparatus. (1) In the indicator of energy, a balanced beam has from one end suspended a counterweight, and from the other a hollow solenoid, free to work up and down into two other fixed solenoids. The movable solenoid is wound with a considerable length of fine wire; in the upper half in one direction, in the lower in the opposite—this is to render it independent of any magnet which may be placed near to it. This solenoid constitutes the high resistance shunt which measures the electro-motive force. The two fixed solenoids are wound with thick wire, and convey the main current. The result of the action of the fixed and the movable solenoids on each other is a force proportional to the product of the two currents, that is the energy being expended, but the external evidence of this is the inclination of the beam, and this inclination, or rather the tangent of the inclination, is proportional to the energy being expended. (2) The recording apparatus consists of a cylinder, which by means of a mangle motion is made to reciprocate backwards and forwards by clockwork, and during its passage in each direction the cylinder is made to bear alternately against one of two tangent wheels, each free to be inclined in its direction of travel; both are fixed on the same swivelling frame, but only one of them bears at the same time against the cylinder. This frame is free to be inclined from the vertical in correspondence with the inclinations of the beam above mentioned; but the tangent of the inclination of the beam, as has been said, is proportional to the energy of the current; so also, therefore, is the tangent of the inclination of the wheels $\left(i.e., \frac{dy}{dx}\right)$. The effect of this inclination of the tangent wheel is to cause the reciprocating cylinder to rotate, the speed of such rotation being proportional to the tangent of the inclination of the wheel, which is likewise proportional to the tangent of the inclination of the beam; that is, to the amount of energy expended. The path of the tangent wheel on the reciprocating cylinder, when not inclined, is simply a straight line lengthways along the cylinder, and no rotation is caused, but when, owing to the inclination of the wheel, the cylinder rotates, the wheel pathos becomes a spiral. The rotations of the cylinder are transmitted to a train of wheels and registered, thus giving a record of the amount of energy expended during a given time.

Ayrton and Perry's erg-meter.—This instrument is but a further development or sequel to their power meter, by the addition of apparatus which integrate and record continuously the time during which the electrical energy has been imparted, as well as the variations in its amount. By this means is preserved a record of the entire work done, or of the total electrical energy supplied. As in the power meter, two coils are here made use of, a thick wire one on the main circuit, to measure the amount of current, and a thin wire one on a shunt joining the ends of the main circuit, to measure the difference of potentials or electro-motive force of the main circuit. In the arrangement, as now shown, the thin wire coil, of say one 1,000 ohms resistance, simply replaces the pendulum bob of a clock. The wires from each end of the coils pass up the sides of the pendulum rod and on to the binding screws A and B, which can be joined to the supply and return cables of a house, or machine, or a system receiving electrical energy. In the immediate vicinity of the fine wire coil, fixed to the clock case, and parallel with the plane of the pendulum path, is fixed the thick wire coil, which forms part of the main circuit, and has a very

small resistance. The effect upon the thin wire coil of its repeated passages in front of the thick wire coil is to cause a certain pull or attraction upon its motion —either of acceleration or of retardation, according to the direction of the coiling. This acting, in addition to the ordinary action of gravity upon the pendulum, will keep constantly adding to, or retarding, its rate of motion in proportion to the electrical power of the circuit. This pull is the product of the magnetic moments of the two coils, and therefore is proportional to the product of the current and the electro-motive force. The effects of these repeated accelerations or retardations upon the progress of the clock keep constantly accumulating, and their total amount can at any time be detected and ascertained by observing the amount of loss or gain which the clock has experienced. As the rate of loss or gain in the clock due to different amounts of electrical power has been previously ascertained, this knowledge of the total retardation or acceleration upon the clock is, in fact, a record of the total amount of electricity energy which has been expended, or of the work done, since the last observation of the clock. The principle upon which the erg-meter is based is as follows: Let B C be the thick wire coil, and A B be the thin wire coil. If c be the current passing into the house and the difference of potential of supply and return cable, then $c\ v$ is proportional to the electric horse-power given to the house. Now the time of vibration of the pendulum is $t=\pi\sqrt{\frac{M}{g-kcv}}$. If the magnetic force is a retarding one M, g, v π are constants—that is, if the time of clock, when no electricity is passing, be called T, hen $\frac{T}{t}=\sqrt{\frac{g-kcv}{g}}$. Now, care is taken that the magnetic forces are very small in comparison with gravitation forces acting on the pendulum bob; so that, to a degree of approximation which is sufficiently correct in practice, the above equation becomes $\frac{T}{t}=1-\frac{k}{2g}cv$. The rate of loss in the clock is therefore represented by $\frac{k}{2g}cv$. That is, it is proportional to $c\,v_1$ the electrical power. Hence, the total loss of the clock during any time represents the total electrical energy given to the house during that time. For the reasons stated in the earlier part of the paper, it is probable that the supply of electricity in a town will be carried out at a certain defined standard pressure of electro-motive force, which will be guaranteed to the consumer; and to assure the fulfillment of this condition, special steps will be taken by the local or other authority. The consumer need therefore but concern himself, as far as the supply he receives and has to pay for, with the quantity of electricity which he has made use of. In fact, the meter he makes use of to record it may be a coulomb meter. The work or energy meters will not be needed for general application, but only in special cases, where the complete record of the total electrical energy supplied is required. In what form, therefore, will it be most convenient for these coulomb meters to present their record? or, in other words, upon what unit should that register be based for commercial calculation? With the sole exception of those meters based upon electrolytic action, and which simply present the total of the increments of the metal, which are proportionate to the total quantity of current which is passed, the record presented is the result of a series of mechanical actions indicative of the strength and duration of the electrical supply. It is seen how these successive mechanical operations may be recorded in a cumulative way by the ordinary arrangement of a train of wheels; both in the case of those meters based upon electrolytic action, and causing periodical reversals in the direction of the current, and also in those where the action due to the current is a purely mechanical one. There remains then but the question of the mode of graduation of the respective indicating dials. The unit already referred to, as being proposed to the Board of Trade in several provisional orders as "the energy in a current of 1000 ampères with an electromotive force of one volt flowing during one hour," has been shown to mean 1000 volt-ampere-hours, or 3,600,000 volt-coulombs. As it is probable that one of these factors—the volt—being fixed and constant, may be taken out of the commercial calculation for the consumer, it

is worth while considering what dimensions the remaining factor—the coulomb—would assume under the ordinary conditions of practice; and hence what would be most convenient graduation of the dials to embrace these dimensions; but the commercial question requires also a financial expression in which to reckon these dimensions. The money value accompanying the suggestion of the above 1000 volt-ampere-hours unit, and which is sought to be attached primarily to 100 of these units is in some provisional orders 70s., in others 75s. Though it would be quite out of place in this paper to attempt to assign any financial value for the supply of electricity, yet for the nonce, and in carrying out the analysis of the above unit, as it might work under the conditions of actual practice, it is necessary to assume a money value. In the annexed table, which takes into consideration the effect of different rates of electro-motive force, according as they may be fixed, 70s. is assumed as the money for 100 units of 1000 volt-ampere-hours each.

Comparative Values of Supply of Electricity per 100 *units* (*of* 1000 *V.-A.-hours*) = £3 10*s.*

(1000 volt-ampere-hours=3,600,000 volt-coulombs.)

E.M.F. Volts.	Price.			
	100 Coulombs	10,000 Coulombs	100,000 Coulombs	1,000,000 Coulombs
	Pence.	Pence.	s. d.	£ s. d.
100	.23	2.33	1 11.3	0 10 5
110	.26	2-57	2 1.7	1 1 5
120	.28	2.8	2 4	1 3 4
130	.3	3.03	2 6.3	1 5 3
140	.32	3.27	2 8.7	1 7 3
150	.35	3.5	2 11	1 9 2
160	.37	3.73	3 1.3	1 11 1
170	.4	3.97	3 0.7	1 13 1
180	.42	4.2	3 6	1 15 0
190	.44	4.43	3 8.3	1 16 11
200	.46	4.67	3 10.7	1 18 11

The prices in this table are based on the assumption that the price per coulomb unit remains at the same rate, irrespective of the intensity of the electro-motive force. As a matter of fact, the amount of the latter ought certainly to be taken into account in fixing the sale price. Thus, with a pressure at 200 volts, the price for a certain number of coulombs ought not to exceed two-thirds of that of the same supply at 100 volts of pressure, in consequence of the economy in the former case, in the distributing mains, and in general working expenses. From a careful consideration of this table, it would appear that the amount of 100,000 coulombs would form a convenient unit of value whereby to reckon the commercial price. It would also serve as a basis of graduation for the indicating dials of the meters, with subdivision to $\frac{1}{10}$ and $\frac{1}{100}$ parts. The annexed diagram shows an arrangement of indicating dials, graduated in the first case with the unit suggested in the provisional orders, 1,000 ampere hours; in the second the unit is 100,000 coulombs as just suggested. The electro-motive force in each is taken at 100 volts. The proportion between the two systems of graduating in the present instance is as 3.6 to 10—3,600 being the number of coulombs which is the result of the flow of a current of one ampere for one hour; $\frac{3{,}600{,}000 \text{ coulombs}}{100 \text{ volts}}$ is, therefore, the actual quantity value of the first unit—1,000 ampere hours; as against 100,000 coulombs, the quantity contained in the second unit.

The values in C. G. S. units are $\left\{ \begin{array}{l} \text{Volt-ampere-hour unit} = 10^{10}. \\ \text{Coulomb unit } 10^{4}. \end{array} \right.$

The advantage of the coulomb unit is that it expresses an actual quantity, whereas the ampere hour unit is but a time-rate, which as a matter of fact, has to be translated into its corresponding quantity. The coulomb method of graduation could be applied uniformly to all quantity meters, whereon they might happen to be afterwards used, inasmuch as the special rate of electro-motive force, or standard pressure, in each particular locality would be taken into account in the price there charged for the electric supply. This uniformity in manufacture would further reduce the expense of this, already the simpler form of registering meter. With the volt-ampere method, to use a quantity meter—as shown in the index-dial diagram—requires the exact position of the unit to be set according to the particular electro-motive force

with which it is used, inasmuch as the quantity record is only one factor in the unit. This limited use of each particular recording meter would tend to create error and confusion. An energy meter alone would supply a complete record of the volt-ampere-hour mode of measurement; and this is a form of meter which involves a clock or some other time record, and consequently is a more expensive apparatus, and one requiring an amount of attention from the consumer which it would be impossible, in many cases, to expect. Thus, though the volt-ampere system may present, theoretically, a complete and convenient means of recording the supply of electricity, yet, in actual practice, the coulomb system would meet all wants, and be more suited to general application.

ON THE EFFECT OF THE EARTH'S ROTATION ON BODIES MOVING ON ITS SURFACE.

BY PROF. J. E. HENDRICKS.

THAT any meridian plane of a revolving sphere will rotate, with respect to a fixed plane, around a normal axis, which pierces the sphere at any point of its surface, with a uniform angular velocity equal to the angular velocity of the revolving sphere multiplied by the sine of the latitude of the point at which the normal axis pierces the surface of the sphere, is a geometrical truth which is wholly independent of any consideration of force whatever. (For a demonstration of this proposition see *American Journal of Science for* 1852, Vol. XIII, p. 212.)

As a consequence of the inertia of matter, a mass in motion, constrained to move about a center, by a constant force directed to that center, will develop a centrifugal force which will be directly proportional to the square of the velocity and inversely proportional to the radius of the circle.

If we pnt R for the earth's equatorial radius and V for its equatorial velocity per hour (sidereal time), we have for its equatorial centrifugal force

$$\frac{V^2}{R}=\frac{g}{289};\ V=\frac{2\pi R}{24} \quad . \quad . \quad . \quad (1)$$

If a velocity v be impressed upon a body, at any point P in latitude λ of the earth's surface, the body will, under the impulse of the impressed force and the earth's gravitating force, move in a fixed plane passing through the center of the earth and therefore intersecting its surface in a great circle of which the point P will be the extremity of a diameter.

Now, in accordance with the above proposition, a meridian plane will, in consequence of the earth's rotation, revolve, with respect to the fixed plane through P, about a normal axis at P, with a uniform angular velocity.

Hence, instead of supposing the moving body to remain in the fixed plane, if we suppose it to be deflected from its path by the rotation of a meridian plane around the normal at P, it will be, at every point of its path, forced to move with a uniform velocity at right angles to a tangent at that point, and hence will be compelled to describe a circle, and will by the above proposition, return to P in the time occupied by the meridian plane in making one rotation about the normal at P, viz., 24 hours divided by sin λ; therefore the circumference of the circle in which the body is forced to move is $24v \div \sin\lambda$, and its radius is $24v \div 2\pi \sin\lambda$, and because the deflecting or centripetal force f acting on the body is to the earth's equatorial centrifugal force directly as the squares of their velocities and inversely as their radii;

$$\therefore f : \tfrac{1}{289}g :: \frac{v^2}{24v \div 2\pi \sin\lambda} : \frac{V^2}{R};$$

$$\text{or } f : \tfrac{1}{289}g :: \frac{2\pi v \sin\lambda}{24} : \frac{4\pi^2 R}{(24)^2};\ \text{whence}$$

$$f=\frac{\tfrac{1}{289}g\,24v\sin\lambda}{2\pi R} \quad . \quad . \quad . \quad (2)$$

Equation (2) represents the total deflecting force at P which results from the rotation of the tangent plane around the normal, and is entirely independent of the direction in which the body moves, provided the centrifugal force of the moving body corresponds with that of

the earth's surface at the point P; but this is only the case when the initial motion of the body is in the meridian of P.

Let β denote the angle between the meridian plane and the initial direction in which the body moves, estimated from the south toward the west, then is the centrifugal force of the earth's surface at P to the centrifugal force in the same orbit due to the velocity v as $V^2 \cos^2 \lambda$ is to $v^2 \sin^2 \beta$ or

$$\tfrac{1}{289} g \cos \lambda : F :: V^2 \cos^2\lambda : v^2 \sin^2\beta,$$

from which we find

$$F = \frac{\frac{1}{289} g v^2 \sin^2\beta \cos\lambda}{V^2 \cos^2\lambda} = \frac{\frac{1}{289} g v^2 \sin^2\beta}{V^2 \cos\lambda}.$$

Resolving F into horizontal and vertical components at P, we find for its horizontal component

$$F' = \frac{\frac{1}{289} g (24)^2 v^2 \sin\lambda \sin^2\beta}{4\pi R^2 \cos\lambda} = \frac{24 v \sin^2\beta}{2\pi R \cos\lambda} \times \frac{\frac{1}{289} g 24 v \sin\lambda}{2\pi R} \quad . \quad . \quad . \quad (3)$$

Adding equations (2) and (3) we get for the total deflecting force at P

$$f + F' = \left(1 + \frac{24 v \sin^2\beta}{2\pi R \cos\lambda}\right) \times \frac{\frac{1}{289} g 24 v \sin\lambda}{2\pi R} \quad . (4)$$

If we suppose the point P to be in latitude 87° 45′ and v to represent a westward velocity of 40 miles per hour, the body though moving at the rate of 40 miles per hour relative to the earth's surface, will be in a state of absolute rest, and will therefore develop no centrifugal force about the earth's axis, and consequently it will tend to a state of equilibrium by moving toward the pole, or to the right, with a force represented by the earth's centrifugal force at that latitude resolved in the direction of the tangent plane, *i. e.*, with a force $= \frac{1}{289} g \cos \lambda \sin \lambda$; this result is obtained independently of any consideration of the rotation of the meridian about a normal axis at P, and from which there results precisely the same deflecting force at that point, as shown by equation (2). Hence equation (4) is true when $v = V \cos \lambda$, and is therefore true for all values of v and β.

If the moving body be a pendulum suspended at P in latitnde λ, its centrifugal force about the earth's axis will correspond with that of the point P, and v in equation (3) will become zero, so that in whatever direction a pendulum may be vibrated, its deflecting force will be completely represented by f in equation (2); for the centrifugal force produced by the motion of the pendulum about its point of suspension is always in the plane of that motion and therefore has no tendency to deflect the pendulum out of that plane; but the velocity v' with which the pendulum passes the point P, if referred to the earth's axis as its center of motion, may be substituted for v in equation (2) and hence f in equation (2) represents the whole of the deflecting force, or reaction, which a pendulum vibrating with a maximum velocity v' would exert on the rotating meridian through P.

As the term F′, equation (3), results from the difference between the centrifugal force of the moving body and that of the point P, about the earth's axis, if the moving body be a projectile this term will vanish, but in its stead we shall then have the term

$$F'' = \frac{v \cos\lambda \cos\beta}{\sin\lambda} \times \frac{\frac{1}{289} g 24 v \sin\lambda}{2\pi R}, \quad . (5)$$

so that, at the end of any time t, the deflecting force will be

$$f + F'' = \left(1 + \frac{v \cos\lambda \cos\beta}{\sin\lambda}\right) \times \frac{\frac{1}{289} g 24 v \sin\lambda}{2\pi R} \quad (6)$$

Because the second term in the parentheses in equation (4) vanishes when $\sin \beta = 0$ and the corresponding term in equation (6) vanishes when $\cos \beta = 0$, it follows that, for a projectile, the deflecting force is a maximum when the direction of the motion is in the plane of the meridian, while for a body moving on the earth's surface, as a locomotive on a straight track, the deflecting force is a maximum when the track is at right angles with the meridian through the origin of the motion.

In a paper on this subject published in *Van Nostrand's Engineering Magazine* for June, 1883, p. 468, Mr. Randolph says truly that, "While the earth was in a sufficiently fluid state its materials adjusted themselves into the form of an oblate spheriod . . . which is exactly that necessary to counteract the tendency of a detached mass to move on such surface by virtue of the centrifugal force"; but he fails to note that the centrifugal force we have to deal with is not $V \cos \lambda$, to which the earth's figure is adapted, but $V^2 \cos^2 \lambda \pm v^2 \sin^2 \beta$, the substitution of which introduces the term F′ as above.

THE TWO-CYLINDER COMPOUND ENGINE IN WHICH THE STROKES ARE SIMULTANEOUS, OR CO-INITIAL AND CO-TERMINAL, WITH RECEIVER, CUSHION, CLEARANCE, ETC.

By S. W. ROBINSON, C. E., Prof. Mech. Eng., Ohio State University, Columbus, Ohio.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

PROBABLY the simplest form for this engine is that one much in use for pumping engines, and illustrated in the accompanying cut, Fig. 1, of a Worthington pumping engine. A is the high-pressure cylinder, B the low-pressure cylinder, and C the condenser.

Steam is admitted directly to A from the boiler; from A it is exhausted through the pipe shown partly obscured by the steam chests, directly to the large cylinder B, and from thence it is taken to the condenser.

The piston rods of A and B are both connected with one cross-head. When the pistons are both moving toward the left, high steam enters at the right-hand side of the piston A, while from the left side the steam passes freely through the connecting pipe to the right hand side of the piston B, the left side of B being open to the condenser.

It is evident that the pump might be removed and a crank and pitman substituted. The engine then becomes "rotative," that is, has a rotating shaft.

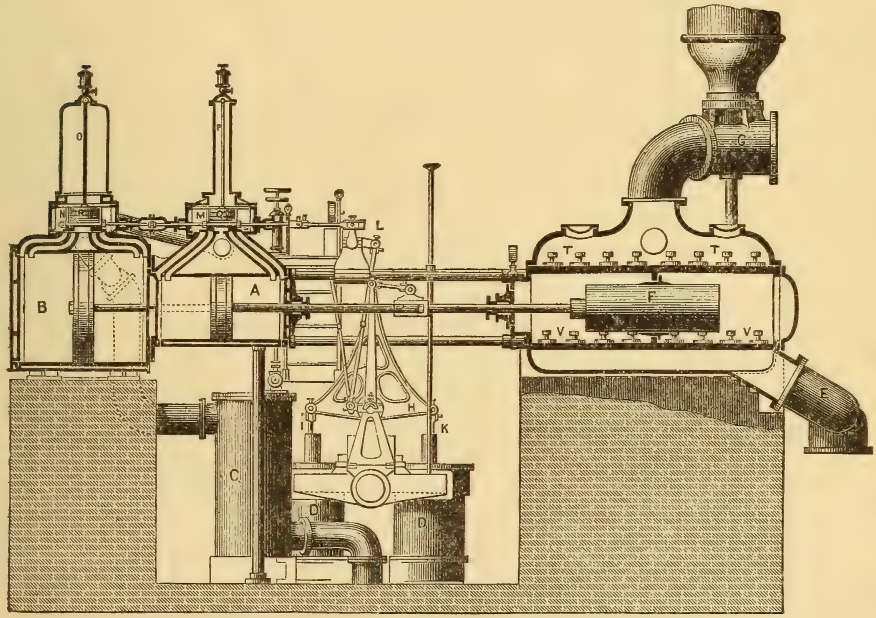


FIG. 1.—A WOOLF COMPOUND ENGINE WITHOUT INTERMEDIATE RECEIVER. STROKES SIMULTANEOUS

It is not essential that the pistons both move in the same direction at the same instant in a rotative Woolf engine, as would be the case in Fig. 1 made rotative; but one cylinder might be connected up with one crank, and the other cylinder with a second crank on the same shaft placed at 180° with the first crank. The pistons would then move simultaneously in opposite directions with strokes co-

initial and co-terminal in points of time.

Or instead of placing the cylinders both on one side of the shaft, they might be placed on diametrically opposite sides of the shaft. Then both could be connected to the same crank, or two cranks at 180° could be put one to each cylinder.

Again, the cylinders could be arranged at quartering points and the cranks likewise.

In the study of any form or type of engine, the correct form of the diagram of the action of the steam in its passage through the cylinders is important; it must precede formulas, because formulas cannot be established until the steam action is perfectly understood. We will now attempt to trace the diagram for any form of the Woolf engine.

In Fig. 1, of Turnbull's Treatise, we have a theoretical diagram of a simple, one-cylinder, expansive condensing en-

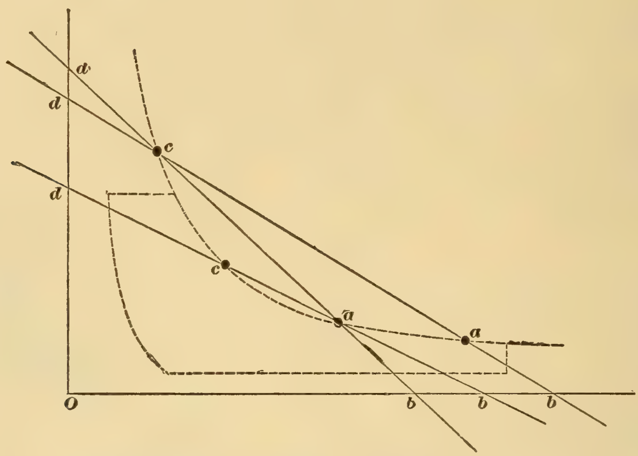


FIG. 2.—TO DRAW THE EXPANSION CURVE.

For the cylinders both at one side, many ingenious combinations of cylinders have been devised, some being of the trunk engine form, etc.

One arrangement is theoretically as economical as another, except for considerations of waste heat, and for large intermediate communicating pipes acting like receivers for remote cylinders. The best arrangement for a minimum of intermediate passage is where the cylinders are placed side by side, separated only by valves and acting on cranks at 180°. Then one piston moves opposite to the other and the steam passes over direct through short passages.

When the pistons are connected to cranks in such manner that the strokes are not co-initial and co-terminal, a considerable volume is required in an intermediate receiver. The engine then becomes the Wolff engine, the special considerations relative to which will not be entered upon here, but will be found treated in *Science Series*, No. 10.

gine. The line AB is the axis of pressures or the line of zero volumes, and AE the axis of volumes or the line of zero pressures. To hereafter express this, we will only speak of the point A as the zero of pressures and volumes. On the supposition that the steam expands, according to the law of Mariotte, the expansion curve CD is a common hyperbola, such that the product of a pressure by its corresponding volume is equal to the like product for any other point.

A very convenient way for tracing this curve on a drawing board is as shown in Fig. 2. Take O as the zero of volumes and pressures. Then, suppose a to be any point in the expansion curve. Draw any straight line $abcd$, and make $cd=ab$. Then c is a point in the curve required. Then draw a second line through c and make $ab=cd$, and a is a second point in the curve. Any number of lines may be similarly drawn to find a like number of points. Thus, when any

one point is given, as, for instance, C or D, Fig. 1, of Turnbull, any other points may be easily located through which to trace the curve CD. See also *American Machinist*, Dec. 16, '82; Wm. Lee Church.

But the actual steam expansion curve differs from Fig. 1, of Turnbull. It is apt to be steeper at the position near C, and flatter toward D. This is explained partly on the ground of partial condensation in the cylinder at the initial expansion, and of re-evaporation near the end of the expansion. In the average actual indicator diagram an hyperbola through C will very nearly strike D, for which reason this curve is mostly used in the examination of diagrams traced by the indicator. The position of the actual curve, however, will vary much with circumstances, and usually the higher the speed the lower will be the point D for a given point C. Expansion lines can therefore be drawn with a degree of approximation with the simple method of tracing given in Fig. 2.

Fig. 2 of Turnbull represents a theoretical indicator diagram for a Woolf compound engine. The part ABCDA is for the high-pressure cylinder; while the part ADEFA is for the low-pressure cylinder. The latter is very much smaller than the other; accounted for by the fact that the pressure is much lower in the low-pressure cylinder than in the other, while the length is made the same by the indicator. In studying these diagrams it is convenient to increase the length of the low-pressure diagram so that it is as many times longer than the high-pressure diagram as the low-pressure cylinder is larger in volume than the high-pressure cylinder.

Then if the areas of the high and low pressure diagrams are equal, the work developed by one cylinder equals that by the other.

Combined Diagrams.

To thus enlarge the low-pressure diagram and put the resulting combined diagram into the best shape for study, horizontal lines should be drawn through the diagram ADEF and increased in length by the ratio of the cylinder volumes as was first done by Rankin and as indicated in Fig. 3. Take AE = the volume of the high-pressure cylinder, and AF = volume of the low-pressure cylinder. Then, as in Fig. 2, of Turnbull, BCDH is the indicator diagram of the high-pressure cylinder, and AHDE of the low-pressure cylinder as taken by the indicator itself from the engine. Draw LKJ and make LJ = LK × ratio of cylinder volumes. Then J is a point in the new curve; any number of points in DJG may be thus found. Then BCDJGFA is the combined diagram, and the work developed per stroke of engine will be represented by it, also the work developed in each cylinder will be represented by its respective part of the area BDCH or

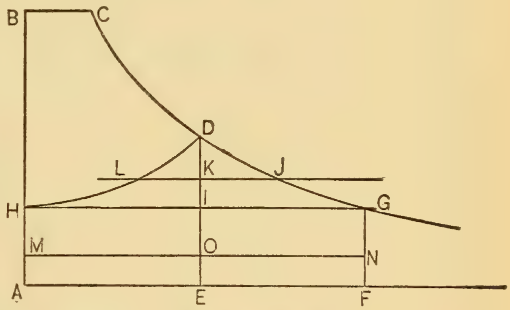


Fig. 3.—combination of diagrams.

AHDGF. The area AHDGF is equal to the area AHDE × ratio of volumes of cylinders as appears from the fact that in the transformation of this diagram the horizontal dimensions only are altered, and these in the ratio of cylinder volumes. This diagram supposes no clearance, no dead space between the cylinders, no cushioning, and no resistance to the motion of the motor fluid.

I.—Diagram of a Compound Engine without Receiver or Clearance, but with High-Pressure Cut-Off.

The action of the steam in passing through the engine whose diagram is presented in Fig. 3, may be thus traced, viz., BC is the admission line of steam at boiler pressure. At C the admission is arrested by a proper cut-off, when expansion goes on till the end of the stroke AE is reached for the high-pressure cylinder. The expansion curve CD has the point A for the zero of volumes and pressures; that is, for hyperbolic expansion the curve CD is to be constructed by aid of the lines AB and AF as explained in Fig. 2 from the lines A*b* and A*d*.

At the end of the stroke of the high-pressure cylinder the volume of steam admitted has become AE the volume of the high-pressure cylinder. Now a valve opens a passageway between the cylinders so as to discharge this steam from the full high-pressure cylinder into that end of the low-pressure cylinder at which the stroke is just beginning; but as there is no clearance, &c., this opening of the valve makes no change in the total volume of steam, the volume being still AE. Now the pistons are simultaneously to move and make their strokes, the steam to flow from the high-pressure cylinder into the low-pressure cylinder. In this act the volume enlarges from that of the high to that of the low-pressure cylinder; that is, from AE to AF, Fig. 3, and the pressure of this expanding steam is the same at any instant in the two cylinders it being a back-pressure in the high and a forward-pressure in the low-pressure cylinder. Taking DG as the expansion line for this change of volume, AE to AF, we would have some line DLH of back-pressure for the high-pressure cylinder. This line begins and ends at the same height as the expansion line DG, in fact the lines throughout are at the same height at corresponding points L and J. At these points KL is the part of the stroke made by the high-pressure cylinder, LJ the part of stroke of the low-pressure cylinder, while JK is the corresponding increase of volume of the steam. The line MN may represent the back-pressure line of the low-pressure cylinder it being the line of pressure of the atmosphere or of the condenser.

Now in the expansion line DG the steam expands from the volume AE to the volume AF, just as though the first or high-pressure cylinder had been long enough to have carried the expansion to that extent. From this it appears that the curve DG is simply the continuation of the curve CD, all drawn from A as the zero of volumes.

From this there appears to be no gain from compounding, because the diagram is the same as though the admission and expansion had all taken place in the low-pressure cylinder, itself and alone. The advantage in compounding is to be found outside of the diagram and will be considered elsewhere.

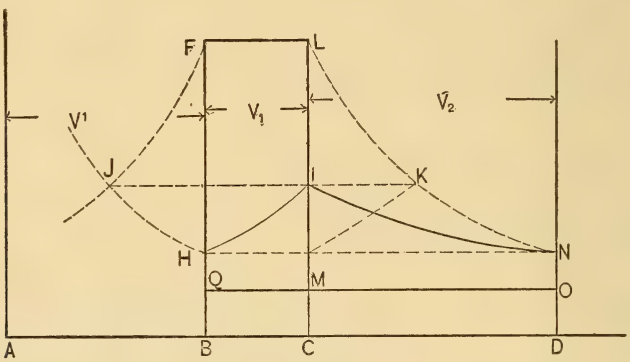


Fig. 4.

II.—Diagram of a Compound Engine with no Clearance or Cut-Off, but with Receiver.

Let the volume of the high-pressure cylinder be BC, of the low-pressure cylinder be BD, and of the receiver AB. Now, supposing the engine in continuity of action, the diagram may be traced as follows: Represent the admission for full stroke by FL. At the end of the stroke the valve between the high-pressure cylinder and receiver opens when the steam in these parts commingle and the pressures equalize, the cylinder steam expanding and the receiver steam being compressed. As LF is the volume of the cylinder steam, this expansion will be along an expansion line FJ with the point C as the zero of volumes. The receiver steam will have the volume AB, and its pressure will be BH=DN=the terminal pressure of the low-pressure cylinder. Now as the "fresh"

steam FL is introduced, this receiver steam will be compressed along a compression line HJ with A as the zero of volumes. The point J of intersection of these curves gives the resulting pressure of the combined steam. The curve MK =HJ, giving K=J, might have been used. This pressure is that with which the low-pressure cylinder begins its stroke; and it should be laid off on the line at CI, because now the volume of steam is AC filling the receiver AB, and high-pressure cylinder BC. Now during the stroke this steam changes volume from AC to AD, because the high-pressure cylinder ejects its steam into the receiver, thus giving cause for a reduction of volume from AC to AB, while at the same time the low-pressure cylinder gives cause for an increase to AD, the difference being CD, the actual increase as stated. The expansion curve IN will be drawn with A as the zero of volumes, because AC is the lesser and AD the greater of the limiting volumes. The back-pressure line IH to the high pressure cylinder will correspond in height with IN, as explained in Fig. 3. The back-pressure line to the low-pressure cylinder is to be accounted for as to whether the exhaust is into a condenser or into the atmosphere.

Detrimental Effect of the Receiver.—This as a steam diagram differs from that of Fig. 3 by a depression of the expansion line IN from the dotted position LN. This engine with its receiver is thus seen to be less efficient than that of Fig. 3 by the absence of the area ILN. The smaller the receiver the less the depression or "drop" of the point I. For AB=*o*, I coincides with L. For AB =infinity the line IN is horizontal and straight. But the point N is fixed, its position being entirely independent of the volume of the receiver, and will always be found on the expansion line LN, the latter being drawn with B as the zero of volumes.

This important truth is apparent from the fact that at the end of the stroke of the low-pressure cylinder we discharge just the cylinder full of steam, the weight of which is the same as the weight of the steam admitted per stroke. Assuming the latter the same for two cases, one having a receiver and the other not; then it follows that the weight of the low-pressure cylinder full of steam exhausted per stroke is the same for both cases, so that if the small cylinders are of equal size the pressure of the contained steam at stroke terminations is the same for both; and also if the large cylinders are of equal volumes the pressures of their contained steam will be the same in both, thus making the points L and N coincide in both cases, both points being found on the line LN drawn with B as the zero of volumes and pressures as stated. This fact gives a ready means for locating the point N in tracing a diagram.

Comparison with an Indicator.—The diagrams taken directly by the application of an indicator to the engine will be HFLI for the high, and QHIM for the low-pressure cylinders. The relation of the work performed by the two cylinders can be obtained by measuring the areas just named with a planimeter, multiplying QHIM by the ratio of volumes and comparing with HFLI. But when the low-pressure diagram is expanded to BD, as in Fig. 4, the areas, and consequently the work of the two cylinders can be compared by sight, or by planimetric measurements.

The diagram that would be obtained by use of only the low-pressure cylinder as a simple engine, would be QFLNO, so that according to the diagrams the simple engine would be more efficient than the compound of Fig. 4 by the ratio

$$\frac{\text{QFLKNOQ}}{\text{QFLINOQ}}$$

Re-heating.—The receiver, which is responsible for the drop LI, appears by the diagram to be a detriment. This drop may, however, be more than compensated by using the receiver for a "re-heater" of the steam, by passing high steam through it in tubes. The intermediate steam may thus be raised in temperature to nearly that of the boiler steam, and consequently be somewhat superheated and made to work dry. The whole line IN may thus be raised to an extent nearly proportional to the elevation of the *absolute* temperatures of the steam by superheating, the absolute zero being a point about 493° F. below melting ice. (See lower part of Fig. 6.)

A small intermediate passage, such as shown in Fig. 1, will have the effect of a small receiver, and cause its corresponding drop LI.

The diagram Fig. 4 supposes no cut-off to either cylinder. There may, however, be one to both, as in the next case.

III.—Diagram of a Compound Engine with a Cut-Off to Each Cylinder, and with an Intermediate Receiver.

In Fig. 5 take BC = volume high cylinder, BD = B′D′ = volume of the low cylinder. AB = volume of the receiver, E = point of cut-off for the high cylinder, and N = the point of cut-off for commingled steam. A horizontal line through J gives I, the point from which lines of further action are to be drawn.

At this point of time the high cylinder piston is at the end of stroke, the low cylinder piston at beginning of stroke, and the volume of steam under action is that of AC, which fills the receiver and high cylinder in common. In the absence of a low cylinder cut-off, the stroke of pistons now beginning would expand the steam from its present volume AC to the volume AD, and the pressure of release would be DG, the point G being on the linè ELJ prolonged, as explained in Fig. 4. But suppose the low cylinder cut-off occurs at half stroke. This

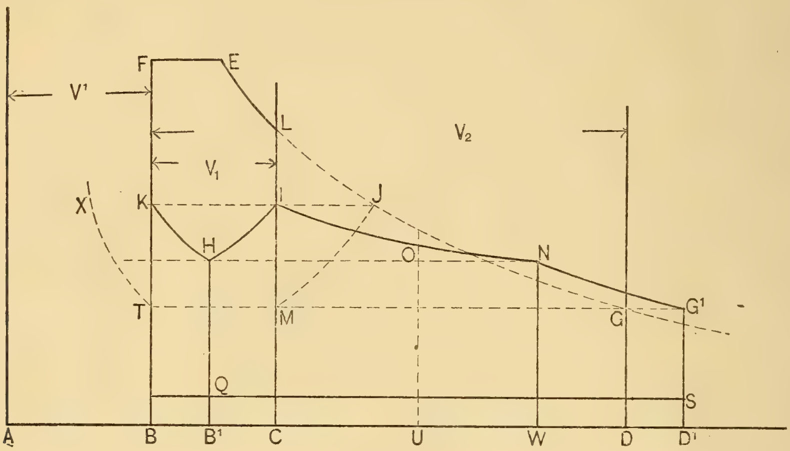


Fig. 5.

the low cylinder. Also G′ = the point of release of the steam from the low-pressure cylinder, and QS = the back-pressure line of the final exhaust.

Now admit boiler steam FE, and cut-off at E, in the high cylinder BC. This steam expands along the expansion line EL, drawn with B for the zero of volumes and pressures. At the stroke terminal L, the valve between the high cylinder and receiver opens and the steam of these parts commingle; that of the former expanding along the line LJG with B for the zero of volumes, and of the latter along the compression line TX, with A for the zero of volumes. By turning over the curve TX, and transferring it to MJ, we obtain the intersection J, the height of which point above AD gives the resulting pressure of the cut-off increases the average receiver pressure, so that the expansion line IN will now cut the line ELG and place N outside. To trace the curves G′NIHK, we find that the expansion line IN will be drawn from I, with A for the zero of volumes. To find the point N, suppose the cut-off in the low cylinder occurs at half stroke. It will also be at half stroke of the high cylinder, and the expanding volume between pistons at the instant of this cut-off will be the receiver AB + half the cylinder BC + half the cylinder BD. Take BU $=\frac{1}{2}$ BD, and UW $=\frac{1}{2}$ BC $=$ BB′. Then N will be found at the point where AW = AB + BU + UW = the total volume expanding from I with the zero of volumes constantly at A.

Now when the steam is cut off at N, and at half stroke, the volume inclosed

in the low cylinder will be the half of that cylinder=B′W. Hence the expansion will go on from N to G′ with B′ for the zero of volumes until we have WD′ $=\frac{1}{2}$ BD$=\frac{1}{2}$ B′D′ completing the remaining half stroke. The line IH will be in common between the two diagrams from the same causes, as is DH, Fig. 3, or IH, Fig. 4, in common for those diagrams. Drawing from the half stroke point H, the line HB′, and also the back-pressure line QS, we complete the theoretical diagram for the low cylinder, HING′SQH. When the cut-off N occurs there is shut up in the receiver and the high cylinder the volume AB+BB′. The remaining stroke of the high cylinder will compress this to AB, giving the compression line HK, with a for the zero of volumes. Hence the high cylinder diagram is FELIHKF.

Effect on Distribution of Work of the Low Cylinder Cut-Off.—Were there no cut-off to the low-pressure cylinder, the expansion in this cylinder would terminate at G instead of at G′, and start from some point below I. From this it appears that the low cylinder cut-off has the effect to raise the mean pressure in the low cylinder and diminish it in the high cylinder. Hence, by applying a cut-off to the low cylinder we may increase the work performed in that cylinder, and diminish that performed in the high-pressure cylinder.

The terminal pressure D′G′ will be the same as though there were no cut-off to the low-cylinder, because the steam admitted is FE, and it must occupy the same volume B′D′=BD=the low cylinder pull at release, whether there be a low cylinder cut-off or not. Hence the terminal pressure D′G′=DG=CM will be

$$D'G'=BF\times\frac{FE}{BD}$$

if the Mariotte law of expansion is assumed to hold. By thus drawing the diagram Fig. 5, the relative or total areas or work of cylinders may be obtained.

To draw IH, take a zero of volumes to the right of C a distance

$$=AC\frac{V_1}{V_2-V_1}$$

In the present case, when the receiver is very large, the line KHIN comes to be nearly horizontal, and is quite so for an infinite receiver. When the receiver is zero in volume, the cut-off N must be dispensed with, and then the point I will rise to L.

IV.—Diagram of a Compound Engine with a Cut-Off to Each Cylinder, with a Receiver, and with Clearance and Cushion.

In Fig. 6 take $AB_1=V'$ = volume of the receiver, $BC=V_1$=volume of the high cylinder B_1B = volume of clearance to the high cylinder, B_1B_2 = volume of clearance to the low cylinder, and $B_2D=V_2$=volume of the low cylinder. Also in the high cylinder let E be the point of cut-off, L the release, IH the back pressure line for $\frac{5}{8}$ the stroke or till the cut-off occurs in the low cylinder, Hb the cushion line against the receiver, and bk the cushion line against the clearance; and for the low cylinder, let I′N be the combined expansion line preceding cut-off at $\frac{5}{8}$ stroke, NG the expansion line subsequent to cut-off. SQ′ the back pressure line, Q′c′ the cushion against clearance, and H′I′ the counterpart of I′N. To better fix the ideas, let Fig. 7 represent the main features of the compound engine now considered. A being the high cylinder, B the low cylinder, C the receiver, F the boiler supply, G the exhaust, D the valves for A, and E the valves for B. The receiver includes all the space between D and E, the clearance of one end of A, all the space between D and the piston displacement for that end of A, and likewise the clearance of B, all the space between E and the piston displacement of B.

Now supposing the engine in continuity of action, let steam be cut off at E, Fig. 6, the clearance F′F, of course, being full of steam also. The expansion for the remainder of stroke will give the expansion line EL, drawn with the bottom of the clearance B_1, as the zero of volumes.

This is due to the fact that F′E and not FE is the original volume of the steam now expanding. At L, the high cylinder and its clearance are opened by a valve into the receiver, and the steam of these parts commingle, that of the former expanding along a line LJ, and of the latter being compressed along the line e'X=MJ; both taking a common pressure J at the intersection. The point M is understood in this to give the pressure in the

receiver and low-cylinder clearance at the instant the stroke is ended; and the valve between receiver and low cylinder is opened. The pressure M will be explained further.

that of the high cylinder and its clearance, and that of the clearance of the low cylinder. It is $AB_1 + B_1B + BC + B_1B_2 =$ a volume extending to the point I′ where $II' = B_1B_2$. This volume will now expand

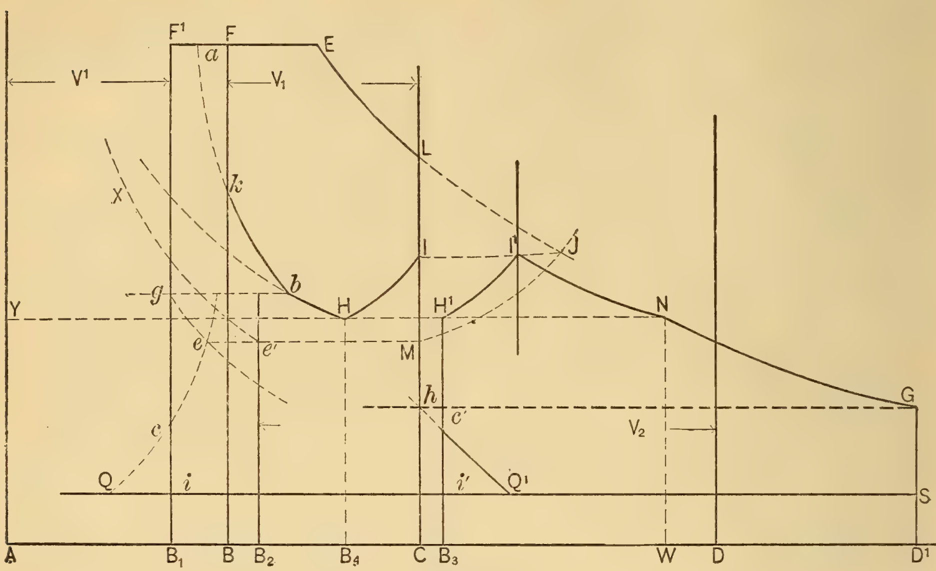


FIG. 6.

Drawing a horizontal line through J we obtain I for the initial back pressure line IH of the high cylinder V_1; and I′ for the initial forward pressure of the low cylinder V_2, I′ being distant from I by the clearance of V_2, viz. B_1B_2. I′H′ is parallel

along an expansion line I′N with A for the zero of volumes.

The point N is found by laying off the total volume for the time of cut-off in the low cylinder which has been taken above at $\frac{5}{8}$ the stroke. This fraction being the

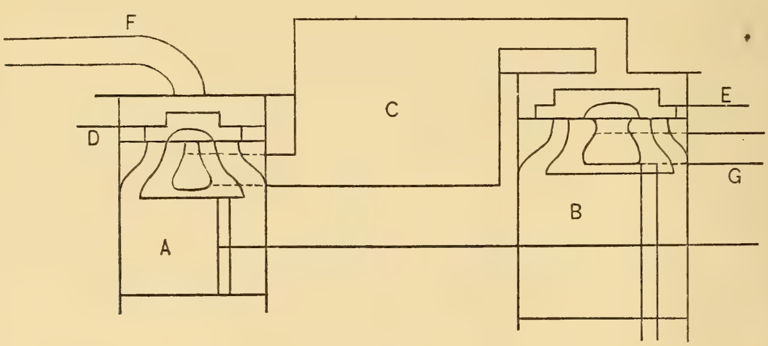


FIG. 7.

to IH, that is, $II' = HH'$. We are now at the point of beginning of another stroke which will force the remaining steam from V_1 and fill V_2.

At this point of time the total volume of steam considered is that of the receiver,

same for both cylinders we have H at $\frac{5}{8}$ stroke from I, so that $YH = AB_1 + B_1B + \frac{5}{8}BC =$ receiver + high cylinder clearance + $\frac{5}{8}$ high cylinder volume. To this add the low cylinder clearance $B_1B_2 = HH' = II'$; and also add $\frac{5}{8}$ the volume of

the low cylinder, or $\frac{3}{8}$ B_2D and we have the total volume $YH+HH'+H'N=YN$, $H'N$ being equal to $\frac{3}{8}$ V_2. Beyond N the steam inclosed in the low cylinder will expand the remaining $\frac{5}{8}$ of stroke, giving the expansion line NG, drawn from B_4 as the zero of volumes; where NG, or $D'W=\frac{5}{8}$ the stroke, NH', or $WB_3=\frac{3}{8}$ of stroke, and $H'H$, or B_3B_4=clearance.

The cushion line $Q'C'$ is due to the compression of a portion of the exhaust steam into the clearance, and is found by drawing Qc with B_2 as the zero of volumes, where $Qi=Q'i'$. The piston will compress the steam to c when further compression will result from the reversal of the valve and the commingling with it of the steam from the receiver. The pressure of this latter steam now admitted from the receiver is found from the point b where the high cylinder closes its valve for cushion, and after which the receiver is abandoned to itself till the end of the stroke is reached. This pressure b is B_1g, and as the steam is thus admitted from the receiver the pressures B_1c and B_1g combine at the intersection e of the line Qce drawn from B_2 as the zero of volumes, and the line ge drawn from A as the zero of volumes. The total volume now is AB_2 and pressure B_2e', so that the line $e'X$ of further compression can be drawn with A as the zero of volumes. Transferring this to MJ, and prolonging EL to J, we have the point of intersection J as that at which the steam now released from the high cylinder, combines with that in the receiver to a common pressure.

In the high cylinder and receiver there will be compression Hb, the curve being drawn with A as the zero of volumes. At b the valve between receiver and high-cylinder is supposed to close giving a compression line bk, which may be produced to a, drawn from B, as the zero of volumes.

The cushioning in the cylinders fills part of the clearances so that the steam admitted per stroke is aE at the pressure BF, and that exhausted is hG at the pressure $D'G'$, the curve $Q'c'h$ being understood to be the same as Qce. The last statement enables us to find the pressure $D'G$; and it is=

$$D'G=BF\,.\,\frac{Ea}{Gh}$$

according to the Mariotte law of expansion.

For convenience in drawing the curve $H'I'$ we observe that the change of volume $H'I'$ is $\frac{3}{8}$ V_1, and the change of volume $I'N$ is $\frac{3}{8}$ (V_2-V_1), so that $H'I'$ may be drawn from a zero of volumes taken at a distance to the right of B_3=

$$AW\frac{V_1}{V_2-V_1}.$$

The larger the receiver the more nearly horizontal becomes the line $bHII'N$, it being quite horizontal for an infinite receiver. On the other hand, the smaller the receiver the steeper become the declivities of that line and the more nearly I approaches to L, and the latter should be the cut off N. But I never coincides with L while clearance exists.

The cut-off N may be made useful in equalizing the amount of work performed by the two cylinders.

Relative Efficiencies.

In all the foregoing diagrams the theoretical diagrams have been sought, such as are needed for studying the relative efficiencies of engines, and not such as would be obtained by the indicator. Of several engines of different volumes of receiver, clearance, cushion, &c., of which the diagrams have the same heights BL, and the same steam volumes Ea; that engine which shows the greatest added area of both diagrams has the highest efficiency. Such efficiency can be investigated graphically as above, and that method is probably the most practical, because analytical formulas for such cases as the last two above, become so excessively complex as to make more trouble and labor than will the drawing board and planimeter.

Doctoring of the Diagrams for Speed, Condensation, Re-Heating, etc.

The faster the engine runs and the greater the amount of work done the greater will be the resistance to the fluid through the passages from the boiler through the engine to the condenser or atmosphere. This resistance causes a lowering of the admission line FE, a raising of the exhaust line $Q'S$, and a vertical separation of the lines $IHbk$ and $H'I'NG$, of Fig. 6, and likewise for the

other cases an approximation to the amounts of these effects can be obtained by the aid of the formulas for flow through orifices and pipes, which are,

For orifices

$$v^2=\frac{q^2}{a^2}=2u^2gh=64u^2h \text{ nearly,}$$

and for pipes or passages

$$v^2=\frac{q^2}{a^2}=\frac{2gha}{fsl}=1100.\frac{ha}{sl} \text{ nearly;}$$

where the head h may be obtained from the pressures and density of steam or air by the formula

$$h=\frac{p'-p''}{\delta}$$

In these formulas v=velocity of fluid through orifice or passage, q=the cubic feet of fluid per second, a=area of cross section, orifice or passage, s=perimeter of same section, l=length of passage, all in feet, u = a coefficient of velocity through orifice, =0.82 for orifice of entry to a tube, or=0.62 for an orifice through a thin partition, f=coefficient of friction=about 0.006, $p'-p''$=the fall of pressure lbs. per square foot in going through the orifice or passage, and δ=the density or weight per cubic foot of the flowing fluid. Its value may be obtained from Table D by dividing the weight of a cubic foot of water (about 62 lbs.) by the value in the volume column of table corresponding to the pressure for which δ is sought.

The formulas apply only for comparatively small falls of pressure, such as that from boiler to steam-chest, or from steam-chest to cylinder.

In selecting the value of δ, care should be taken to find its true value. In wet steam the moisture should be included. From this cause the value of δ may rise to two, or even three times its value for dry steam.

If w=velocity of piston in feet per second, and A=area of piston in square feet, we have

$$q=\mathrm{A}w$$

for steam passages leading to and from cylinders. Introducing this, and taking u at .8, also eliminating h, we obtain the practical formula for entry to passages, and for short passages

$$\frac{\mathrm{A}^2w^2}{a^2}=41\frac{p'-p''}{\delta}$$

and for passages only

$$\frac{\mathrm{A}^2w^2}{a^2}=11000.\frac{a}{sl}.\frac{p'-p''}{\delta}.$$

Combining the two formulas into one for application to passages whose lengths vary from 10 to 300 or 400 times their thickness, we obtain

$$\frac{\mathrm{A}^2w^2}{a^2}\left(1+.0074\frac{sl}{a}\right)=41\frac{p'-p''}{\delta}$$

It is generally best to assume a value for the "a" in the parenthesis of the last formula.

The rounding of the corners of the diagrams can only be done by the judgment of the engineer. The greater the speed of the engine the earlier must be the release, in order that the release end of the diagram shall not be too sharp at the top corner at the expense of excessive curtailing of the lower corner. The rounding of the cut-off corners must be done with reference to both the speed of engine and rapidity of closure of cut-off valve.

Considerable condensation is liable to occur in the high-pressure cylinder when it has an early cut-off, so that the expansion line for it may fall with undue rapidity. This may be avoided in a measure by use of the steam jacket. The condensed steam flowing from this cylinder will accumulate in the receiver unless disposed of by escape traps, or by reheating, the latter process consisting of placing hot surfaces in or about the receiver. These heating surfaces may be tubes passing through and containing high steam from the main boiler, or from a second boiler, which may be at even a higher temperature and pressure than the steam in the first named one. The steam may thus become superheated for the low-pressure cylinder. If now no water has been drawn off, but if all the contents from the high-pressure cylinder is changed into superheated steam, then the expansion curve for the low-pressure cylinder may first be drawn on the supposition of dry steam, and then to be raised nearly to the extent of the elevation of the absolute temperature of that steam above the temperature due to its pressure as dry steam. (See Fig. 14). The latter temperature is given by tables of the properties of steam. (See Table D in Turnbull's Treatise.) If any con-

densed steam is trapped out, the expansion curve of the low-pressure cylinder must, provided the steam admitted be dry, be below that due to dry steam.

Precise Form of the Expansion and Compression Curves.

In a diagram such as that of Fig. 5 or Fig. 6, the steam curves differ in character. From the standpoint of thermodynamics, the curve EL, for instance, is an adiabatic for initial dry steam. Steam thus expanding fogs up with particles of condensed steam forming throughout the mass, and a rainstorm begins in the cylinder at each stroke. That steam does its full work due to expansion, and EL falls more rapidly than the hyperbola. But from L to J the steam does but partial work, viz., that under the compression curve MJ. The action LJ and MJ, of combining, is "isodynamic," that is, no external work is performed. Both these curves may therefore be drawn as isodynamic curves, they being very nearly hyperbolic.

If the steam is reheated to superheat in the receiver, the line IN will be the adiabatic for superheated steam, also the line NG. Such lines fall much more rapidly than the hyperbola. Compression lines, as in cushioning, rise more rapidly than the hyperbola, because the steam is superheated by the compression, and if any water is present, there may be re-evaporation.

The equations of all these curves may be expressed approximately by the equation,

$$pv^x = \text{constant}.$$

in which, for the Mariotte law of expansion, $x=1$. To supply the constant we may put the equation in the form

$$\frac{p}{p_1} = \left(\frac{v_1}{v}\right)^x \quad . \quad . \quad . \quad . \quad (1)$$

An equation which is approved for ordinary purposes by high authority, and will be adopted in the present case.

Table E, next column.

This table and the equation will give us the equation of any curve desired.

In these we not only have authority for a variety of values of x, but in m the "steam quantity" ∞ suggests that the value of x may be determined even by estimation from such facts as may effect ∞. Accordingly, for an expansion where only partial work of expansion is performed, x may be found to lie between $\frac{4}{3}$ and 1.0456 for superheated steam. When $\infty = 1$ we have $x=1.135$. It is to be observed, however, that the form of curve, according to equation (1), is dependent on the value of the exponent, so that the general equations may be considered as suitable for any forms of these lines.

Table E:—Of Values of x, and Authority.

Values of $x=$	Adiabatic expan. Initial saturation, ∞. m	Curve of saturation. s	Adiabatic for superheated steam. a	Isodynamic for superheated steam. n
Rankine.	$\frac{10}{9}=1.11111$	$\frac{17}{16}$, and 1.0646	1.3	Not given.
Cotterill.	$1.035+\frac{\infty}{10}$	$\frac{17}{16}$, and 1.0646	"	Bet. s and 1
Zeuner...	" "	1.0646	$\frac{4}{3}=1.3333$	1.0456
Röntgen.	" "	"	"	"

The table shows a very considerable range in the value of x, some going so far from $x=1$, as to advise the abandonment of the Mariette law of expansion in which $x=1$. An example of carefully computed volumes for several curves and chosen pressures will serve to illustrate the departure of the expansion lines of different kinds from each other.

Table F.—Example. Steam.

Absolute pressures. lbs.	Common hyperbola. vols.	Saturation curve. vols.	Adiabatic curve. vols.
23.	122.0	122.0	122.0
50	56.1	58.3	60.6
83	33.8	36.5	38.4

Thus for these curves running through a point at 23 lbs., and volume 122, at about a threefold greater pressure the saturation curve deviates 8 per cent. and the adiabatic curve 14 per cent. in volume from the hyperbola.

But as the curve for which $x=1$, viz., the common hyperbola drawn by its asymptotes, is so very convenient for tracing, (see Fig. 2,) and falls so near the required curves, we are lead to look for some rule by which to "correct over" from the hyperbola to the curves sought. Such rule was given by the writer in the *American Machinist*, for Feb. 10, 1883, the convenience of which assures the desirability of the following abstract here:

Some engineers judge of the steam action, leakage of piston, valves, &c., by a simple reference to the hyperbola.

Then what here follows could be dispensed with, but the more fastidious will probably wish to compare with other curves, which can be easily laid off from this hyperbola by correction, or difference quantities as follows, as hinted by Cotterill, page 341.

Differentiate the equation given above, regarding x and v as variable. The result can be reduced to

$$\frac{dv}{v}=\frac{dx}{x}\text{ hyp. log. }\frac{v_1}{v}=\frac{dx}{x}\times k$$

in which v_1 is one volume, as, for instance, a, Fig. 8, and v, the other, for any point as c; and k, a coefficient. The logarithmic part, or coefficient, k, may be tabulated, and becomes a convenient coefficient, or factor, for the second member of the equation, or for the quantity $\frac{dx}{x}$ which quantity is definitely known as soon as x is decided upon. For instance, take $x=\frac{10}{9}$ for the adiabatic curve, it being 1 for the common hyperbola. Then dx will be the difference of these values, or $\frac{10}{9}-1=\frac{1}{9}$, and $\frac{dx}{x}=\frac{1}{10}$.

If the coefficient were 1, then the correction dv would be a tenth of v.

More definitely, suppose in Fig. 8, $OB=v_1$ and $OA=v$; then the correction is $ce=\frac{1}{10}$ OA, for the values just named.

The values for the coefficient, k, are given in the table G.

TABLE G:—OF COEFFICIENT k.

$\frac{v_1}{v}$	k		$\frac{v_1}{v}$	k
1.1	.095	..	3.3	1.194
1.2	.182	..	3.4	1.224
1.3	.262	..	3.5	1.253
1.4	.336	..	3.6	1.281
1.5	.405	..	3.7	1.308
1.6	.470	..	3.8	1 335
1.7	.531	..	3.9	1.361
1.8	.588	..	4.0	1.386
1.9	.642	..	4.2	1.435
2.0	.693	..	4.4	1.482
2.1	.742	..	4.6	1 526
2.2	.788	..	4.8	1.569
2.3	.833	..	5.0	1.609
2.4	.875	..	5.5	1.705
2.5	.916	..	6.0	1.792
2.6	.955	..	6.5	1.872
2.7	.993	..	7.0	1.946
2.8	1.030	..	7.5	2 015
2.9	1.065	..	8.0	2.079
3.0	1.099	..	8.5	2.140
3.1	1.131	..	9.0	2.197
3.2	1.163	..	9.5	2.251
		..	10.0	2.303

TABLE H:—OF VALUES OF $\frac{dx}{x}$.

For the saturation curve,

$$x=\frac{17}{16},\text{ and }\frac{dx}{x}=\frac{1}{17}=.0588.$$

For the adiabatic curve,

$$x=\frac{10}{9},\text{ and }\frac{dx}{x}=\frac{1}{10}=0.1.$$

For the adiabatic curve, $x=1.035-.1\,\infty$ and

$$\frac{dx}{x}=\frac{.035-.1\,\infty}{1.035-.1\,\infty}.$$

For the isodynamic curve, $x=1.0456$, and

$$\frac{dx}{x}=\frac{.0456}{1.0456}=.0436.$$

For the adiabatic of steam gas, $x=1.3$, and

$$\frac{dx}{x}=\frac{.3}{1.3}=.2308.$$

For the adiabatic of gases, $x=1.408$, and

$$\frac{dx}{x}=\frac{.408}{1.408}=.29.$$

To illustrate the use of these tables, suppose ac, Fig. 8, to be the hyperbola of reference (the common hyperbola), with $Ba=23$ lbs., $Ac=83$ lbs., in absolute pressures; and OB, the volume 122, as in Table F. Then the volume OA, will be 33.8 for the common hyperbola. Hence for $v_1=122$, and $v=33.8$; $\frac{v_1}{v}=3.61$.

In Table G, we look under $\frac{v_1}{v}$ for the value 3.61 and find the nearest to be 3.6, but 3.61 is one-tenth the way over toward 3.7. Consequently we look for that value of k, which lies one-tenth the way from 1.281 toward 1.308, and obtain $1.284=k$. For k, this answers for all the curves.

Now in Table H, we find, for adiabatic expansion of steam,

$$x=\frac{10}{9}\text{ and }\frac{dx}{x}=0.1$$

consequently

$$\frac{dx}{x}k=0.1\times1.284=.1284=\frac{dv}{v}.$$

The correction is therefore $dv=v\times.1284$ $=33.9\times.1284=4.9=ce$, Fig. 8. This added to the volume $v=OA$ gives

$$v+dv=33.8+4.8=38.7=fe,$$

a value which differs less than 1 per cent. from the computed value 38.4 in Table F.

As a second case, suppose the expansion be according to the saturation curve, when by Table H

$$\frac{dx}{x}=\frac{1}{17},$$

then

$$\frac{dx}{x}k=\frac{1}{17}\times 1.284=.0745$$

and $dv=v\times .0745 = 33.8\times .0745 = 2.86$. This added to the volume v gives

$$v+dv=33.8+2.86=36.7$$

a value which differs but slightly from the calculated value 36.5 at the foot of the second column of Table F.

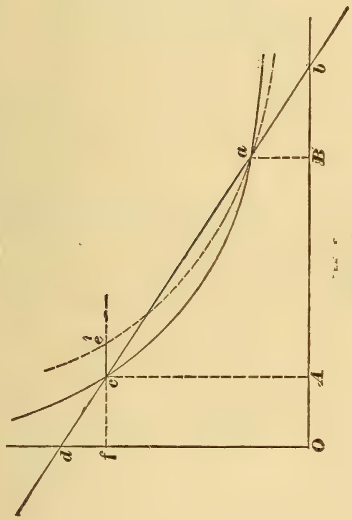


FIG. 8.

In these calculations we observe that the volume v, with which to multiply, and to which to add, is the one which is to be corrected, viz., in this case we seek the correction $c\ e$, Fig. 8, and hence the volume v is to be taken as AO.

But the rule may be applied the other way. That is to say, we may make c the point of intersection of the two curves, and correct the volume BO. In that case k is to be found as before, and given the contrary sign. In fact the whole quantity $\frac{dx}{x}k$ will be the same as before, except for the contrary sign; if the volumes on the common hyperbola are the same, viz., 33.8 and 122. But in this calculation we should multiply by the volume BO, because the correction is for this volume. That is, the correction is $122\times .1284=15.7$ for the adiabatic expansion, a quantity which, by the accurate method, is found to be 16.6, differing be less than one per cent. or the value of BO to be corrected. For the saturation curve the correction is $122\times .0745$, &c.

For points beyond a the correction is subtractive instead of additive, and multiply tabular quantity by the greater instead the lesser volume; that is, by the volume to be corrected.

Let us apply the first rule to correct the hyperbolic volume 56.1, to the adiabatic volume 60.6, for pressure 50 lbs. absolute; the final volume and pressure being 23 lbs. and 122 respectively.

Dividing greater by lesser volume we get $\frac{122}{56.1}=2.175$. For this ratio of volumes we find k from Table G to be $k=.777$. For the present adiabatic expansion, Table H calls for $\frac{1}{10}$ giving .0777. Multiplying by the volume 56.1 gives the correction of 4.47. Adding this to the volume 56.1 to be corrected, we obtain $56.1+4.47=60.6$; or, in this case, exactly the value given by the logarithmic calculation of Table F.

This method of corrections by which the curves desired can be easily obtained from the simple hyperbola, will greatly facilitate the graphical method of accurately calculating the relative efficiencies or the relative area of diagrams, or of work performed in the cylinders of compound engines.

ACTUAL EFFICIENCY.

The actual efficiency of the engine is obtained by dividing the whole ft. lbs. of work performed in both cylinders per stroke by the dynamical value of the heat required to produce the steam consumed for that stroke.

Thus, in Fig. 6 the work performed per stroke in ft. lbs. is obtained by multiplying the mean pressure lbs. per square inch in a cylinder by the piston area in square inches, and by the length of stroke in feet, and adding the products thus obtained for all the cylin-

ders. Call this quantity U. The mean pressure is obtained by taking the mean height of the diagrams in the usual way, the scale of heights being that of pressures per square inch. To find the mean height of a diagram by aid of the planimeter, find the area by the instrument and divide that area by the length of the diagram expressed to the proper unit. If the instrument gives the area in square inches divide the area by the length in inches, and lay off the result as a height in inches on the diagram. A horizontal line drawn through this point is the line of mean pressure, and that pressure is to be measured by the scale of pressures.

The steam consumed per stroke has already been stated to be E*a*, Fig. 6, which must now be measured with the proper scale. Measured by the same scale which makes BC the volume of the high cylinder in cubic feet, gives the quantity of steam *e a* in cubic feet, as desired. Call this V′.

The heat required to produce the steam used at a stroke of the high-pressure cylinder will be the total heat of evaporation from the temperature of feed water, and at the temperature of boiler pressure.

Let V_1' = the volume of steam required per stroke of high cylinder in cubic feet.

D_1 = the weight per cubic foot of the steam as supplied to the engine.

U = the ft. lbs. of work performed per stroke of engine.

H_1 = the dynamical value of the total heat of evaporation per pound from 32° F. at the temperature of boiler pressure steam; that is, the heat to raise the temperature of the water from 32° F. to the temperature of the steam in boiler, added to the latent heat of evaporation at the latter temperature. (Convenient values are given for this in the tables of *Rankine's Steam Engine.*)

t_1 = the actual temperature of the feed water.

J = Joule's equivalent, = 772 ft.-lbs.

Then the total heat consumed per high-pressure cylinder full of steam in dynamical value, or ft.-lb. units, is

$$H = V_1' \, D_1\left(H_1 - J(t_1 - 32°)\right) \quad . \quad (2)$$

and the efficiency $= \dfrac{U}{H}$ . . . . (3)

Area of Diagrams by Calculation.

Some of the more simple diagrams may be more readily calculated than drawn on a drawing-board and measured.

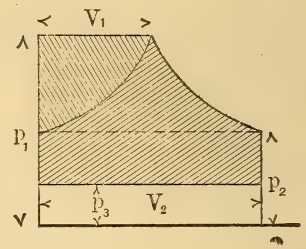


Fig. 9.

For such a diagram as shown in Fig. 9 the area of that portion falling under the expansion line between two verticals and above the vacuum line for one pound of steam in the usual notation is

$$\int pdv \quad . \quad . \quad . \quad . \quad . \quad (a)$$

where p_1 as shown, is the specific pressure of the steam, or absolute pressure per unit surface; usually pounds per square foot; v_1 = the specific volume, or volume per unit weight; usually cubic feet per pound; and where p is the varying pressure under the curve

$$\int pdv - p_3 v_2.$$

This is for one pound, and not for the actual volumes V_1 and V_2. Hence the area in (*a*) is simply *like* that designated in Fig. 9, but not equal it.

But in the present case it will probably be most convenient to apply our reasoning to the actual volumes V_1, V_2, and V′ of the high cylinder, low cylinder and receiver respectively.

For the present convenience let A = the area of the high-pressure piston, and L the length of its stroke. Then

$$V_1 = AL,$$

or multiplying through by P

$P_1V_1 = P_1AL =$ actual area under the admission line . . . . (4)

Similarly below the expansion line

$$\text{actual area} = \int_{L_1}^{L_2} PAdL \quad . \quad . \quad . \quad (5)$$

if L_1=length of the admission line, and L_2 = length of the exhaust line. The pressure P is here variable, but according to (1), the zero of volumes being noted, is

$$\frac{P}{P_1} = \left(\frac{L_1}{L}\right)^x = \left(\frac{V_1}{V}\right)^x$$

Hence (5) becomes

$$\text{actual area} = AP_1L_1^x \int_{L_1}^{L_2} \frac{dL}{L^x}$$

$$= AP_1L_1^x \frac{1}{1-x}\left(L_2^{1-x} - L_1^{1-x}\right);$$

or, since $L_2 = L_1 + l$, where l is the length under the expansion line.

Actual area

$$= AP_1L_1^x \frac{1}{1-x}\left\{(L_1+l)^{1-x} - L_1^{1-x}\right\}$$

$$= AP_1L_1 \frac{1}{x-1}\left\{1 - \left(\frac{L_1}{L_1+l}\right)^{x-1}\right\}$$

$$= \frac{P_1V_1}{x-1}\left\{1 - \left(\frac{V_1}{V_2}\right)^{x-1}\right\}$$

$$= P_1V_1 \frac{1 - \frac{1}{r^{x-1}}}{x-1} \quad . \quad . \quad . \quad (6)$$

that is, the area under any expansion or compression line is equal the rectangle to the pressure and volume of the highest point in the curve multiplied by the function of the ratio r, of expansion, and of the exponent x as shown.

To express the area in terms of the rectangle to the pressure and volume of the lowest part of the expansion curve we have, according to eq. (1).

$$\frac{P_1}{P_2} = \left(\frac{V_2}{V_1}\right)^x = r^x$$

and $$\frac{V_2}{V_1} = r$$

$$\therefore \quad P_1V_1 = P_2V_2r^{x-1}$$

and hence we may write in place of (6)

$$\text{actual area} = P_2V_2 \frac{r^{x-1}-1}{x-1} \quad . \quad . \quad (7)$$

For the particular case $x=1$ the equation becomes inapplicable. But for this case the general integral gives the

$$\text{Actual area} = P_1V_1 \text{ hyp. log. } r \quad . \quad (8)$$

which is equal to the rectangle to the highest point of the curve, multiplied by the hyperbolic logarithm of the rate of expansion. These logarithms are found in Table B.

As x is here $=1$, we have

$$P_1V_1 = P_2V_2$$

and hence we may put in place of (8) when more convenient,

$$\text{Actual area} = P_2V_2 \text{ hyp. log. } r \quad . \quad (9)$$

Applying these to Fig. 9, observing that it is the diagram of a compound engine in which there is no clearance nor receiver, and no cut-off to either cylinder, we have to add the rectangle P_1V_1 and deduct the rectangle below the exhaust line P_3V_2, and hence, calling the work performed by this engine per stroke U, we have, by application of (6),

$$U = P_1V_1 + P_1V_1 \frac{1 - \frac{1}{r^{m-1}}}{m-1} - P_3V_2$$

$$= \frac{P_1V_1}{m-1}\left(m - \frac{1}{r^{m-1}}\right) - P_3V_2 \quad . \quad . \quad (10)$$

P_3 being the back pressure.

This is the same expression as obtained for a single cylinder engine, the whole expansion taking place in the one cylinder.

The exponent x is probably m in this instance for the reason that there is no isodynamic "drop" in the pressure between the cylinders.

Similarly for the case $x=1$ we have, by (8),

$$U = P_1V_1(1 + \text{hyp. log. } r) - P_3V_2 \quad (11)$$

I. Case of Fig. 3.

Calling the volume of admission BC= V_1', and the ratio of expansion in the high-pressure cylinder r_1, we have

$$\frac{V_1}{V_1'} = r_1$$

V_1 being the volume of the high cylinder,

and to be so regarded in every application. Hence V_1' in the present case is to replace V_1 of Eq. (10) or (11). Hence, our expression for the whole work per stroke is

$$U=\frac{P_1V_1}{r_1(m-1)}\left(m-\frac{1}{r^{m-1}}\right)-P_3V_2 \quad . \quad . \quad (12)$$

r_1 being the ratio of expansion in the high-pressure cylinder itself, and r the ratio of the entire expansion C to G, also P_1=the boiler pressure (absolute), and V_1 the volume AE of the high-pressure cylinder.

The work performed in the low-pressure cylinder alone, according to (6), is

$$AHDGFA-P_3V_2=EDGFE\frac{AF}{EF}-P_3V_2$$

$$=\frac{AE\times ED}{m-1}\left\{1-\frac{1}{\left(\frac{AF}{AE}\right)^{m-1}}\right\}\frac{AF}{EF}-P_3V_2$$

$$=\frac{P_1V_1}{r_1^{\,m}(m-1)}\left\{1-\left(\frac{V_1}{V_2}\right)^{m-1}\right\}\frac{V_2}{V_2-V_1}-P_3V_2$$

The work performed in the high-pressure cylinder is, of course, the difference between the last equation and (12).

For the case $x=1$, we have

$$U=\frac{P_1V_1}{r_1}(1+\text{hyp. log. } r)-P_3V_2$$

and the work performed in the low-pressure cylinder according to (8) is

$$=AE\times ED\frac{AF}{EF}\text{ hyp. log. }\frac{AF}{AE}$$

$$=\frac{V_1P_1}{r_1}\ \frac{V_2}{V_2-V_1}\text{hyp. log. }\frac{V_2}{V_1}$$

II. Case of Fig. 4.

As to the exponents x for this case, the combined expansion and compression LJ and HJ is isodynamic, that is, unaccompanied with external work, and the steam is probably slightly superheated, so that for the fall from L to I the proper value for x is probably n, or nearly it. Granting this, then the x for the other expansion should be a, or between a and m, as may be judged by the engineer in particular cases. With these values of x, we have

$$\frac{P_i}{P_n}=\left(\frac{V_2+V'}{V_1+V'}\right)^a$$

$$\frac{P_1}{P_i}=\left(\frac{IJ}{V_1}\right)^n$$

$$\frac{P_n}{P_i}=\left(\frac{IJ}{V_2}\right)^a$$

whence

$$\frac{CL}{CI}=\frac{P_1}{P_i}=\left(\frac{V_2}{V_1}\cdot\frac{V_1+V'}{V_2+V'}\right)^n$$

$$\frac{CL}{DN}=\frac{P_1}{P_n}=\left(\frac{V_2}{V_1}\right)^n\left(\frac{V_2+V'}{V_1+V'}\right)^{a-n}$$

by aid of which the diagram can be accurately constructed. The subscripts here refer to the Fig.

For an infinite receiver $V'=\infty$; and the equations for pressures reduce to

$$\frac{P_1}{P_i}=\frac{P_1}{P_n}=\left(\frac{V_2}{V_1}\right)^n. \quad . \quad . \quad (13)$$

By application of Eq. (7), observing that for IN the ratio of expansion is

$$=\frac{V_2+V'}{V_1+V'},$$

we have,

$$U=$$

$$P_1V_1+P_n\ \frac{V_2+V'}{a-1}\left(\left(\frac{V_2+V'}{V_1+V'}\right)^{a-1}-1\right)-P_3V_2$$

and eliminating P_n

$$=\left\{\begin{matrix}1-P_3V_2 \\ +\left(\frac{V_1}{V_2}\right)^n\frac{V_2+V'}{V_1(a-1)}\left(\left(\frac{V_2+V'}{V_1+V'}\right)^{a-1}-1\right)\end{matrix}\right\}(14)$$

If $V'=0$ the receiver is dispensed with, and this case reduces to that of Case I. The above then for $n=a$ reduces to

$$U_1=P_1V_1\left\{\frac{a}{a-1}-\frac{1}{a-1}\left(\frac{V_1}{V_2}\right)^{a-1}-\frac{P_3V_2}{P_1V_1}\right\}(15)$$

which is the same as (10) if a is changed to m, as it should be for the present supposition where $V'=0$ of no isodynamic expansion nor consequent superheating. The change in diagram, Fig. 4, due to making $V'=0$, consists in the raising of the point I up to L while N remains fixed. The diagram then agrees with Fig. 9 below.

When $x=1$ we have for Fig. 4 and Equation (9)

$$\text{area CIND}=P_n(V_2+V')\text{hyp. log.}\frac{V_2+V'}{V_1+V'}$$

. . . . (16)

Adding BCLF and deducting for back pressure we have for the work performed per stroke of the engine

$$U=$$

$$P_1V_1+P_n(V_2+V')\,\text{hyp.}\log.\frac{V_2+V'}{V_1+V'}-P_3V_2$$

$$=P_1V_1\left\{1+\left(1+\frac{V'}{V_2}\right)\text{hyp.}\log.\frac{V_2+V'}{V_1+V'}\right\}$$

$$-P_3V_2 \quad . \quad . \quad . \quad . \quad (17)$$

It is to be observed that the zero of volumes for the expansion IN is at A, and for the curve LN we have, for $x=1$,

$$P_nV_2=P_1V_1$$

If $V'=0$ we have Eq. (11), as we evidently should.

IIA. Case of Cut-Off and Expansion in the High-Pressure Cylinder, Together with a Receiver.

Fig. 10 is the diagram representing the present case, where BC is the volume of the high-pressure cylinder, BD the volume of the low-pressure cylinder, and AB the volume of the receiver.

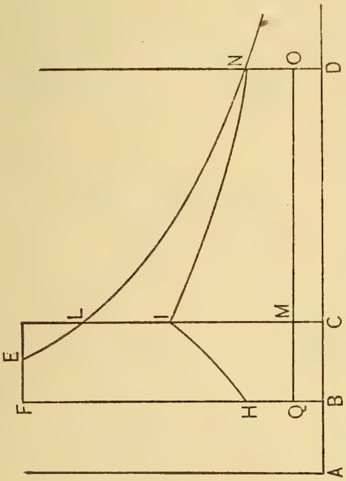


Fig. 10.

The admission at boiler pressure P_1 is from F to the cut-off at E. From E to L is the expansion curve answering to the expansion in the high-pressure cylinder, L to I is the fall of pressure due to exhaust from this cylinder into the receiver AB, and IN is the expansion curve for the forward stroke of the low-pressure cylinder, the zero of volumes for which curve is at A, because at the beginning of the stroke the volume AC is that which fills the receiver and high-pressure cylinder, while at the end of the stroke the same steam has the volume AD of the receiver and low-pressure cylinder.

The work done in the high-pressure cylinder per stroke is represented by the area HFELIH, while the same for the low-pressure cylinder is QHINOQ, OQ being the exhaust line.

Let r_1=ratio of expansion in the high-pressure cylinder, giving

$$r_1=\frac{BC}{EF}.$$

An expression for the

$$\text{area BCLEFB}=\frac{BF\times FE}{m-1}\left(m-\frac{1}{r^{m-1}}\right)$$

$$=\frac{P_1V_1}{r_1(m-1)}\left(m-\frac{1}{r^{m-1}}\right) \quad . \quad . \quad (18)$$

is given in (10).

Referring to the equation preceding (14), we see that it may be modified to represent the work per stroke in the present case by substituting (18) for the first term, and by substituting $P_1\div r_1{}^m$ for "P_1" of the second term, because there "P_1"=the terminal pressure CL, Fig 10, while BF must stand for P_1. Making these changes in (14) we have, for the work per stroke of both cylinders for the case now considered,

$$U=\frac{P_1V_1}{r_1(m-1)}\left(m-\frac{1}{r^{m-1}}\right)+$$

$$\frac{P_1V_1}{r_1^m}\left(\frac{V_1}{V_2}\right)^n\frac{V_2+V'}{V_1(a-1)}\left(\left(\frac{V_2+V'}{V_1+V'}\right)^{a-1}-1\right)$$

$$-P_3V_2 \quad . \quad . \quad . \quad . \quad (19)$$

$$=P_1V_1\left\{\frac{m-\frac{1}{r^{m-1}}}{m-1}\right.$$

$$+\frac{V_2+V'}{V_1r_1^m(a-1)}\left(\frac{V_1}{V_2}\right)^n\left(\left(\frac{V_2+V'}{V_1+V'}\right)^{a-1}-1\right)$$

$$\left.-\frac{P_3V_2}{P_1V_1}\right\} \quad . \quad . \quad . \quad . \quad (20)$$

The work performed in the low-pressure cylinder is found by taking the last two terms of (19) for the area MINO, multiplying it by BD, and dividing by CD, that is,

$$\text{area QHINOQ} = \text{MINO} \times \frac{V_2}{V_2 - V_1} \quad . \quad (21)$$

For the case $x=1$ we have the 1st term of (11) as an expression for the area of the diagram of the form BCLEF, Fig. 10, where the "V_1" in (11) is $=$FE. But here $BC = V_1 = r_1 FE = $ "V_1"r_1, whence

$$\text{“}V_1\text{”} = \frac{V_1}{r_1}.$$

Putting this value of "V_1" into the first term of (11), and we have

$$\text{BCLEFB} = \frac{P_1 V_1}{r_1}(1 \times \text{hyp. log. } r_1) \; . \; (22)$$

The area for CIND is

$$AD \times DN \text{ hyp. log. } \frac{AD}{AC}$$

and $BD \times DN = BC \times CL = V_1 \times CL$.

But $r_1 \times CL = P_1 = BF$,

and $DN = \frac{P_1 V_1}{r_1 V_2}$.

Hence the area

$$\text{CIND} = \frac{P_1 V_1}{r_1 V_2}(V_2 + V') \text{ hyp.log.} \frac{V_2 + V'}{V_1 + V'} (23)$$

and the area representing the work done by both cylinders per stroke is the sum of (22) and (23), less $P_3 V_2$.

The work per stroke of the low-pressure cylinder is

$$= (23) \times \frac{BD}{CD} - P_3 V_2 \quad . \quad . \quad . \quad (24)$$

and this taken from the work performed by both cylinders per stroke gives the work performed per stroke of the high-pressure cylinder.

The expression (24) becomes

$$\frac{P_1 V_1}{r_1}\left(1 - \frac{V'}{V_2}\right)\left(\frac{V_2}{V_2 - V_1}\right) \text{hyp.log.} \frac{V_2 + V_1}{V_1 + V'} - P_3 V_2$$

in which, if we neglect the last term and make $V' = 0$ we obtain the

Case of no Receiver,

$$\frac{P_1 V_1}{r_1} \frac{V_2}{V_2 - V_1} \text{ hyp. log. } \frac{V_2}{V_1}$$

If we put the ratio of expansion $\frac{V_2}{V_1}$ in the low pressure cylinder $= r_2$ and observe that CL is then the initial pressure in the low-pressure cylinder, which for the moment we may call $P' = \frac{P_1}{r_1}$ the above becomes

$$\frac{P' V_2}{r_2 - 1} \text{ hyp. log. } r_2 = V_2 p'_m \quad . \quad . \quad (25)$$

an expression which is essentially the same as that given just preceding Table C.

Similarly for $V' = 0$ and omitting $P_3 V_2$, the work for the high-pressure cylinder per stroke is

$$\frac{P_1 V_1}{r_1}\left(1 + \text{hyp. log.} r_1 - \frac{1}{r_2 - 1} \text{hyp.log.} r_2\right) (26)$$

and for both cylinders together the work per stroke is

$$\frac{P_1 V_1}{r_1}\left(1 + \text{hyp. log. } r_1 + \text{hyp. log. } r_2\right) . \; (27)$$

By placing the expressions for the work performed in the two cylinders equal to each other we obtain, after reduction,

$$\text{hyp. log. } r_1 = \frac{r_2 + 1}{r_2 - 1} \text{ hyp. log. } r_2 - 1 \quad . \quad . \quad (28)$$

an expression which gives the value of r_1 the ratio of expansion in the high-pressure cylinder in terms of r_2 the ratio of the volume of the cylinders and also the ratio of expansion in the low-pressure cylinder.

Example:

Placing $r_2 = 3.464$
we get $r_1 = 3.464$

for the only case where the work in one cylinder can equal that in the other, and yet with equal ratios of expansion. But this equality of work fails when there is an appreciable exhaust pressure.

As a 2d example take $r_2 = 2$

Then for equal work performed in the cylinders we have $r_1 = 2.94$

Indeed, equation (28) shows that there are an infinite number of values of r_2 which will satisfy r_1 for equal work performed in the cylinders.

Effect of a Receiver.

As the volume of the receiver is changed from zero to infinity, the point I, Fig. 4, falls from L to the horizontal through N; while the point N remains stationary. To find the point I on a drawing-board for any set of values of

V_1 V_2 and V'; first find N by aid of (13) then draw the adiabatic NI with the proper exponent, and with A for the zero of volumes.

We thus arrive at the important conclusion that by increasing the size of the receiver we diminish the amount of work performed by the same weight of steam, and to the extent due to the corresponding lowering of the line NI.

In the volume V' of receiver must be included all the space about the valves and of the communicating pipes between the cylinders, as well as that in a receiver proper. Thus the superheating chambers sometimes employed between the cylinders must be counted in.

The effect of superheating in the receiver is that to change the pressures and not the volumes. Such superheating simply raises the lines NI and HI. If the steam is dry without a superheater, then with it these lines will be raised nearly in proportion to the elevation of the absolute temperature by superheating.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—September 5th, 1883.

The death of the following named members was announced: Col. F. W. Farquhar, Corps of Engineers, U. S. A., who died July 3, 1883; and Mr. R. J. Brough, of Toronto, Canada, who died July 21, 1883.

The following elections were announced:

As follows: John Lawler, Prairie du Chien, Wis.; Albert Conro, Milwaukee, Wis.; Alex. Mitchell, Milwaukee, Wis.; D. A. Wells, Milwaukee, Wis.; Chas. L. Colby, Milwaukee, Wis.; E. P. Allis, Milwaukee, Wis.; F. de Garay, Mexico.

As members:

Andrew Bell, Carillon, Canada; Henry I. Bliss, La Crosse, Wis.; Wm. W. Card, Pittsburgh, Pa.; Frank C. Doran, Richmond, Ind.; George Downe, Ranwick, Sydney, New South Wales, Australia; Christopher L. Gates, Milwaukee, Wis.; Wm. H. Jennings, Columbus, Ohio; Albon P. Man, Jr., St. Louis, Mo.; Daniel M'Cool, Marquette, Mich.; Wallace M'Grath, Parkersburg, W. Va.; John L. P. O'Hanly, Ottawa, Canada; Geo. W. Polk, San Antonia, Tex.; Watson W. Rich, St. Paul, Minn.; Leonard W. Rundlett, St. Paul, Minn.; Edward H. Williams, Philadelphia, Pa.

As associates:

Joseph P. Card, St. Louis, Mo.; George F. Swain, Boston, Mass.

As juniors:

George B. Francis, Portland, Oregon; Alfred W. Trotter, New York City; Fredk. N. Willson, Princeton, N. J.; Herbert M. Wilson, New York City.

A paper by James L. Randolph, member of the Society, and Chief Engineer Baltimore and Ohio Railroad, upon "Vibration, or the Effect of Passing Trains on Iron Bridges, Masonry, and other Structures," was then read. Mr. Randolph refers to the fact that double track bridges are moved in the direction of passing trains, and are consequently twisted, and strains are produced not provided for. Also that cattle stops and open culverts where built of rubble work have the walls shaken to pieces by vibration.

The remedy he has supplied for these culverts and stops has been to build them of large stone as nearly the same size as possible. The tall, thin bridge piers and abutments on which iron bridges rest have their stone so much disarranged by vibration as to make it necessary to secure them with timber and iron straps. Iron bridges resting on stone pedestals vibrate in this manner and receive a return blow from the vibration of the pedestal, particularly if the pedestal is a light structure, but as the iron and the stone do not vibrate in the same period, there must be times when the result is a movement in the direction of the force. The effect of this vibration has been particularly noticeable at the Harper's Ferry bridge where there was a movement of four inches in four years. After the insertion of planks between the stone and iron this movement ceased. Where the masonry of piers has a platform of timber between its foundation and solid rock, no displacement of stone has been noticed. Mr. Randolph contends that a monolith would be the best support for structures subject to vibration caused by strains, but that a monolith of the specific gravity of granite would give a damaging return blow.

Timber would answer the purpose, but is perishable.

The material which, in his opinion, is most serviceable is an artificial stone which is about two-thirds the weight of granite, is compact, durable, and with very little elasticity.

The paper was discussed by Messrs. Theodore Cooper, Charles E. Emery, H. D. Blunden, and Wm. H. Paine.

ENGINEERING NOTES.

According to a correspondent of the *Times* there are now altogether six lighthouses and one light vessel in the Red Sea; four of these are in the Gulf of Suez, and of the remaining three one—that upon the Brother Islets—is not yet lighted. Between the Dædalus Shoal and Perim Island, a distance of more than 800 miles, there is no light at all; and though for 600 miles, after leaving the Dædalus, there are no dangers in the track of steamships, after that the sea is studded with islands and rocks, which render the navigation difficult and dangerous, especially on dark and hazy nights. The places which are more especially dangerous are Jibbel Zukur Island and the Mokha Shoals. On Jibbel Zukur Island there are now the remains of three or four large steamers which have been wrecked there during the last year or two. By placing a lighthouse on Abu Ait Island, three miles to the eastward of the

northern point of Jibbel Zukur, and a light vessel on the Mokha Shoals, the navigation of this most dangerous part of the Red Sea would be rendered much more safe and easy. For homeward-bound ships there is also a great necessity for a light on the south-east end of the Shadwan Island, as a guide to the entrance of the Straits of Jubal. With the great increase of the traffic through the Red Sea which has taken place during the last few years, it is now high time that there should be some improvement in the lighting of this great highway to the East. We may point out that the Canal Company is now extensively adopting Pintsch's fixed and floating gaslights for the canal entrance and elsewhere, and no doubt will soon employ them in the Red Sea, where they are very much wanted.

The Forth Bridge.—Major-General Hutchinson and Major Marindin have reported to the Board of Trade upon an inspection which they made of the works in progress for the construction of the bridge over the river Forth at Queensferry. After giving details of the progress made with the excavation of the foundations of the viaduct piers, which are on Whinstone rock, the inspectors say:—"The engineers have fornished us with diagrams of the strains upon the piers and other parts of the bridge, showing that, according to the result of their calculations, under no possible combination of a 56lb. wind blowing in any direction, and a rolling load of 3400 tons on the span (*i.e.* two tons to the foot), will the stress either in tension or compression exceed one-fourth of the ultimate resistance of the steel to be used in the construction of the bridge, viz., 30 tons per square inch in tension, and 34 tons in compression. We can report that the preparations which have been made, and the machinery and plant which we are informed have been ordered, indicate that it is the intention of the engineers and contractors to carry out the works in a manner suitable to the magnitude of the undertaking, and that, so far, these works have been completed in accordance with the authorized plans, and in a satisfactory manner."

The following figures relating to the proposed Messina tunnel have been published:—Length, 13,546.17 meters; fall from Sicily to datum, 154.28 meters; length below the level of the sea on this side, 4688.62 meters; length under the strait, 4299.9 meters; fall from Calabrian side to datum, 153.15 meters; length below level of sea this side, 4565.63 meters; fall of straight parts, 35 in 100, and in curves 32 in 100; cost, 71 million francs.

ORDNANCE AND NAVAL.

Trials of Machine and Breechloading Guns.—A number of machine and breechloading guns were tried on July 24, at Shoeburyness. The practice commenced with the trial of the improved Gatling gun with the Accles magazine drum. This mode of loading, which precludes the possibility of jamming, enables the gun to fire 104 rounds, or one drum in 2½ seconds. and it has been shown to be possible to fire 10 drums, or 1,040 rounds, in 68 seconds. The invention of these magazine drums is credited to Mr. Accles, of the American Gatling Company. The next gun which was fired at a range of 500 yards, was the single barrel Gardner, many of which have already been issued to the navy. A Nordenfelt 1-inch gun was then fired at 200 yards at a ½-inch wrought iron plate for the purpose of demonstrating its power of penetration. This is a gun with which the navy has also been supplied. The 38-ton 12.5 inch gun then fired Palliser shell, and made excellent practice at a 6-foot 6-inch target at 1,000 yards. Sixteen of these guns have been mounted on board men-of-war. The new 43-ton 12-inch breechloading gun was next tried. This gun, of which two are already mounted on board her Majesty's ship *Conquerer*, it is understood, will with certain slight modifications, in all probability, be distributed to the navy. At present it has obtained a muzzle velocity of 1,770 feet, but with a new powder which is now being made this number will be increased to 2,010 feet. The gun was fired at a range of 1,200 yards, with Palliser shell, at a 6-foot 6-inch target, and made excellent practice. During the day practice with an 8-inch Armstroug breechloading gun on a hydraulic carriage was shown, also a 6-inch breechloading wire gun (Armstrong) fired shrapnel with medium time and percussion fuses at a 6-foot 6-inch target, the range being 2,000 yards. The new Woolwich 9.2 inch breechloading gun was fired, and gave satisfaction. This is an experimental gun, and improvements are expected to be made in it. Its weight is 18 tons, and with a charge of 140 lb. prism powder, it throws a projectile of 380 lb. Its power of penetration of a wrought iron armour plate at 1,000 yards is 15.4 inch, and its muzzle velocity is 1,731 feet. With this gun Captain Goold Adams, R. A., fired a short time since 9 out of 11 shots through a 9-foot target at 2,500 yards. Among the most interesting of the experiments witnessed were those made with the new 6-pounder breechloading quick-firing guns, which have been constructed mainly for the purpose of protecting ships against torpedo-boats. Some ten months ago the authorities invited inventors to construct a 6-pounder gun which should possess a velocity of 1,750 feet, weigh, when mounted, not more than 10 cwt., be capable of firing 11 aimed rounds per minute, require the assistance of only three men to work it, and be also without recoil. At Shoeburyness, on July 24, three such guns, conforming more or less with the conditions laid down, were tried—a Hotchkiss, a Nordenfelt, and an Armstrong gun. The Hotchkiss gun exceeded the weight laid down by the conditions, the Nordenfelt was not free from recoil, and the Armstrong gun did not fulfil the requirements in other particulars. However, when the guns were tried, and it was seen that they were capable of remarkably fine practice, it was felt that, though all the conditions of the Ordnance Committee may not have been fulfilled, yet a great step had been taken towards the solution of the problem presented to the inventors. When the experiments with the quick-firing breechloading guns

had been disposed of, and some practice with a disappearing carriage had been shown, the new 12-pounder breechloading gun, on a hydraulic traveling carriage, fired a number of rounds at ranges varying between 2,000 and 3,000 yards. The practice of this gun, which was placed in soft ground and in a most unfavorable position, was highly satisfactory, and it was understood that it had been accepted as a fact that in it had been found the solution of the field artillery problem. The particulars of this gun are that it is a 12-pounder, its weight seven tons, its charge 4 lb., its projectile 12½ lb., and its muzzle velocity 1,700 feet. The practice with it, as with all those which were tried, was conducted by Captain Goold Adams, who, besides being the inventor of several valuable appliances in connection with his profession, possesses a remarkable skill in the use and manipulation of the various artillery weapons. Some days since, while practicing with the new 12-pounder, he placed nine rounds in succession into a 3-foot 4-inch target at 1,500 yards, which, though it says much for the excellence of the gun, shows also a great proficiency in its management. The practice at Shoeburyness was, on the whole, of the most interesting nature.

The Eighty-one-Ton Guns at Dover.—The experiments with the two 81-ton guns placed some time since in a turret at the end of the Admiralty Pier, Dover, were successfully made on July 16. There had been some apprehension that the discharging of the gun would be followed by a fall of the cliff, with the probable loss of many lives, and every precaution for the safety of the lives and property of the people of Dover had been taken. For this purpose a general order had been issued and posted up in prominent places throughout the town, stating that the 81-ton guns would be tried on the morning in question, and signifying the signs that were to indicate the moment of discharging. These arrangements were admirably carried out by the Royal Artillery. The previous night, the wind being favorable, blowing strongly from the west off the coast, Colonel Goodenough telegraphed to the Large Ordnance Committee at the War Office inviting them to witness the experiments. A west wind being a favorite trading wind for ships up and down channel, added to the fact that the wind had been blowing for several days from the south, which filled the Downs with shipping, the proposed range was, during the early part of the morning, obstructed by craft going up and down channel. At 10.30 a slight mist came over from the French coast, and the way was thought to be clear. It was well that the artillery officers were not too precipitate in firing the gun, for by the time the loading of one of the guns had been completed, it was seen, the mist having cleared off, that a three-master was right in the line of fire. By the time the range was clear again two hours and a half had elapsed. The loading of one gun, which was only half a full charge of powder, viz., 225 ℔., occupied a very few seconds Four men loaded. The rammer being of huge dimensions, took three men to handle it. It was found, however, when all was prepared for discharging, that the men had loaded as they would have, had there been a full charge, that is, with the projectile, that the powder charge had not reached the vent, and that there was a space of at least 6 inches between the powder and the electric igniting wire. This was rectified by further ramming home the charge. At five minutes past one a huge volume of smoke, preceded by a great flash, informed the many thousands that had congregated on the different situations from whence a good view of the proceedings could be had that the greatly feared gun had been discharged. The gun had an elevation of two degrees, and three seconds after being discharged, the projectile, 17 cwt., with which it was loaded, struck and ploughed up the water at a calculated distance of a mile, and, ricocheting, fell again at a short distance. The effect of the discharge was barely felt in the town and neighborhood, although the report was loud and reverberated to an alarming extent off the cliffs. On examination of the gun-carriage gear, it was found that the recoil was indicated as 7 feet, the runners had acted well, and the carriage and aiming apparatus had worked admirably. The succeeding trials were as follows: Second round (fired at 2.10): Charge, 336 ℔. of powder; projectile, 17 cwt., elevation, 3 degrees; distance before projectile striking water, four miles; recoil, with greater pressure on the brake than at the preceding discharge, 5 feet. The effect of this shot in the neighborhood was alarming. With a loud crash, one of the large panes in the lighthouse was blown out, and fell within a few feet of the many officers watching the experiment. In expectation of such a mishap, the Trinity House Committee had had a temporary mast erected in close proximity to the lighthouse, whereon a light could be displayed. Third round (2.50 o'clock): Full charge, 450 ℔. of powder, projectile, 17 cwt.; elevation, point blank. Projectile struck water half a mile from pier; recoil of gun, 5 feet. The rush of air to the vacuum caused by the tremendous explosion that took place blew out two more of the panes of the lighthouse lantern, the glass of which was over ¼ inch in thickness. The effect in the town and on the cliffs was impressing. No material damages could be seen, but doors and windows rattled, while some of the older houses shook. Fourth round (3.10 o'clock): Charge, 225 ℔. of powder; projectile, 17 cwt.; elevation, 3 degrees; recoil, 4 feet. Projectile struck water after traveling four miles; the report had little effect. Fifth and last round (3.30 o'clock): Full charge, 450 ℔. powder; projectile, 17 cwt.; direction of gun, lowest possible depression; recoil, 5 feet. Projectile struck water almost immediately after being discharged, 150 yards from the turret, whence it ricocheted out to sea. The report of this discharge was louder than any preceding, and shook to a considerable extent the ground surrounding. No damage was done beyond cracking one of the remaining windows of the lighthouse. Colonel Goodenough and the many officers who had the experiments in hand were perfectly satisfied with the result of the trial.—*Iron.*

IRON AND STEEL NOTES.

Cast-Iron of Unusual Strength.—By Edward Gridley, Wassaic, N. Y.—Members of the Institute of mining engineers who were present at the Amenia, N. Y., meeting, in October, 1877, will remember their visit to the hematite mines, just west of the village of Amenia, and some of them may perhaps recall the deposit of carbonate ore south of the opening made for the hematite. The smelting of this carbonate during the last few months, has produced an iron that seems worthy of being brought to your notice. A little more than a year ago, at the Wassaic Furnace, Dutchess County, N. Y., we made a few hundred tons of iron from a mixture of two-thirds raw carbonate and one-third Chateaugay ore, hoping that it would be suitable for steel purposes, but as the iron showed phosphorus 0.189 per cent., it was not offered. This iron looked well and seemed quite strong, and gave good results in malleable castings; but no special tests of strength were made.

About February 1st, of this year, we began using two-thirds roasted carbonate, and one-third Chateaugay, and noticing that the iron was stronger than usual, we had two samples tested, which showed tensile strength of 32,014 and 34,176 pounds per square inch. After our stock of Chateaugay ore was exhausted, we put on one-third raw carbonate with the two-thirds of roasted carbonate, and the first test made of the iron, showed 40,008 pounds per square inch.

The three tests given above were made by Mr. A. J. Copp and Mr. E. B. Manning, of the Phœnix Furnace, Millerton, N. Y., on a machine of Riehle Bros., Philadelphia. Since these tests were made, they have broken samples made with all carbonate ore as follows: 39,669, 40,816, 41,882, 42,281, 39,902, and 40,130 pounds per square inch.

A test taken from the same bed of iron as the last-mentioned (40,130), was broken by Mr. A. Blass, at Irondale Furnace, showing 40,151 pounds per square inch. A sample broken on the Riehle Bros. machine, at Stevens Institute, under direction of Professor R. H. Thurston, showed 40,000 pounds per square inch. Another sample was broken by Professor Thurston on his torsion machine, and gave torsion 7°, and tensile strength of 44,500 pounds. And still another sample, broken by Davenport, Fairbairn & Co., Erie, Pa., on their Thurston torsion machine, gave torsion 9°, and tensile strength 47,500 pounds per square inch. The average of the thirteen tests by Fairbanks, is 41,349 pounds.

These tests were all made from iron cast in the pig-bed, direct from the furnace. Some were made from the full pig turned down, but most of them from samples obtained by making a hole in the sand at the end of the pig, from 10 to 20 inches long, and about $1\frac{1}{4}$ inches in diameter. No tests have yet been made from the re-melted iron.

I add an analysis of the roasted ore, made by Messrs. Booth, Garrett & Blair, February, 1883, and an analysis of the iron, made by Dr. T. M. Drown, May, 1883:

Analysis of Roasted Carbonate.

Silica	8.240
Peroxide of Iron	77.202
Alumina	2.768
Red Oxide of Manganese	3.005
Lime	1.650
Magnesia	1.167
Phosphoric Acid	.275
Sulphur	.224
Loss by Ignition	5.684
Metallic Iron	54.042
Metallic Manganese	2.165
Phosphorus	.120

Analysis of Pig Iron.

Graphite	2.310	Total, 3.090.
Combined Carbon	.780	
Silicon	1.307	
Sulphur	.086	
Phosphorus	.294	
Manganese	1.512	
Iron	93.700	
	99.989	

Improvements in Welding.—The *Mining and Scientific Press* says it seems almost probable that, just at the time when the chief difficulties in the way of welding disappear by reason of our better knowledge and skill in manipulation, the necessity for welding in large masses will be avoided by the use of cast metal. "Heavy hammers, furnaces with neutral or reducing flames, and increased facilities for handling masses of metals, have been making more and more difficult forgings possible, but the introduction of cast steel of almost any desired quality seems to be likely to render forging an art of the past. In smaller masses, like boiler plates, great progress is being made both in this country and abroad. Bottles, buoys, small boiler shelts, fire-boxes, and, in fact, an immense variety of shapes, are now being made out of plate iron without seams or rivets. Very considerable advance is being made in the production of seamless tubes of large size, flat-welded, and cylinders with the head welded together seems to be an unanswered question, the only difficulty being the want of proper plant to manipulate the sheets. Hydraulic welding is making great advances, and some of our large locomotive works are doing very remarkable work in this way. It is notable, however, that in this large work, although an enormous pressure is necessary, the pressure must not be too great, for otherwise the metal which is sufficiently soft to form the weld will be squeezed out and the cooler metal only remain."

BOOK NOTICES.

Practical Treatise on Lightning Protection. By Henry W. Spang. New York: D. Van Nostrand. Price, 75 cents.

This little book affords specific instruction in the method of protecting buildings from lightning. The directions are of the most practical kind and may be safely followed by any builder

whether he is skilled in electrical science or not.

The illustrations are exceedingly well designed to elucidate the text.

LINEAR ALGEBRA. By Hussein Tevfik Pacha. Constantinople: A. H. Boyajian.

We believe that this essay will be read with delight by mathematical students.

The subject is developed so far as to include the equations of the conic sections.

The author is evidently accustomed to imparting instruction, as the presentation of the successive steps is done with much skill.

Linear algebra as treated in this book resembles quaternions, but is much less difficult.

PHILIP REIS, INVENTOR OF THE TELEPHONE. By Silvanus P. Thompson, B. A. London and New York: E. & F. N. Spon. Price, $2.00.

The account of Reis's labors is of special interest just now as the question of the claims of later inventors is being hotly disputed.

The historical part is well prepared, and contains fac-similes of the original pen drawings illustrative of the early instruments.

The appendices which contain documentary evidence of the later inventions of Philip Reis will be regarded by many as of more interest than the history which precedes.

THE NATURALIST'S ASSISTANT. By J. S. Kingsley. Boston: S. E. Cassino. Price $1.50.

This is a valuable aid to young collectors of Zoölogical specimens.

The directions for preserving and for preparing for microscopic work are clear and complete.

The illustrations are only fair.

THE WATCHMAKER'S HAND-BOOK. By Claudius Saunier. London: J. Triplin. Price $5.00.

This is a guide to the artisan, and is of a thoroughly practical character. All the tools of the trade are illustrated by carefully prepared cuts, which are well printed.

A large portion of the book is devoted to the selection, treatment and final finishing of the useful metals, and is thus designed to be serviceable to workers in other trades.

THE FERTILIZATION OF FLOWERS. By Prof. Hermann Muller. Translated and edited by D'Arcy W. Thompson, B. A. London: Macmillan & Co. Price, $5.00.

The interest felt in the subject of this treatise is yearly increasing. Most of the knowledge extant has been accumulated through the researches of a very few workers. The literature of the subject is not extensive.

This work is a record of an enormous mass of original observations. References are made to everything that has been written on the subject of fertilization of flowers by insects.

The illustrations are numerous and excellent.

MISCELLANEOUS.

THE POPULATION OF THE EARTH.—As an authority concerning the population of the different coutries of the world, the publication called "Die Bevolkerung der Erde," published by Justus Perthes, of Gotha, occupies a high position. From the seventh issue of this work, which has recently appeared, we find the total population of the globe estimated at 1,433,887-500, an apparent decrease in the estimate of 1880 of about 22,000,000, while the recent censuses of all the great countries show an increase of over 30,000,000. This is, however, partly explained by a readjustment of the population of China, which, formerly given at 434-626,500, has now been carefully revised and estimated at 371,200,000. After this change of figures for China, Asia is set down as possessing a population of 795,591,000; this includes the 252,000,000 for British India, and the 14-000,000 of the territory of Russia in Asia. The results of recent censuses in Europe show an increase in the population, which is now stated at 327,743,400, as compared with 315,929,000 in 1880—an increase of about 12,000,000. Africa is set down as having a population of 205,823-260; America, 100,415,400, and Australia and Polynesia, 4,232,000. Before some of these vast numbers the total population of the United Kingdom at last census (35,000,000) does not bulk largely, but this is more than counterbalanced by the vast power and influence wielded by our country in every portion of the habitable globe.—*Chamber's Journal.*

THE TEHUANTEPEC SHIP RAILROAD.—Captain Eads states that work has now actually been commenced on the Ship Railway, in the neighborhood of Minatitlan. MM. Van Brocklin is the engineer at present in charge of the work. Assisted by four parties of engineers thoroughly supplied with outfits and instruments, he is now engaged in the task of surveying the proposed route. The first sections near Minatitlan have been approved, and official copies of the surveys filed with the Government in Mexico; and in accordance with the terms of the concession, the Minister of Public Works has detailed one of the most eminent Mexican engineers to be associated in the work. Captain Eads expects that the railway will be completed in less than five years, for the transportation of ships weighing (with their cargoes) 5000 tons gross.

ENGINEERING AND METAL TRADES EXHIBITION.—In order that this exhibition may be made as thoroughly representative as possible of all the branches of trade comprised within its title, and as an encouragement to working men to take a direct part in securing the success of this object, it has been decided to allot free space to such of the working classes as may wish to exhibit inventions or models of their own make. Further it is intended to appoint a committee of manufacturing exhibitors for the purpose of awarding three prizes of ten, five and three guineas respectively to the three most deserving exhibitors in this class. The Exhibition has received the certificate of the Board of Trade which protects unpatented inventions which may be exhibited, and reserves the right to the inventor to apply for a grant of letters patent or provisional protection for six months from the date of opening.

SILICA AND LIME-WATER.—M. Ed. Landrin recently brought before the notice of the French Academy of Sciences some experiments which he has made, proving that silica had the property of absorbing lime-water. Four varieties of silica were employed—hydraulic, gelatinous, Graham's soluble silica, and hydrofluoric acid silica. The process consisted in placing 0.03 grammes of silica in 100 cubic centimeters of lime-water, which had been treated with dilute nitric acid, and after a given time separating the silica charged with lime, and estimating the quantity of lime absorbed. Hydraulic silica absorbs more slowly than Graham's soluble silicate; but the silica of hydrofluoric acid absorbs slowest of all. In all cases the final absorption or saturation is for one equivalent of silica from 36 to 38 of lime. The formula $3\ Si\ O_2\ 4\ CaO$, which requires for 30 of silica 37.3 of lime, therefore expresses the limit towards which these puzzuolanic phenomena tend.

ACCUMULATORS AND METALLODION.—Dr. H. Aron, of Berlin, who is well known for his investigations of secondary batteries, has enriched scientific phraseology with the somewhat peculiar term of metallodion, by which he denotes a mixture of any metallic oxide with collodion, the solution of gun-cotton in a mixture of alcohol and ether which is so largely used in photography. Dr. Aron's experiments commenced in the year 1880, when it appears that he, before the proposals of M. Faure became known, used plates consisting of spongy lead, which, however, he found in subsequent experiments suitable only as negative electrodes, the formation of suboxide of lead at the positive electrode probably diminishing the conductivity of the battery. Dr. Aron proposed, it is stated, the use of minium, and once even asked his assistant to prepare a minium plate; he did not carry out the idea, as he considered it impossible to fasten the minium properly to its plate. Later on, however, he prepared a lead plate with a côat of minium and collodion made into a paste, and proceeding on these lines he obtained some interesting results. Metallodion seems to admit of further useful application. If the carbon of a Leclanche cell is brushed over with a paste of pyrolusite and collodion, the carbon rod needs no further adjustment, and may directly be placed with the zinc in the vessel containing the solution of chloride of ammonium without requiring a special porous cell. Dr. Aron further observed that to impart to the lead plate the necessary crystalline structure, it need not be placed in nitric acid previous to its further preparation, it will be sufficient to use a mixture of sulphuric acid with a little nitric acid in the battery, or, better still, to employ at first pure sulphuric acid, and then after the lead has become covered with a thin coating of peroxide, to add the nitric acid, when this peroxide, being practically impenetrable to nitric acid, will prevent any too energetic action. Plates may thus be prepared to the depth of one half millimeter much more quickly than under ordinary circumstances, and they readily acquire a good charge, but they do not appear to be able to hold this charge so long as a Faure or metallodion battery.

THE ELECTROMOTIVE FORCE OF BATTERIES.—Recent experiments by Mr. W. H. Preece, F.R.S., communicated to the Royal Society show that changes of temperature do not practically affect the electromotive force of a battery, but they materially affect the internal resistance. Faraday's observation that the improved current from a heated cell is due to increased conductivity is thus confirmed. Mr. Preece's results also show that of the various forms of batteries in practical use the Daniell is most seriously influenced by variations in temperature, and that in all experiments with that battery either the temperature must be kept constant or frequent measurements should be taken of the internal resistance of the battery and allowance made for the variation. Elaborate curves of variation with temperature in Daniell, bichromate, and Leclanche cells, together with corresponding tables, are given in Mr. Preece's paper. The laborious observations have been made by Mr. Shida. The electromotive forces and resistances of the batteries were measured by the discharge test.

FERRO-PRUSSIATE MULTIPLYING PROCESS.—An improvement has recently been made in this very convenient process for producing copies of drawings in white lines on blue ground by Messrs. Schleicher and Schüll, of Duren, Rhenish Prussia. These enterprising paper manufacturers have introduced a continuous transparent drawing parchment in rolls 40 in. wide, and at a very reasonable cost, which is sufficiently transparent to be used in place of the usual tracing, and is still an excellent drawing paper, with a very fine surface, takes pencil and ink well, and will allow lines in pencil to be rubbed out or ink lines to be either scraped out or washed off the surface. It is, moreover, exceedingly tough and well suited for small scale drawings. The instructions for producing blue prints supplied by the above-mentioned firm are as follows: Ammonia citrate of iron 2lb. 5½ oz. avoir., red prussiate of potash 1lb. 9oz. avoir., dissolve separately in water, mix and make the whole up to one gallon, this solution to be carefully kept from light. Ordinary paper upon which the copy is to be produced is then well brushed over with the solution in a dark room and there left to dry. The drawing in transparent parchment or a tracing is then placed in a copying frame with its face to the glass, a piece of ferro-prussiate paper is placed behind and the frame closed, taken out of the dark room and exposed to sunlight. The yellowish green color of the prepared paper changes through bluish green and bluish grey tints into an olive green with metallic reflections; at this stage the process must be interrupted, the frame taken back to the dark room and opened, the drawing washed in cold rain water until the lines are pure white on blue ground, when it can be dried between blotting paper. To be able to watch the progress of the process better, it is advisable to leave the ferro-prussiate paper longer than the frame; the exposure varies with the intensity of the light from five to thirty minutes, the correct time for stopping is soon learned by experience.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXIX.—NOVEMBER, 1883.—VOL. XXIX.

THE TWO-CYLINDER COMPOUND ENGINE IN WHICH THE STROKES ARE SIMULTANEOUS, OR CO-INITIAL AND CO-TERMINAL, WITH RECEIVER, CUSHION, CLEARANCE, ETC.

By S. W. ROBINSON, C. E., Prof. Mech. Eng., Ohio State University, Columbus, Ohio.

Written for Van Nostrand's Engineering Magazine.

II.

Compound Pumping Engines.

In rotative compound pumping engines any of the foregoing cases will apply as well as for other purposes. But for non-rotative pumping engines some difficulty is experienced in maintaining motion near the end of the stroke, due to the fall of pressure of the expanding steam.

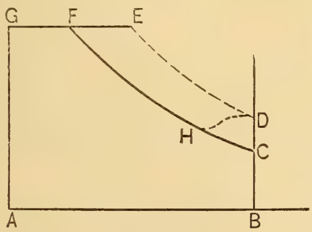


Fig. 11.

To admit a puff of steam near the end of stroke for a last lift that shall carry the engine over is very wasteful of steam. To illustrate by a diagram, suppose the expansion be carried from F to H, Fig. 11, at which latter point the engine falters. Now to admit a portion of steam that shall cause the pressure to rise to D, and add the area HDC over what it would have been without the added steam; the effect is a loss of work from what it would have been had the same steam been admitted at the beginning of the stroke as represented by the area HFEDH, and the expenditure of steam is increased by the ratio EG÷FG. (This estimate, however, will be slightly modified by the fact that the steam thus admitted is reduced in pressure at the admission and superheated somewhat.)

Owing to this difficulty it is customary to use no cut-offs on the cylinders, but to secure the expansion by passing the steam from the high-pressure into the low-pressure cylinder.

If there be no receiver the diagram is of the kind shown in Fig. 9, and the work performed per stroke is given by equation (12) for $r_1=1$. An example of such an engine is shown in Fig. 1, though the intermediate steam pipe must have the effect of a receiver in small degree.

If there be a receiver and no cut-offs, the diagram is shown in Fig. 4. In practice the communicating passage between the cylinders amounts to a small

receiver in effect, however small it may be, and the expression of amount of work per stroke is given in (14). The receiver has the effect to reduce the work per stroke by the ratio, see Fig. 4,

$$\frac{QFLINOQ}{QFLKNOQ} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

if there be no reheating in the receiver. But in reheating great advantages may arise, as already considered.

The Tank Engine.*

An interesting form of compound engine for pumping has been introduced to some extent by H. R. Worthington, and by Cope and Maxwell, in which there are two cylinders, one larger than the other, and an intermediate "tank" or receiver.

The high-pressure cylinder in this engine is supposed to take its steam at constant pressure from a boiler during its full stroke, then to exhaust into the receiver. The back pressure is the pressure of the steam in the receiver, which will be somewhat variable according to relation of sizes of receiver and cylinders. The low-pressure cylinder steam supply will also vary somewhat in pressure according to pressure in the receiver. This cylinder exhausts into the air, or into a condenser; the back pressure due to which is assumed constant in the calculations.

As to the piston movements three are possible, viz., 1st, they may have entire independence; 2d, they may be limited to an equal number of strokes per minute, with initial points in certain relation; and, 3d, they may be further limited by periods of tarrying. The latter is the case with the engine now considered.

As to the valve movements, four are possible, each differing from another in efficiency of the engine. Two are selected as special objects for investigation here, viz., the one with the lowest efficiency, and that with the highest. But all these efficiencies approach, and, finally, have a common value as the receiver is enlarged to infinity. For this latter and special case the formulas are much simplified.

One important fact to be pointed out is that with a certain mode of working the valves, a comparatively small receiver gives a higher efficiency of steam than a larger, or infinite one.

The engine, with two pistons and one receiver, is briefly shown by diagram in Fig. 12, where b, d, are admission valves

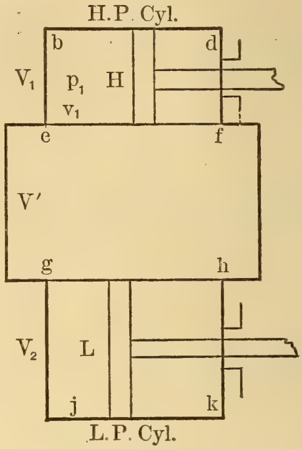


Fig. 12.

for high steam, into small cylinder e, f, exhaust valves from the small cylinder into the receiver; g, h, admission valves from the receiver to the low-pressure cylinder; and j, k, exhaust valves from the low cylinder.

The action of the pistons, except where mentioned as otherwise, is considered as alternating; that is to say, one piston stands still at the end of the cylinder, while the other makes its stroke, and conversely. This is the third above mentioned.

The diagram will enable us to indicate the fact of different ways for working the valves. For instance: 1st, suppose b and f open, e and d closed, piston H moving, and piston L standing at end of cylinder. Then with j and k closed, g and h may be either opened or closed. If closed, the steam from f accumulates only in the receiver, while if open, it flows into both the receiver and L cylinder. This option in the valves g and h occasions two valve movements. Again: 2d, suppose piston L moving, while piston H stands at the end of the stroke, then with b and d closed, e and f may be either opened or closed, a second option which occasions two separate valve movements,

* Extracted from a paper read before the American Society of Mechanical Engineers.

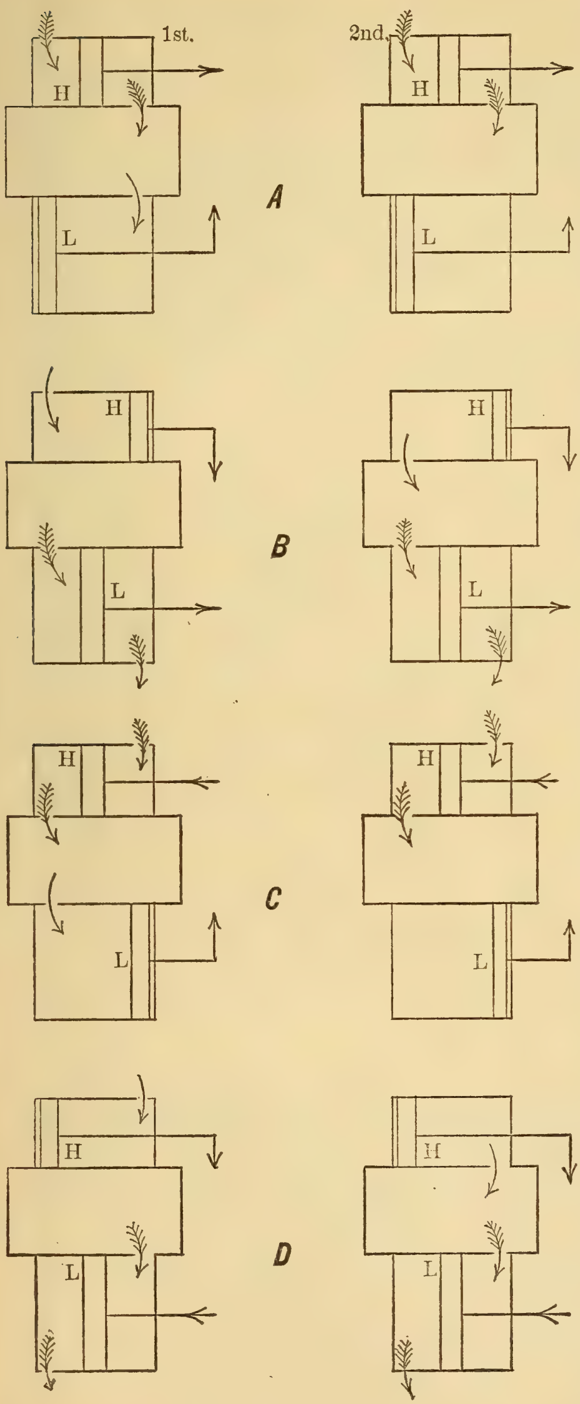


Fig. 13.

for this side of the engine. In the combined action of both sides, it appears from the fact that these options are independent of each other, there results four possible valve movements, two of which, as above mentioned, are selected for investigation and comparison, and are shown in Fig. 13. The 1st is the least, and the 2d the most efficient of the four.

In the 1st, and also 2d, of Fig. 13, the engine is represented in the four relations necessarily undergone for the completing a cycle of operations. For instance in 1st, we have piston H moving while piston L stands, then H stands while L moves, next H moves opposite to the first as L stands, and finally H stands as L moves, contrawise to that of its first stroke. The next move is the same as the first, and thus they continue in repetition. Piston-rods are shown by arrows, which indicate direction of motion. Broken arrows mean standing still.

In Fig. 13 all open valves are indicated by arrows. Feathered arrows are put for valves necessarily open, while naked arrows are at valves in option. In the light of these remarks, the valve movements of 1st and 2d may be traced throughout, also steam action. For convenience let the four relations of Fig. 13 be designated as A, B, C, and D, as shown.

Then in 1st: for the A relation we have H moving with high steam on one side and exhaust on the other. These are necessary conditions while H moves, and hence the arrows are feathered. The exhaust, however, is shown as into the receiver not only, but the L cylinder as well, because the valve to the L cylinder is open. But that valve might be closed when the steam would be confined to the receiver with a more rapid rise of pressure. The

latter condition is shown in the A relation of 2d, hence this arrow is left naked.

Again in the B relation, the H piston is standing while L is making its stroke. The valve between H and the receiver is closed in 1st, or it may be open as in 2d, and hence the arrow is featherless, as shown.

These references suffice to explain the C and D relations also.

V. Solution for the First Valve Movement of Fig. 13.

Before making statements of steam action it will be necessary to know specifically the condition of the steam at every point as it passes through the engine. For the 1st part of Fig. 13, the complete diagram of steam action, for continuity of engine movement, is given in Fig. 14.

Let V_1=volume of the high pressure cylinder, V_2=volume of the low-pressure cylinder, and V'=volume of the receiver. These volumes are shown on the diagram in their proper relations. Also the absolute pressures P_1 of the boiler, P_2 of terminal expansion, and P_3 of the exhaust, are shown. Other pressures will be indicated according to lettering of diagram, as, for instance, $P_i = P_g$ being the pressures at the points I and G. In short, the pressures and volumes are represented by vertical and horizontal distances respectively, with A for the zero or origin.

The Complete Diagram and Indicator Cards.—Now, referring to 1st of Fig. 13, we see that in the A relation the low-pressure or L-piston is standing at the end of its stroke, and with the valve between that cylinder and the receiver open; the steam that made the last L-stroke being retained. Hence the L-cylinder and receiver both together are serving as receiver, while the high-pressure or H-piston is making its stroke and forcing the steam from the H-cylinder into the receiver. The H-piston is moved by steam from the boiler, the same being admitted at full pressure P_1, and full stroke.

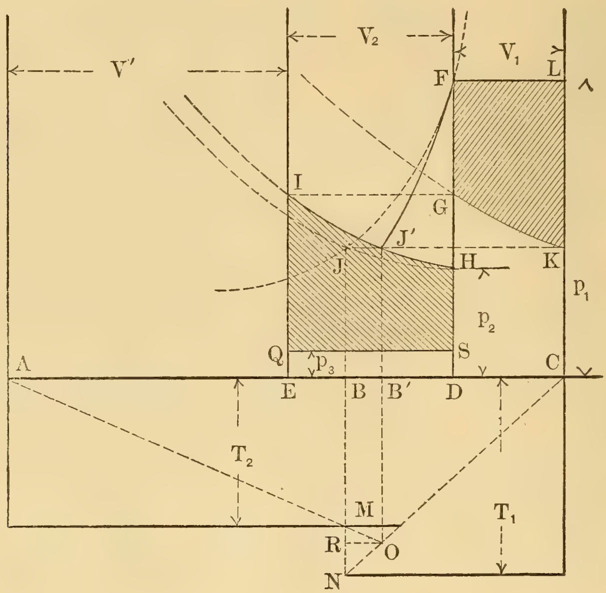


Fig. 14.

Now in Fig. 14, FL represents this high-pressure line of P_1, while KG represents the compression line of the H-piston stroke. It is to be observed that at the initial return of H, the volume is $AC = V_1 + V_2 + V'$; and at the end, it is $AD = V_2 + V'$.

Next, in the B-relation of 1st of Fig. 13, the valves of L are reversed, as H

completes its stroke, the steam in the L-cylinder being exhausted. In Fig. 14 this exhaust is shown by a cutting off of the steam GI, and leaving only that in the receiver, of volume $V' = AE$, to act upon the L-piston.

The H-piston now stands while the L-piston makes its stroke with a change of active steam volume from V' to $V' + V_2$; and producing the expansion line IH, Fig. 14. Next the valves of L, Fig. 13, remain, while the valves of H reverse; so that the L-piston stands while the H-piston moves as in the C movement.

Now as the valves of H reverse, the high steam exhausts into the receiver and L-cylinder together, giving the expansion line FJ for the high steam, and the compression line HJ for the low steam, till both arrive at a common pressure at J. The expansion line FJ is to be drawn with C as the point of zero volumes and pressures; while HJ has A for the like point. Here the steam, isodynamically brought down from P_1 at F, will be hotter than that brought up from P_2 at H; and as they commingle, the volume JK decreases by giving up heat, and the volume AB increases by receiving that heat. But as the same weight of steam must necessarily be exhausted from the high cylinder as from the low at a stroke, it follows that the steam JK introduced must have equal weight with the steam GI exhausted. Hence the volume JK must shrink by cooling to some volume J′K, such that J′ lies on the expansion curve IH; because when AC is compressed along KG to AD, then J′K must compress to GI, and AB′ to AE. The curves KG and J′I are of like sort, with the point A for the zero of pressures and volumes.

Now H commences its stroke with a back pressure $P_j = CK$, and the volume $AC = V_1 + V_2 + V'$. At the end of the stroke, this volume has been reduced to AD, giving the compression line KG. At the end of this stroke the valves of L are reversed, causing the immediate exhaust from L of the volume $GI = V_2$, when in the L-stroke the expansion curve IH, etc., is formed as before.

Thus it appears that FGKL is the indicator card of the H-cylinder, while HIQS is the card for the L-cylinder. The work done by these cylinders will be proportional to these areas. The back pressure is $EQ = P_3$.

Nature of the Expansion and Compression Lines.—Having traced out the action of the engine and of the steam as it is worked in it, we are prepared to inquire into the specific nature of the various lines of the complete diagram, Fig. 14, from the standpoint of thermodynamics. Thus the compression curve KG is adiabatic, and such that if the steam is saturated at K, it is superheated at G by the compression. Hence, as the steam will be shown to be not far from saturated at K, this curve is an adiabatic for superheated steam. The same is true of J′I, because as K is compressed to G, J′ is compressed to I. After the exhaustion of the volume GI, the curve IJ′ is retraced by expansion with the same steam as was just compressed along J′I. But with J′H the case is to some extent different, before explaining which, we must consider JH and JF. With respect to the latter, it is to be observed that at the end of the stroke of the L-piston, the volume $V' + V_2$ is ready to receive the high steam exhaust from the H-cylinder. Here the low-steam pressure is $DH = P_2$, and high-steam pressure $DF = P_1$; while the volumes are $V' + V_2 = AD$, and $V_1 = DC$, respectively. Now as the valve from H to the receiver opens, these two portions of steam coalesce * into a common body, whose volume is simply the sum of the previous partial volumes; that is to say,

$$(V_2 + V') + V_1 = V_1 + V_2 + V';$$

and the resulting pressure is BJ. In this act no exterior work is performed by the mass of steam in the volume AC, because, for the instant during which this commingling occurs, the pistons of the engine do not move perceptibly. This act is therefore governed by *Hirn's law*; that is to say, because no external work is done during the change, the internal energy of the mass of steam remains constant. The curve representing this action is the so-called *isodynamic curve.* Hence the pressure BJ is to be found at the inter-

* This, however, is only partially true, since some of the steam remains in the *H*-cylinder till forced out through the open valve into the receiver during the return stroke. Though the pressure is common, yet the temperature and density of that remaining in the cylinder will probably differ somewhat from that in the receiver.

section of two isodynamic lines FJ and HJ; the first representing the isodynamic expansion of the volume V_1, and the second, the compression of the volume $V'+V_2$.

Though these curves serve to determine the point J, or resulting pressure BJ, yet it is to be understood that they do not represent the operations which actually take place in the two portions of steam, because the low steam is in reality compressed adiabatically from H to J′, and the work B′DHJ′ performed. This work is performed by the expansive action of the high steam while actually expanding along some line FJ′ and not FJ. This curve, however, will lie above the adiabatic through F, because in expanding along FJ′ the steam is not doing its full work, the work actually done being only B′DHJ′ instead of an area extending fully up to the expansion line. Hence, J′ instead of J, represents the actual resulting point of pressure.

Now the points J and J′ are expected to be *very nearly* in a horizontal line, but may not be exactly so for several practical reasons. The expansion J′ H will probably be mostly adiabatic below the saturation point, the point of change from superheat to supersaturation lying somewhere on J′H, but not far from J′. Much of the supersaturation moisture due to this expansion may be precipitated while the L-piston tarries at the end of its stroke, so that the compression heat of HJ′ acts at a disadvantage in re-evaporating that moisture. The moisture thus precipitated may accumulate in the receiver as water, and require to be drawn off by a cock. To show that at the end of the stroke IH the steam is necessarily below the saturation point, we observe that KG and J′I are counter actions which offset each other, as already explained, and hence have no influence on the final condition of saturation. Now, referring to the isodynamic lines, the expansion FJ gives rise to superheat, while the compression HJ occasions condensation. These actions nearly compensate each other, because, though the intensity for FJ is greater than for HJ, yet the quantity of steam concerned in JH is greater : assuming that these exactly neutralize, then the saturation point lies very near to J′, and below it if supersaturation moisture is precipitated in the receiver.

It appears, therefore, that the expansion line IH is of two kinds, viz., from I to some point near J′ it is adiabatic for superheated steam, while from the latter point to H it is adiabatic for supersaturated steam.

To account for JJ′ on the Supposition of Heat.—In this case the drawing-board may be preferred. Use temperatures and a diagram, as shown in the lower part of Fig. 14. As the temperatures for the isodynamic lines through J are each nearly constant, let the absolute temperatures τ_1 and τ_2 of the steam at admission and at the end of J′H be laid off downward, as shown. Draw lines to M and N, meeting JMN. Then draw straight lines AMO and CNO, giving the intersection O. Then the line through O parallel to NJ should give the point J′. The diagram ANC depends upon the law of Gay Lussac, relating to volume and temperature, for constant pressures in gases, viz., by symbols

$$\frac{v}{\tau}=\frac{v'}{\tau'}$$

In superheated steam this very nearly holds true. In the diagram the application is AB : BM : : RO : RM, and CB : BN : : RO : RN. The two portions of steam are first supposed reduced to the common pressure P_j, by the isodynamic or nearly constant temperature expansion FJ, and the like compression HJ. The volume BC then has the temperature τ_1 of P_1, and the volume AB the temperature τ_2 of p_2. The change JJ′ is that by which the portion BC shrinks, and of AB expands, while the temperatures τ_1 and τ_2 merge into one temperature, $\tau_j =$ BR.

Repeated trials should be made until JJ′ is a horizontal line from the intersections J of the isodynamics to the adibiatic IH, and with J′K and GI also horizontal.

But as the correction to the volume AB $=V_j$ is usually less than one per cent., it is probable that the discrepancies in results of efficiency or duty of engine due to it will be too small to merit serious consideration.

Hence, in the following general equations the points J and J′ will be regarded

as coincident; and the entire expansion line IH will be treated as of one form, or as characterized by one value of the exponent x in Eq. (1).

Area of the Indicator cards; and General Equations,—To find the area of the diagram, we have from equation (6) and Fig. 14, observing that the rectangle of the pressure and volume of the initial point of the curve GK is AD×DG, and that the ratio of expansion is AC÷AD,

area GDCK=

$$P_g \frac{V_2+V'}{a-1} \left\{ 1 - \frac{1}{\left(1+\frac{V_1}{V_2+V'}\right)^{a-1}} \right\}$$

$$= P_g V_1 M \quad . \quad . \quad . \quad (30)$$

if

$$M = \frac{V_2+V'}{V_1(a-1)} \left\{ 1 - \frac{1}{\left(1+\frac{V_1}{V_2+V'}\right)^{a-1}} \right\} \quad (31)$$

For the particular case $a = 1$, the integral becomes, for the small cylinder

$$\left.\begin{aligned} (GDCK)_{a=1} &= AP_g L_1 \text{hyp.log.}\left(1+\frac{l}{L_1}\right) \\ &= P_g(V_2+V')\text{hyp. log.}\left(1+\frac{V_1}{V_2+V'}\right) \end{aligned}\right\} \quad (32)$$

In these equations P_g is the absolute pressure DG, =EI.

Similarly for the area IEDH, taking the point A, Fig. 14, as the zero of volumes, we have by aid of (6)

$$IEDH = P_g \frac{V'}{a-1} \left\{ 1 - \frac{1}{\left(1+\frac{V_2}{V'}\right)^{a-1}} \right\}$$

$$= P_g V_2 N \quad . \quad . \quad . \quad (33)$$

if

$$N = \frac{V'}{V_2(a-1)} \left\{ 1 - \frac{1}{\left(1+\frac{V_2}{V'}\right)^{a-1}} \right\} \quad . \quad . \quad (34)$$

For $a=1$, we have

$$(IEDH)_{a=1} = P_g V' \text{ hyp. log.}\left(1+\frac{V_2}{V'}\right).$$

Now the work done in the high-pressure cylinder, per stroke, is

$$P_1 V_1 - GDCK.$$

Also in the low-pressure cylinder it is

$$IEDH - P_3 V_2.$$

Hence, when the engine is working in continuity under the first valve movement of Fig. 13, and giving the diagram of Fig. 14, then, for one stroke of each piston in regular order of continuity, the same being the half of a complete cycle of operations detailed in Fig. 13, or, for each high-pressure cylinder full of steam, the work done per stroke of both cylinders is $U=(P_1V_1-GDCK)+(IEDH-(P_3V_2)$

$$U = P_1V_1 - P_g V_1 M - P_3 V_2 + P_g V_2 N \quad (35_3)$$

all being in known terms except P_g. Fig. 14 shows that this depends on many things, and that it is in no case, in itself, arbitrary, but that, in an actual engine it is self-adjusting according to pressure of admission, dimensions of cylinders and receiver, mode of valve action, and laws of expansion.

To express P_g in convenient terms, we may write from Fig. 14, and by aid of (1) and the following table, regarding J and J′ as coincident,

$$\frac{P_1}{P_j} = \left(\frac{JK}{FL}\right)^n = \left(\frac{JK}{V_1}\right)^n.$$

Also,

$$\frac{P_i}{P_j} = \frac{P_g}{P_k} = \frac{P_g}{P_j} = \left(\frac{AC}{AD}\right)^a$$

$$= \left(\frac{V_1+V_2+V'}{V_2+V'}\right)^a = \left(1+\frac{V_1}{V_2+V'}\right)^a$$

Again, from the fact that the intercepts on horizontal lines lying between two curves constructed from (1) with one value for x and different values of $P_1 V_1$, are proportional to the abscissas to the intersection points of the horizontal lines with either curve, we may write,

$$\frac{JK}{GI} = \frac{JK}{V_2} = \frac{AC}{AD} = \frac{V_1+V_2+V'}{V_2+V'}$$

$$= 1 + \frac{V_1}{V_2+V'}.$$

Eliminating JK and combining, we get

$$\frac{P_g}{P_j} \cdot \frac{P_j}{P_1} = \frac{P_g}{P_1}$$

$$= \left(1+\frac{V_1}{V_2+V'}\right)^a \cdot \left(\frac{V_1}{V_2}\right)^n \cdot \left(1+\frac{V_1}{V_2+V'}\right)^{-n}$$

$$= \left(\frac{V_1}{V_2}\right)^n \cdot \left(1+\frac{V_1}{V_2+V'}\right)^{a-n} \quad . \quad . \quad . \quad (36)$$

Eliminating $\frac{P_g}{P_1}$ by aid of (36), we find for the work of the half cycle, or for one

stroke each of the two cylinders in continuity,

$$U=P_1V_1\left\{1+\left(\frac{V_1}{V_2}\right)^n\cdot\left(1+\frac{V_1}{V_2+V'}\right)^{a-n}\cdot\left(\frac{V_2}{V_1}N-M\right)-\frac{P_3V_2}{P_1V_1}\right\}\quad . \quad . \quad . \quad (37)$$

and for the hyperbolic expansion, where a and $n=1$, we have

$$U'=P_1V_1\left\{\begin{array}{l}1-\frac{V_2+V'}{V_2}\text{hyp.log.}\left(1+\frac{V_1}{V_2+V'}\right)\\-\frac{P_3V_2}{P_1V_1}+\frac{V'}{V_2}\text{hyp.log.}\left(1+\frac{V_2}{V'}\right)\end{array}\right\}\quad(38)$$

all quantities being in known terms. If $a=n$, the term $\left(1+\frac{V_1}{V_2+V'}\right)$ drops from (37).

If $V'=\infty$, and a and n as in (37), we get

$$U''=P_1V_1\left\{1-\left(\frac{V_1}{V_2}\right)^n+\left(\frac{V_1}{V_2}\right)^{n-1}-\frac{P_3V_2}{P_1V_1}\right\}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

If $V'=\infty$, and a and $n=1$, we get

$$U'''=P_1V_1\left(2-\frac{V_1}{V_2}-\frac{P_3V_2}{P_1V_1}\right)\quad . \quad . \quad (40)$$

In these expressions,

P_1=absolute pressure of admission, lbs. per square foot=144 ($p_1'+14.7$), for p_1'=lbs. per square inch of boiler pressure.

P_3=absolute back pressure to low-pressure cylinder lbs. per square foot of the atmosphere for exhausting into air, or of the condenser for condensing engines.

V_1=volume of high-pressure cylinder in cubic feet.

V_2=volume of low-pressure cylinder in cubic feet.

V'=volume of receiver and connecting pipes in cubic feet.

M and N being given by equations (31) and (34).

These equations are useful in calculating the amount of work that can be done by an engine, or in calculations for duty.

In practice any one of these equations can be used according to accuracy required, (37) being necessary for the greatest degree of exactness. It is seen, however, by (1) and the accompanying table that a much wider departure from truth results when $a=1$ than when $n=1$. When the steam is very wet the actual value of a will be much nearer unity than for dry steam. In such case some value of m should be used in place of a. Fig. 14, also, will indicate in some degree to what extent, in any practical case, we jeopardize the result by taking V' infinite in the calculations.

Pressure at Different Points of Steam Action.—It is often desirable to know the pressure at different points of the steam action in the engine. To this end we have (36) for the terminal pressure, p_g, in the high cylinder, and initial pressure in the low cylinder. This shows that if $a=n$ it matters not with p_g whether $V'=\infty$ or not. From this it appears that for $a=n$ the smaller we make V' the greater is the proportion of work done by the small and less by the large cylinder.

For the initial and final pressures in the low-pressure cylinder we have the relation

$$\frac{P_g}{P_2}=\left(\frac{V_2+V'}{V'}\right)^a=\left(1+\frac{V_2}{V'}\right)^a\quad . \quad . \quad (41)$$

combining with (36)

$$\frac{P_2}{P_1}=\left(\frac{V'}{V_2+V'}\right)^a\cdot\left(\frac{V_1}{V_2}\right)^n\cdot\left(1+\frac{V_1}{V_2+V'}\right)^{a-n}\quad(42)$$

The 4th equation preceding (36), combined with the 2d, gives

$$\frac{P_1}{P_k}=\frac{P_1}{P_j}=\left(\frac{V_2}{V'}\right)^n\cdot\left(1+\frac{V_1}{V_2+V'}\right)^n\quad . \quad . \quad (43)$$

These equations show that for $V'=\infty$ we have

$$\frac{P_g \text{ or } P_j \text{ or } P_2}{P_1}=\left(\frac{V_1}{V_2}\right)^n\quad . \quad . \quad (44)$$

which is the constant pressure of the infinite receiver, and toward which this pressure approximates as V' is made large.

Equation (36) shows that if $a=n$, as it will very nearly for wet steam, the pressure at G or I, $=P_g$, will be entirely independent of V' whether infinite or finite. The effect of this on the diagram is that the lines GK, or IH, swing around the points G and I as pivots, becoming flatter as V' is enlarged, or steeper as it is diminished. Hence, as V' is enlarged,

the work produced in V_1 decreases, while in V_2 it increases. Considering these variations, a look at the diagram, Fig. 14, shows that the efficiency of this engine increases with V', and is a maximum when V' is infinite.

When $V'=0$, V_2 does no work, and the engine does worse than a non-compound, but equals it when IQSH=GSS′K.

Relative Areas of Indicator Cards.—It is also desirable to know the relation between the work developed in the high-pressure cylinder and the low. From the expressions (35) and (36) given above, we readily find,

$$\frac{\text{Wk. H.-Cyl.}}{\text{Wk. L.-Cyl.}}$$

$$=\frac{1-M\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_1}{V_2+V'}\right)^{a-n}}{N\frac{V_2}{V_1}\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_1}{V_2+V'}\right)^{a-n}-\frac{P_3V_2}{P_1V_1}} \quad . \text{ (45)}$$

For the hyperbolic expansion this ratio is

$$=\frac{1-\frac{V_2+V'}{V_2}\text{hyp. log}\left(1+\frac{V_1}{V_2+V'}\right)}{\frac{V_1}{V_2}\text{hyp. log.}\left(1+\frac{V_2}{V'}\right)-\frac{P_3V_2}{P_1V_1}} \quad . \text{ (46)}$$

If $V'=\infty$, and a and n are as in (45)

$$=\frac{1-\left(\frac{V_1}{V_2}\right)^n}{\left(\frac{V_1}{V_2}\right)^{n-1}-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad \text{(47)}$$

If $V_1=\infty$ and a and $n=1$

$$=\frac{1-\frac{V_1}{V_2}}{1-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad . \quad \text{(48)}$$

Heat for Producing the Steam.—The total heat consumed per high-pressure cylinder full of steam in dynamical value, or ft.-lb. units, is, see equation (2),

$$H=V_1D_1[H_1+J(t_f-32°)] \qquad \text{(49)}$$

and the efficiency is given by (3).

Maximum Efficiency.—In the compound engine now considered, if the two cylinders are of nearly equal size, one does almost no work, with slight expansion, and with a corresponding low efficiency. On the other hand, if there be a very great disparity of cylinder sizes, the low-pressure cylinder may exhaust the steam so rapidly from the receiver as to carry its pressure as low or even lower than the back pressure. At the limit of equal back and receiver pressure, no work will be done by the low-pressure cylinder, and the high-pressure cylinder exhausts, in effect, into the back pressure direct. Here again we have no working expansion, and a correspondingly low efficiency. It is evident that between these limits there exists a relation of sizes which will give a maximum of efficiency.

To determine this relation, assume a fixed volume of V_1 and of V', while V_2 is made to vary. In this way the quantity of steam used per stroke is invariable, so that the denominator of (3) is constant.

Hence, for the maximum of (3), we have only to examine the numerator, or to examine (37), (38), or (39), etc., according to contemplated accuracy. This could be done by working out several values from which to construct a curve, the maximum ordinate of which corresponds to the maximum sought. This plan is probably advisable for (37) and (38).

If we place the differential co-efficient of U'' with respect to V_2 from (39) equal zero, we get after reduction, for the case $V'=\infty$,

$$\frac{P_3}{P_1}=\left(\frac{V_1}{V_2}\right)^n\left(n\left(\frac{V_1}{V_2}\right)-(n-1)\right) \quad . \text{ (50)}$$

an equation which is irresolvable for the desired quantity, viz., $\frac{V_1}{V_2}$. But for a series of assumed values of $\frac{V_1}{V_2}$ the corresponding values of $\frac{P_3}{P_1}$ may be computed and tabulated. A sufficiently extended table would answer all cases, requiring conditions for the maximum efficiency.

If $n=1$, then (50) reduces to

$$\frac{P_3}{P_1}=\left(\frac{V_1}{V_2}\right)^2 \quad . \quad . \quad . \quad \text{(51)}$$

Eliminating $\frac{P_3}{P_1}$ between (50) and (39) we find the maximum of U'', or for the infinite receiver, with n as in (39);

$$U''_{\text{max.}}=P_1V_1\left\{1-\left(\frac{V_1}{V_2}\right)^n\left(1+n-n\frac{V_2}{V_1}\right)\right\} \quad . \quad . \quad . \quad . \quad . \quad \text{(52)}$$

From this, for $n=1$, or for (51) in (40) we obtain for $V'=\infty$ and $n=1$ the maximum of U'''

$$U'''_{max.}=2P_1V_1\left(1-\frac{V_1}{V_2}\right)$$

$$=2P_1V_1\left(1-\left(\frac{P_1}{P_3}\right)^{½}\right) \quad . \quad . \quad (53)$$

Conditions for Equal Work of Cylinders.—In practice it is probably desirable that the cylinders do equal work, particularly so in the Worthington pumping engine, where the especially important point is made of destroying the "water hammer" in connection with the water

If $n=1$, also,

$$\frac{P_3}{P_1}=\left(\frac{V_1}{V_2}\right)^2 \quad . \quad . \quad . \quad (56)$$

This last is the same as (51), which gives the condition for a maximum efficiency. Hence, for the case that the receiver has an infinite volume, and $n=1$, the engine works with its maximum efficiency when the cylinders do equal portions of the work. When the receiver is eight or ten times as large as the low-pressure cylinder, this engine, with its valves, working as stated, is not far from working with its maximum efficiency.

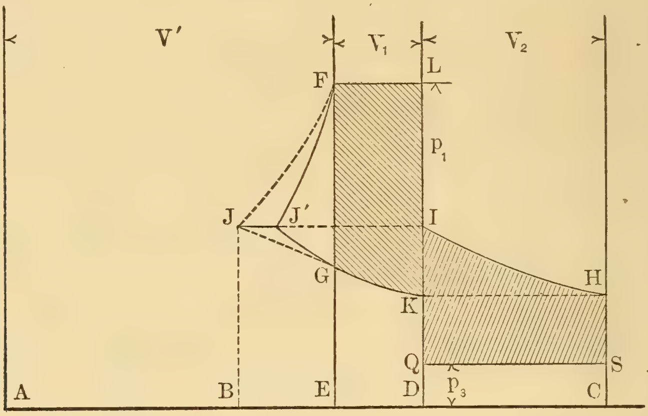


Fig. 15.

valve action, and of maintaining perfect uniformity of pressure and motion of the traveling water column.

To obtain an equation which shall express the conditions necessary for such equality of work of the two cylinders, we have only to place the numerator and denominator of (45) equal to each other, which becomes

$$\frac{P_3V_2}{P_1V_1}=\left(M+N\frac{V_2}{V_1}\right)\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_1}{V_2+V'}\right)^{a-n}-1$$

$$. \quad . \quad . \quad . \quad (54)$$

In using this equation it will be necessary to assume a set of volumes and find the ratio $\frac{P_3}{P_1}$, a satisfactory value for which may require several trials.

If $V'=\infty$ (54) reduces to

$$\frac{P_3V_2}{P_1V_1}=\left(1+\frac{V_2}{V_1}\right)\left(\frac{V_1}{V_2}\right)^n-1 \quad . \quad . \quad (55)$$

VI. Solution for the Second Valve Movement of Fig. 13.

The complete diagram of steam action in continuity for this case is shown in Fig. 15. The notation is as before, AC being the volume of the three parts V_1, V_2, and V'.

The piston motions are the same here as shown in Fig. 13, and, indeed, the same as in V. The sole cause for the difference in diagrams is in the valve movement, as indicated in the second part of Fig. 13.

The Complete Diagram and Indicator Cards.—When the H-piston has completed its forward stroke, all under the full boiler pressure P_1, the boiler steam is to be immediately cut off, and to be exhausted into the receiver. The resulting pressure is $BJ=DI=P_j$. The valve remains open, and ths H-piston stands,

while the L-piston makes its stroke. The latter starts while the volume of active steam is that in the receiver and H-cylinder together, and equals $AD = V_1 + V'$. Hence, the expansion line for this stroke is IH, drawn with the point A for the zero of volumes. Immediately on completing the stroke, the admission valve from the receiver is closed, retaining the steam in the L-cylinder while its piston stands. Consequently the H-piston starts its stroke with a back pressure $DK = CH$ of the terminal stroke of the L-piston. The volume subject to this back pressure is AD at the start of the H piston, but during this stroke it is compressed to AE, giving the compression line KG. Then the valve immediately opens, as explained, to release the high steam which made this stroke. This steam expands against that in the receiver, which was compressed on the back stroke, giving the actual compression curve GJ′ and expansion curve FJ′; the pressures EG and EF meeting in a common pressure at J′. Then the L-cylinder starts again on the curve IH, while the H-piston stands, and so on in repetition.

The valve movement is thus seen to be peculiar. Here, instead of the valves of one cylinder all moving simultaneously, and so with the other cylinder, they move in alternation. This will complicate the valve-gear somewhat.

It is to be observed that by this arrangement, the high steam is at once released from the H-cylinder on completion of stroke, so that it is available to the L-cylinder. Also, in the L-cylinder, the steam is retained while that piston stands, instead of being at once exhausted; the object being to keep this cylinder hot as possible.

Nature of the Expansion and Compression Lines.—The point of zero of volumes for the curves HI and KGJ′ or J, is A, Fig. 15, while for FJ, or FJ′, it is D The back pressure line of the L-cylinder is QS.

Area of Diagrams and General Equations.—By aid of (7) and Fig. 15 we are able to write out

$$\text{Area GEDK} = P_k \frac{V_1 + V'}{a-1}\left\{\left(1 + \frac{V_1}{V'}\right)^{a-1} - 1\right\}$$

$$= P_k V_1 Q \quad . \quad . \quad . \quad (57)$$

if

$$Q = \frac{V_1 + V'}{V_1(a-1)}\left\{\left(1 + \frac{V_1}{V'}\right)^{a-1} - 1\right\} \quad . \quad (58)$$

and

$$\text{IDCH} = P_k \frac{V_1 + V_2 + V'}{a-1}\left\{\left(1 + \frac{V_2}{V_1 + V'}\right)^{a-1} - 1\right\}$$

$$= P_k V_2 S \quad . \quad . \quad . \quad (59)$$

if

$$S = \frac{V_1 + V_2 + V'}{V_2(a-1)}\left\{\left(1 + \frac{V_2}{V_1 + V'}\right)^{a-1} - 1\right\} (60)$$

For the particular case that $a = 1$, we have

$$(\text{GEDK})_{a=1} = P_k(V_1 + V')\,\text{hyp.log.}\left(1 + \frac{V_1}{V'}\right) \quad . \quad . \quad . \quad (61)$$

and

$$(\text{IDCH})_{a=1}$$

$$= P_k(V_1 + V_2 + V')\,\text{hyp.log.}\left(1 + \frac{V_2}{V_1 + V'}\right) (62)$$

in all of which P_k = the pressure DK = CH.

Now the work done in the high-pressure cylinder will be

$$\text{FGKL} = P_1 V_1 - \text{GEDK} \quad . \quad . \quad (63)$$

and in the low-pressure cylinder it will be

$$\text{IQSH} = \text{IDCH} - P_3 V_2 \quad . \quad . \quad (64)$$

For the engine working in continuity the work developed during a half cycle, or for one stroke of each cylinder, or by each high-pressure cylinder full of steam, will be the sum of (63) and (64), or

$$U = (P_1 V_1 - \text{GEDK}) + (\text{IDCH} - P_3 V_2)$$
$$= P_1 V_1 - P_k V_1 Q - P_3 V_2 + P_k V_2 S \quad . \quad . \quad (65)$$

But in these equations P_k is as yet unknown. Regarding J and J′ as coincident on the adiabatic KG produced, then from the proportionality of horizontal intercepts between adiabatics, we may write

$$\frac{HK}{IJ} = \frac{AC}{AD} = \frac{V_2 + V_1 + V'}{V_1 + V'} = 1 + \frac{V_2}{V_1 + V'}$$

$$= \frac{V_2}{IJ} = \left(\frac{P}{P_k}\right)^{\frac{1}{n}} = 1 + \frac{V_2}{V_1 + V'}.$$

Also,

$$\frac{P_i}{P_1} = \left(\frac{V_1}{IJ}\right)^n = \left(\frac{V_1}{V_2}\right)^n \left(1 + \frac{V_2}{V_1 + V'}\right)^n . (66)$$

Hence

$$\frac{P_i P_k}{P_1 P_i} = \left(\frac{V_1}{V_2}\right)^n \left(1 + \frac{V_2}{V_1 + V'}\right)^{n-a} = \frac{P_k}{P_1} = \frac{P_2}{P_1} \ (67)$$

This introduced in (65) gives in known terms the amount of work per high-pressure cylinder full of steam, viz.:

$$U=P_1V_1\left\{1+\left(\frac{V_2}{V_1}S-Q\right)\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_2}{V_1+V'}\right)^{n-a}\right.$$
$$\left.-\frac{P_3V_2}{P_1V_1}\right\} \quad . \quad . \quad . \quad . \quad (68)$$

If $a=n$, the term $\left(1+\frac{V_2}{V_1+V'}\right)$ drops out. For a and $n=1$,

$$U'=P_1V_1\left\{\begin{array}{l} 1-\frac{V_1+V'}{V_2}\text{hyp.log.}\left(1+\frac{V_1}{V'}\right) \\ -\frac{P_3V_2}{P_1V_1}+\left(1+\frac{V_1+V'}{V_2}\right)\times \\ \qquad \text{hyp. log.}\left(1+\frac{V_2}{V_1+V'}\right) \end{array}\right\}$$
$$. \quad . \quad . \quad . \quad . \quad (69)$$

If $V'=\infty$, and a and n as in (68), we get

$$U''=P_1V_1\left\{1-\left(\frac{V_1}{V_2}\right)^n+\left(\frac{V_1}{V_2}\right)^{n-1}-\frac{P_3V_2}{P_1V_1}\right\}$$
$$. \quad . \quad . \quad . \quad (70) \text{ or } (39)$$

the same as (39).

If $V'=\infty$, and a and $n=1$, we get

$$U'''=P_1V_1\left(2-\frac{V_1}{V_2}-\frac{P_3V_2}{P_1V_1}\right) \quad . \quad (71) \text{ or } (40)$$

the same as (40).

One fact to be observed particularly, in comparing these equations with those for the V solution, is the perfect agreement of the expressions for the work U as soon as V' is made infinite. This is evidently as it should be, since for an infinite volume of receiver it would not matter whether the high pressure exhausted into it immediately on completion of stroke, or whether the exhaust were stayed each time during the half stroke; because the pressure could not vary appreciably in the infinite receiver during this time. For the infinite receiver it is only essential that equal weights be received from the high cylinder, and delivered to the low cylinder per stroke, the strokes being regarded as the same per minute for one as for the other cylinder.

The coincidence of the expressions for work, for the case of an infinite receiver, is therefore expected; and the fact of coincidence corroborates the analysis.

These equations (68) to (71) serve in calculations for duty where the whole work done for a period is compared with the coal consumed. In selecting the equation for use, judgment must be exercised as to the degree of approximation necessary in the case, and as to the proper values of a and n. Wet steam will require different values than dry, the value of a for such case being more properly m, as found in the table following equation (1).

Pressure at Different Points of Steam Action.—To determine the pressure of the steam at different points of action in the engine, we observe first that for $V'=\infty$, the pressures at H, I, J, and K, Fig. 15, become one and the same; that is to say, the pressure in the receiver remains constant, and the indicator diagrams for the two cylinders are simple rectangles.

For the pressure at I or J see Eq. (66).

For the pressure at H or K see Eq. (67), where $P_k=P_2$.

These give the initial and terminal back pressures in the low cylinder, and the initial back pressure in the high cylinder. The terminal back pressure in the high cylinder is P_g =EG, for which, by aid of Fig. 15, we may write

$$\frac{P_g}{P_k}=\left(\frac{V_1+V'}{V'}\right)^a=\left(1+\frac{V_1}{V'}\right)^a.$$

Combining with (67),

$$\frac{P_g}{P_k}\cdot\frac{P_k}{P_1}=\left(1+\frac{V_1}{V'}\right)^a\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_2}{V_1+V'}\right)^{n-a}=\frac{P_g}{P_1}$$
$$. \quad . \quad . \quad . \quad . \quad . \quad (72)$$

If $V'=\infty$, these equations, as well as (66) and (67), reduce to

$$\frac{P_g \text{ or } P_i \text{ or } P_2}{P_1}=\left(\frac{V_1}{V_2}\right)^n \quad . \quad . \quad (73)$$

a common pressure, or a constant pressure of receiver, as above stated, for the case the receiver is infinite in volume. This equation also shows that n is the only exponent upon which the pressure of the infinite receiver depends. It also gives the pressure to which that of the receiver approaches as V' is made relatively very large.

Equation (67) shows that if $a=n$, as it will nearly for wet steam, the pressure at H or K, $=P_2$, will be entirely independent of V' whether $V'=\infty$ or not. In this case the larger the receiver the greater will be the proportion of work done by the high cylinder, and the less by the low

cylinder, as indicated by Fig. 15. The pressure P_2 is here the pivotal pressure.

It is a curious fact that for $a=n$, the so-called pivotal pressures, for both valve movements shown in Fig. 13 have one and the same value; as indicated by equations (36) and (67) for $a=n$. That is to say, for given initial pressures and ratio of cylinder volumes the pressures of P_g of Fig. 14, and P_k of Fig. 15, are equal and constant, whatever the value of V'. Again it is immaterial to this pressure whether $a=n$, or $V=\infty$; and it is given by (44) or (73), another evidence that for the infinite receiver the mode of operation of valves is unimportant, whether according to 1st or 2d of Fig. 13.

The ratio of equations thus:

$$\frac{(42)}{(67)}>1$$

shows that the second valve movement results in a greater ratio of expansion than the first when V' is not infinite.

Relative areas of Indicator Cards.—To find the relation between the amounts of work developed by the high pressure cylinder and the low, we may take their ratio thus:

$$\frac{\text{Wk.H.Cyl.}}{\text{Wk.L.Cyl.}}=\frac{1-Q\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_2}{V_1+V'}\right)^{n-a}}{S\frac{V_2}{V_1}\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_2}{V_1+V'}\right)^{n-a}-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad (74)$$

For a and $n=1$;

$$=\frac{1-\frac{V_1+V'}{V_2}\text{ hyp.log.}\left(1+\frac{V_1}{V'}\right)}{\left(1+\frac{V_1+V'}{V_2}\right)\text{hyp.log.}\left(1+\frac{V_2}{V_1+V'}\right)-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad (75)$$

If $V'=\infty$, and a and n as in (74),

$$=\frac{1-\left(\frac{V_1}{V_2}\right)^n}{\left(\frac{V_1}{V_2}\right)^{n-1}-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad (76)$$

If $V'=\infty$, and a and $n=1$,

$$=\frac{1-\frac{V_1}{V_2}}{1-\frac{P_3V_2}{P_1V_1}} \quad . \quad . \quad . \quad . \quad . \quad (77)$$

Equations (76) and (77) are seen to be identical with (47) and (48), which is evidently correct for the infinite receiver.

The heat required for making the steam, also the expression for the efficiency is evidently the same here as in the first form of engine.

Maximum Efficiency.—The maximum of (68) or (69) may be found by the same process as indicated for (37) or (38). The maximum of (70) is the same as for (39), because the equations are identical. Hence for U'' the conditions for a maximum are obtained from (50); also (51) follows for $n=1$. Hence the maximum values of U'' and U''' for the present case are found in equations (52) and (53).

But it is a remarkable fact that the maximum efficiency of this engine is exactly equal to that of the Woolf engine, without receiver, shown in Fig. 9 above. That is to say, when $a=n=m$, equation (15) is the maximum of (68). This can be proved by using for the expression of the work, the same as (68) obtained by aid of (57), and the corresponding one for the L-cylinder; also (67); and finally the relations

$$\frac{V_2}{V_1}=\frac{V_1+V'}{V'}=\frac{V_1+V_2+V'}{V_1+V'},$$

obtained from Fig. 15, on the supposition that V' is made so small that the compression line KG is so raised that it extends from K direct to F. This compression line then becomes a cushion line, such that on the return stroke of the H-piston, the remaining steam is compressed and forced into the receiver until at the end of the return stroke the boiler pressure is just restored at F.

The size of the receiver for this special case is $V'=\frac{V_1^2}{V_2-V_1}$; also the isodynamic expansion now vanishes so that this maximum is general.

Conditions for Equal Work of Cylinders.—That the cylinders do equal work, the numerator and denominator of (74) must equal each other which condition gives

$$\frac{P_3V_2}{P_1V_1}=\left(Q+S\frac{V_2}{V_1}\right)\left(\frac{V_1}{V_2}\right)^n\left(1+\frac{V_2}{V_1+V'}\right)^{n-a}-1 \quad . \quad . \quad . \quad . \quad (78)$$

the solution of which may be proceeded with as suggested for (54).

If $V' = \infty$, this expression reduces to the same as (55), as it should; also to (56), for $n=1$. This last is seen to be the same condition as for the maximum efficiency, for $V' = \infty$ and $n=1$, as stated for the first engine, in which respect the engines agree.

The Infinitude of Possible Pump Movements.—In all the preceding cases the piston motion has been regarded as such that when one piston moves for making its complete stroke the other piston stands still at the end of its cylinder, and *vice versa*, as first done in the celebrated Worthington pumping engine, and recently in many others. But compound pumping engines are in use in which we find the three parts V_1, V_2 and V' as above, but with different piston and valve motions, as, for instance, in case of certain Cope & Maxwell pumping engines.

Respecting the possible variety of engines due to unlimited suppositions for piston motion, or valve motion, or both, it appears to be infinite. For almost any one of these the diagrams corresponding with Figs. 14 or 16 become exceedingly difficult to delineate, except for the simple case $V' = \infty$. For instance, there might be the tarrying of the pistons, each for half or other fraction of its time; but with the relative period of strokes indifferent. The steam cylinders might be of equal volume, while one makes twice as many strokes as the other, and thus obtain expansion. This is admissible with or without tarrying of pistons. While the high piston tarries, its valve might open into the receiver at any point of time in the period of tarrying. And the low cylinder might close its valve from the receiver at any point in the tarrying of its piston. A comparatively simple case is that where the pistons do not tarry, and where the valves all work promptly on the termination of the strokes of their respective pistons. But here the periods of strokes of pistons might be in any given relation.

VII.—Strokes Co-initial and Co-terminal; without Tarrying.

For this case a little consideration will show that it matters not at which end of stroke one piston is, while the other starts at a particular end; because, for either one piston, the same changes occur at one end as at the other end of stroke. And let there be no tarrying, but both pistons moving continuously. For this, a little consideration will show that this engine may be rotative, with cranks at 180°, and one cylinder to each, and that it is the same as Case I, and Fig. 4.

VIII.—Strokes Interfluent, without Tarrying.

As an example of a case not quite so simple, and to show the effect of interruption due to the exhaust into the receiver from the high cylinder while the low cylinder is making its stroke, let the reversal of stroke for either piston be at the midstroke of the other. Then Fig. 16 may serve to indicate the relation between piston positions for continuity. The part H refers to the high cylinder, and L to the low. Now suppose H on the point of exhausting into the receiver, and let a represent the beginning and a' the end of this operation. This takes place at the midstroke, a, of the L-piston, as shown in the L part of the diagram. As H exhausts, the volume of receiver and connections will increase in the operation by an amount aa' on a proper scale. Now the H-piston travels from a' to b, while the L-piston travels from a to b. Then the L-cylinder exhausts into the air or condenser, and its volume is cut off from the receiver to begin a new cylinder full. This volume cut off from the receiver is bb', as shown on the L part of the figure. This takes place at the midstroke b of the H-piston; see Fig. 16. And thus these operations continue, as can readily be traced from Fig. 16.

Now when a piston makes a half-stroke, the change of volume of the receiver and connections will not change by that amount alone, because two pistons are in action simultaneously, and the change of volume just referred to will be due to the combined movements of pistons, one of which (the H) is compressing, and the other (the L) is expanding this volume. We observe that the high piston *always* compresses, and the low expands this volume.

The letters a, b, c, d, etc, on Fig. 16, for any one letter denotes a single point of time, so that by referring to any single letter we can at once see the relative po-

sitions of both pistons. The stretches between a and a', c and c', etc., or b and b', d and d', etc., indicate shifting of action from end to end. Thus, when the exhaust is completed from one side of the H-piston, it immediately begins from the other side.

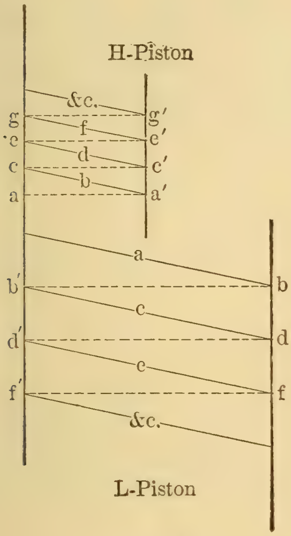


FIG. 16.

For the simultaneous positions, a, we see that the L-piston is at midstroke, while the H-piston is exhausting. Just before beginning the exhaust, a, the total volume in the receiver and connections is $V' + \frac{V_2}{2}$, and just after it is $V' + \frac{V_2}{2} + V_1$, as can easily be traced. For the point b, the H-piston is at midstroke, and the L-cylinder is on the point of exhausting and changing the volume from

$$V' + V_2 + \frac{V_1}{2} \text{ to } V' + \frac{V_1}{2}, \text{ etc., etc.}$$

These changes of volume of receiver and connections are better shown in Fig. 17.

The Complete Diagram.—The shaded areas of Fig. 17 are the indicator cards for the cylinders. The volumes indicated show to which either card belongs. All besides the shaded cards are construction lines used in obtaining the expansion lines and cards.

The lower part of the figure shows the variations in volume. $AB = V'$ is the volume of the receiver itself. At a we have the L-piston at midstroke, while the volume in the receiver and connections is $V' + \frac{V_2}{2}$ as shown. Also the H-cylinder is ready to exhaust, and add the volume $aa' = V_1$. The line at $a'b$ is the variation of volume in the receiver and connections for the half-strokes shown for $a'b$ and ab in Fig. 16. Both pistons move for this, one to reduce, and the other to enlarge the volume considered. The result is an enlargement to the point b, or to the volume $V' + V_2 + \frac{V_1}{2}$. Then the L-cylinder valves are reversed, and the volume $bb' = V_2$ is cut off, leaving only $V' + \frac{V_1}{2}$. The volume $b'c$ is due to combined two half-strokes similarly as in $a'b$. Then cc' is like aa', etc.

Now the expansion line for the change of volume $a'b$, or $c'd$, etc., is $c'd$ above, in the upper part of Fig. 17. This is drawn from A as the zero of volumes. Also the expansion line for $b'c$, or $d'e$, etc., is $d'e$ above. For this the zero is also at A. When the H-cylinder exhausts into the receiver and connections, the volume is raised from

$$V' + \frac{V_2}{2} \text{ to } V' + \frac{V_2}{2} + V_1, \text{ or from C to D;}$$

the steam following from a high-pressure point, C, giving the expansion line Cc', the latter meeting the compression line $cd'c'$ as shown. The resulting pressure is that for the point c'. The expansion from C is for a zero of volumes at D, as will be understood after studying Figs. 14 and 15.

The diagrams are drawn at one side and the other, to avoid confusing the figure.

Now as the H-cylinder exhausts, the pressure becomes that at c', when the H-piston begins its back stroke with a like back pressure. Owing to an increase of volume during this back stroke, the line that would be a compression line for this cylinder alone, becomes a falling or expansion line as shown; first, as far as to d while the L-piston makes a half-stroke; and then as continued to c for the other half, but with a diminishing volume in the receiver and connections. Hence,

the lower line $c'de$ of the H-cylinder card is a broken line as shown.

The L-cylinder expansion line is in two parts also, that at $d'e$ being due to the fall of pressure for the first half of stroke as traceable by lettering. At the mid-stroke there is a sudden rise of pressure cc', due to the exhaust of the H-cylinder into the receiver. From this point expansion continues on a new line due to a different pressure and volume.

Without entering into analysis, it is probably safe to predict that for an infinite volume the pressure in the receiver cylinders, or unequal strokes with equal cylinders, or both combined.

IX.—The Tandem Duplex Compound Pumping Engine.

This engine differs essentially from the preceding by having four cylinders and no receiver.

Such an engine is shown in Fig. 1, in which there are four steam cylinders, two of one size and two of another size; one large and one small cylinder are seen arranged in line of each one of the two piston-rods. The two on one piston-rod

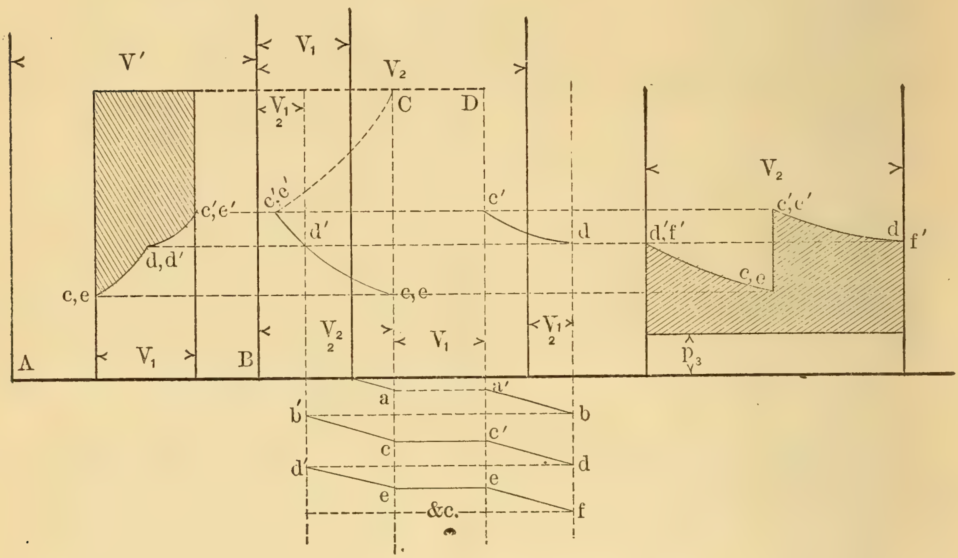


Fig. 17.

will be the same as by (14) and other equations; that is to say, the efficiency is still the same as previously given for an infinite receiver, and nearly so for receivers relatively large. Hence, when the receiver is very large as compared with the cylinders, and when there is no cut-off to either cylinder, the efficiency of engine remains very nearly the same, irrespective of the piston motion or valve motion. Whether there be an advantage in any form of valve or piston motion, for a finite receiver over the infinite, will perhaps be best shown by the numerical results subsequently given.

For the large receiver it is evident that expansion may be obtained equally well, either with equal strokes and unequal form one complete engine of the Woolf form.

Thermodynamically, this engine might be treated without the consideration of duplex, because in reality there are two distinct engines in this form of duplex. We will treat one part as covering the whole in principle.

Here one cylinder may be placed in line with the other, on a common piston-rod, and when so related the arrangement is called "tandem." But the arrangement is immaterial, provided the strokes are co-terminous and without tarrying. The steam is to be delivered directly from the high-pressure cylinder into the low, without the intervention of the receiver. Also, here the steam is

supposed to be admitted to the H-cylinder for the full stroke, and full pressure of boiler; it is then to be exhausted, or transferred to the other cylinder during the whole stroke, which stroke is in common for the two pistons. In this way the volume of steam which occupies the H-cylinder at the end of one stroke, is expanded to the volume of the L-cylinder by the end of the next stroke.

Equation (15) is to be employed for this engine if there be no receiver; or, in the existence of a receiver, even including the intermediate communicating pipe as such, equation (14) will apply.

In these discussions the engine itself is the only part of the machine brought into account, but in the practice of steam pump engineering there are essential considerations relative to the pump as well, some of which have already been referred to.

Numerical Results For Comparison.

A few results have been computed and collected in the following table:

For the first column, (37) was used, and for the second (68); that is, the complete formulas were put to the test.

The feed-water was assumed at the temperature 149° F., and the back pressure 3.62 pounds per square inch, absolute.

The exponent a was taken at $\frac{4}{3} = 1.3333$, n at 1.0456, and for the tandem engine, m was taken at 1.135. The value of a is probably too high for ordinary practice where steam is likely to be supersaturated; probably the best value for practice lies between 1.1 and 1.2.

Table of Results for the V, VI and IX Cases.

P_1	$\frac{P_1}{P_3}$	$\frac{V_2}{V_1}$	*Effy.* V. $V'=2V_2$	*Effy.* VI. $V'=2V_2$	*Effy.* $V'=\infty$, and max. of V.	*Effy.* $V'=\infty$, $n=1$, for VI.	*Effy.* Tandem. and max. of IX.	$\frac{\text{Work in } V_1}{\text{Work in } V_2}$ V.	VI.	$V'=\infty$.	$V'=\infty$ $n=1$	$\frac{\text{Press. of admission, } P_1}{\text{Press. of exhaust, } P_2}$ V.	VI.	$V'=\infty$.	Tandem IX.
1	2	3	4	5	6	7	8	9	10	11	12	13.	14	15	16
15.60	4.31	2	.0597	.0790	.0718	.0700	.0842	1.77	.78	1.01	1.	1.96	2.27	2.08	2.17
37.68	10.41	3	.0824	.1055	.0968	.0962	.1238	1.51	.86	1.03	1.	3.03	3.45	3 13	3.45
71.49	19.75	4	.0974	.1230	.1126	.1114	.1547	1.47	.83	1.04	1.	4.17	4.76	4.35	4.76
119.2	32.91	5	.1060	.1326	.1217	.1213	.1755	1.47	.90	1.05	1.	5.26	5.88	5.26	6.25

In looking over the table we at once notice the glaring fact that the tandem engine excels all the other pumping engines in efficiency, and by a considerable percentage. At 23 pounds per square inch apparent pressure, the excess of efficiency of the tandem over the VI is about 17 per cent. of the efficiency of the latter. For 104 pounds per square inch apparent pressure, the same is about 32 per cent.

A second important fact respecting the two valve movements, viz., that of the V and VI is that the latter is a high percentage above the other.

A third important fact appearing in the table, is that in the VI movement, the efficiency is a very considerable percentage greater when the receiver is small than when it is infinite, a fact in support of the conclusions of solutions of VI and VII. Compare columns 5 and 6. For the 5, the receiver is only twice as large as the low-pressure cylinder, and yet the efficiency here is from 8 to 10 per cent. higher than for the engine with an infinite volume.

For V, however, the case is the reverse, but in a greater ratio; that is to say, the efficiency is lowered from 13 to 17 per cent., by changing the receiver from an infinite volume to one only twice as large as the low-pressure cylinder.

As to the value of n, it appears that the efficiency does not change materially when n changes from 1 to 1.0456.

The Cincinnati Telephone Convention, from present indications, promises to be more interesting than any heretofore held.

THE BOWER-BARFF PROCESS.

By A. S. BOWER, C. E., St. Neots, England.

Transactions of the American Institute of Mining Engineers.

Any process which has for its object the preservation of iron and steel from rust, and which will make these metals more applicable than they now are to the requirements of mankind, will be sure to meet with attention from members of this association, and from all those who are either engaged in the extraction of the ore, its reduction to metal, or the subsequent application of the metal itself.

It is, perhaps, not too much to say that when iron and steel are rendered secure against corrosion and decay, they will be used to an indefinitely greater extent than they now are. The whole realm of science has, therefore, been explored in the attempt to discover some method by which the formed article may be preserved, leaving its strength undiminished by the destructive action of rust. Paints, oils, varnishes, glazes, enamels, galvanizing, electro-depositing and what is called "inoxidizing" are among the many systems now in vogue to effect the preservation of iron and steel from the corrosive action of air and water.

The object of this paper is to show what may be done in protecting iron and steel from rust by forming upon their surfaces a film of magnetic oxide by an inexpensive process. It is no new thing to be told that magnetic oxide of iron is unaffected by exposure to the atmosphere or to salt water for any length of time. The black sand of Taranaki, in New Zealand, is a sufficiently good example of this. Dr. Percy has pointed out that the reason why Russian sheet iron is less affected by exposure than ordinary sheet iron is because of a coating of magnetic oxide; but this was not known until Dr. Percy discovered it. That such a coating is produced is quite certain, but it is only an accident of manufacture. To Professor Barff is due the credit of being the first to deliberately undertake to coat iron and steel with magnetic oxide, produced designedly for the purpose of protecting their surfaces from rust.

Some 16 or 17 years ago my father made a series of experiments in the production of heating gases, one set of them being the decomposition of water by passing superheated steam through masses of red-hot iron. He noticed that the iron became less and less active until it ceased to decompose at all, when, on examining it, he saw that it was coated with a kind of enamel. It at once occurred to him, on seeing this, that the process in question might be used to obtain such a coating, but he found after a few days' exposure of the iron to the atmosphere, that the coating shelled off, and he pursued the matter no further. The iron employed in this case was rusty, but if it had been new my father would in all human probability have been the accidental author of the process which Professor Barff discovered ten years afterward. I only mention this to show how advisable it is to investigate the causes of unexpected effects. Professor Barff's process consists in subjecting iron or steel articles to the action of superheated-steam, and when they are at a temperature sufficiently high, three equivalents of iron combine with four of oxygen, forming one equivalent of magnetic oxide, and setting eight of hydrogen free, or symbolically (1) $Fe_3 + 4H_2O = Fe_3O_4 + 8H$.

Upon reading a description of the Barff process in the London *Times*, it occurred to my father that what the Professor could effect with steam he might also effect with air, and several experiments were made to this end, which were very varied in character, as were also the results obtained. The first was made with cast iron, by placing the articles to be treated in a cast-iron retort, heated externally, and then passing superheated air over them; and it was successful, while nearly all the others afterwards were quite the reverse, as sesquioxide was copiously produced as well as the magnetic. Another experiment was made by placing a bar of polished cast iron in the main duct of superheated air to a blast furnace, and this, though covered with a red sesquioxide powder easily brushed off, had a thin, but very firm and

tenacious coating of magnetic oxide in contact with the iron. This bar has been exposed to the weather ever since, or over four years, without the slightest appearance of rust. Ultimately, when thinking over the fact that air is oxygen and nitrogen in mechanical combination only, I came to the conclusion that, to form the lower or magnetic oxide, the quantity of free oxygen, and so of the air employed, must bear some proportion to the surface of the articles exposed to its action, more especially when a comparatively low heat is employed. This is so, and it has been proved that the quantity of air passed through the retort during most of the unsuccessful experiments was 300 or 400 times more than was actually necessary. The reasons also why the first experiment was successful were that a great number of articles were in the muffle, that a very high heat was employed, and the retort had been previously used for coal-gas making, and had a deposit of carbon in it, which to a great extent neutralized the effect of the large excess of air.

All the unsuccessfully treated articles were red with sesquioxide outside; but there was, nevertheless, a coating of magnetic oxide in close proximity with the iron, due to the reducing influence of the metal in contact with the sesquioxide at an elevated temperature. The general appearance however, of iron so treated was disagreeable, to say the least of it. The mode of action I then adopted was to admit a few cubic feet of air into the retort at the commencement of every half-hour, and then to leave the iron and air to their own devices, the retort, of course, being tightly closed. During each half-hour a coating of magnetic oxide was formed, and the operation was repeated as often as was considered necessary. Effective as this was for cast iron, the cost of producing the coating was as great as by the Barff process, for both of them required that the chamber should be heated externally, and this with large furnaces is very expensive. Another plan that I adopted was to first find out approximately the extent of the surface of the goods to be treated, by first dipping them all into a tank of water of known area, lifting them out, and noticing the amount of water taken out of the tank by the wetted surface, and regulating accordingly a slow, continuous air supply by meter, of course keeping the temperature of the muffle as nearly constant as possible. This, too, was successful; but the same objections applied to that mode of procedure as to the other.

There was commenced a series of experiments with carbonic acid chemically produced by the decomposition of chalk, the idea being that three equivalents of iron would unite with four of carbonic acid, forming one equivalent of magnetic oxide, and four of carbonic oxide, if the heat were sufficiently high. This reaction is expressed symbolically thus: (2) $3Fe + 4CO_2 = Fe_3O_4 + 4CO$. This is the simplest action that could take place, but it was evident from the results that something quite different was obtained, inasmuch as the coating was very light in color, pleasing to the eye, but easily removed, and in that sense entirely differing from the articles you see before you. This coating, from effects exactly similar and designedly produced by a studied manipulation in the furnaces in successful operation in England, France and here, proves pretty conclusively that carbonic acid, practically pure, produces upon iron, at an elevated temperature, a film which is, in composition, a mixture of FeO and Fe_3O_4, or, at all events, it is nearer the metallic state than is magnetic oxide. But even supposing that the results obtained by the carbonic acid had been successful as then carried out, the objections referred to concerning the air process would still exist, as external heat and a closed iron muffle would always be necessary. I therefore proposed to use a fuel-gas producer, similar in principle to the Siemens generator, but altered practically to suit other requirements, to burn the combustible gases thus produced with a slight excess of air over and above that actually required for perfect combustion, and to heat and oxidize the iron articles, placed in a suitable brick chamber, by these products of combustion. I also arranged a continuous regenerator of fire-clay tubes underneath the furnace, so that the products of combustion leaving the oxidizing chamber passed outside the tube, imparting a portion of the waste heat to them, which was taken up by the ingoing cold air passing through their interior on its way to the

combustion chamber. I had hoped in this way to be able to so regulate the excess of air over that required for complete combustion as to be able to produce magnetic oxide directly, instead of the lower and useless oxide, or combination of oxides, produced by carbonic acid alone. I obtained some beautiful results, and some again were unaccountably bad, and I soon found that it was as difficult to regulate the precise amount of oxidation as it first was in the Bessemer process, and I was fortunate enough to hit upon an almost parallel remedy—that is to say, I increased the quantity of free oxygen mixed with the products of combustion, and oxidized the iron articles to excess during a fixed period of generally 40 minutes, when magnetic oxide was formed close to the iron and sesquioxide over all. Then for twenty minutes I closed the air inlet entirely, leaving the gas-valve open, and so reduced the outside coating of sesquioxide to magnetic oxide by the reducing action of the combustible gases alone.

The excess of oxygen in the first instance produces Fe_2O_3, or sesquioxide of iron, and the under surface of this being in contact with metallic iron, undergoes reduction to magnetic oxide in the following manner: Four equivalents of sesquioxide unite with one of metallic iron, forming three equivalents of magnetic oxide, or symbolically (3) $4Fe_2O_3+Fe=3Fe_2O_4$.

When deoxidizing by combustible gases, consisting mainly of carbonic oxide, three equivalents of sesquioxide unite with one of carbonic oxide and form two equivalents of magnetic oxide and one of carbonic acid, or symbolically, (4) $3Fe_2O_3+CO=2Fe_3O_4+CO_2$. Another method of reduction is by carbon itself, when the formula stands thus: (5) $3Fe_2O_3+C=2Fe_3O_4+CO$.

Formula (4) is also the reaction when rusty iron is reduced by producer gases which consist largely of carbonic oxide; and by the specimens exhibited it will be seen that articles completely pitted with rust may have their surfaces rendered rustless. In this case of oxidizing and deoxidizing are reversed—that is to say, the latter occupies 40 and the former 20 minutes. No oxidizing is theoretically necessary, but practically a certain amount is requisite to keep up the heat in the chamber, which, of course, could not be done unless combustion took place some time or other. I only mention the reduction by carbon as exemplified by formula (5) because, while experimenting with a furnace, I was asked by the proprietors of a valuable red-oxide deposit, which was found in so finely divided a state as to be capable of being used at once as a paint, whether I could reduce it to a magnetic oxide. I tried to do so by carbonic oxide, but I found that only the surface of it was affected, and that even this, when taken out of the furnace, speedily returned to its original red color, by the combined actions of the hot unconverted material underneath and the air above. It will be found from formula (5) that $2\frac{1}{2}$ pounds of carbon are required to reduce 100 pounds of red oxide. This I mixed intimately, in the shape of powder, with the red oxide, brought the mixture to a red heat and the result was black magnetic oxide. Not only this, but by adding more carbon I could make the color lighter and lighter until it was almost identical with the coating produced in my previous experiments with carbonic acid, and by reducing the quantity of carbon below $2\frac{1}{2}$ per cent. various shades of purple were obtained, the red appearing more and more prominent as the quantity of carbon was diminished.

It will be as well, before I make any comparison between Professor Barff's process and the processes patented by my father and myself, to state that the whole of the Professor's patents, wherever existing, have been purchased by my father, so that in this case, at least, I hope you will not say that "comparisons are odious." Professor Barff's process is better than ours for wrought iron, and perhaps for polished work of all kinds, as iron commences to decompose steam at a very low temperature; in fact, much below visible redness. Only the other day at the annual meeting of the Association of American Stove Manufacturers, held in New York, I was asked whether stove patterns might not be made of cast iron, polished and then oxidized? Here is one among many instances where the steam process is almost invaluable. For ordinary cast iron, and especially that quality which contains much carbon, the Barff process is much too slow in its

action, and some specimens that I have treated in England have taken as much as 36 hours to coat effectually, which could readily have been finished off in five hours by the Bower process.

The main distinction between the two is that the Bower is much more energetic in its action than the Barff process. The carbon in cast iron impedes oxidation, and so, while cast iron is far more readily treated in the Bower furnace, wrought iron is apt to scale unless it is rusted beforehand. The rust then eats into the metallic surface under the influence of heat, and forms a tenacious combination with it. The objection to the use of a closed muffle externally heated, as in the Barff process, has been almost entirely overcome by simply putting wrought iron into a Bower furnace, previously well heated, then shutting off both the gas and air supplies, and admitting steam into the regenerator tubes. The steam thus passes through the red-hot tubes, then through the combination chamber and its contingent passages already highly heated, over the articles in the oxidizing chamber, heating and oxidizing them, and thence over the outside of the regenerator tubes, depositing a great portion of its heat there before passing to the chimney, and which is again picked up by the ingoing fresh, cooler stream. In this way the heat in the chamber is highest shortly after the commencement of the operation, and gets gradually lower during the time of exposure, which varies according to the class of goods, from five to ten hours. At the close of the operation, just before the articles are taken out, everything is moderately cool, and this for steam is the perfection of action, as stated by Professor Barff himself. Steel, I consider, can be equally well treated by both processes, and, indeed, it is natural to expect this, steel being, so far as the quantity of carbon it contains is concerned, between cast and wrought iron. Polished steel, however, is better treated in a low-temperature Barff furnace.

With regard to the quality of fuel burned in the gas producers, a non-coking gas coal is the best, and Virginia splint has suited very well in this country, and of this about 1 ton every three days is required for a furnace with an oxidizing chamber 13 feet long, 4 feet 3 inches wide and 4 feet 3 inches high. When a gas coal is employed, it should be fed through the charging hoppers just before each deoxidizing operation, when a smoky flame is of great advantage. I have, however, discovered that anthracite can be used as well as a gas coal, by simply allowing petroleum to drop at the rate of 1 gallon per hour upon the red-hot surface of the coal in one of the gas producers. This method has been exclusively used in the coating of the articles exhibited in this room, at the works of Messrs. Poulson & Eger, architectural engineers, at North Eleventh and Third Streets, Brooklyn, E. D., N. Y., to whom I am much indebted, not only for these beautiful castings, but for the constant courtesy and energy they have always exhibited during the erection of their furnaces. At present they have two erected, one a Bower furnace of the size before mentioned, and the other a small Barff furnace for the treatment of very delicate or polished articles.

These magnetic-oxide processes not only protect from rust, but the coating is of such a beautiful color as to render articles ready for the market as soon as they are out of the furnace and cooled. One remarkable feature of them is that there is no more cost (except in the labor of handling them) in treating 2240 articles, each weighing a pound, than there is in coating a cube of metal weighing a ton; and so penetrating is the process that no matter how intricate the pattern may be, every crevice—which it would be almost impossible to get at with a paint-brush—is as effectively coated as the plainest surfaces as will be observed by examining the specimens exhibited. For art purposes the French gray color, with shades approaching to black, might not always be suitable; but if it should be necessary to use paint on the iron so coated, there is an absolute certainty that it will remain on in the same way as it does on wood or stone, and thus iron may be used for constructive work in a thousand directions in which it has not up to the present time been possible on account of its liability to rust, no matter what the coating used to protect it has been.

I can give an instructive instance of this. A company in Paris had expended a very large sum over Dode's inoxidizing process, which process consists in the

depositing of a layer of borate of lead on iron or steel and then gilding, platenizing or bronzing them; and certainly the articles so treated were exceedingly beautiful to look at. But the iron ultimately rebelled and threw off the coating, so that the shareholders were in a fair way of losing all their capital, when it was suggested to the directors that if their compositions could be deposited direct upon magnetic oxide they would conquer the difficulty. They then applied to my father for specimens of coated iron to experiment upon, and they were so well satisfied with the result that the company purchased all our European patents except those for England, and are carrying on the combined processes on a large scale. They have, besides their furnaces for the Dode process, four large Bower furnaces, two being 36 feet long by about 6 feet 6 inches wide and 6 feet high, and a Bower-Barff furnace, also of large size. Others, moreover, are in course of erection.

Engineers and manufacturers appear far more ready to apply the processes here and on the Continent of Europe than up to the present time they have been in England. Perhaps the reason has been that, so far as Professor Barff's process is concerned, it has only just been shown how large masses can be dealt with—namely, by the use of the Bower furnace. I can show that, for the treatment of underground pipes, wrought-iron sleepers, roofing, and the like, the process can be readily applied, and at a cost much less than that of galvanizing, and they will at the same be infinitely more durable; while for ornamental cast and wrought iron it is scarcely possible to imagine anything more artistic in color than some of the articles after they have been treated. For ordinary hollow-ware for kitchen use, whether of cast or wrought iron, this process is admirably adapted, and though I have been told that the gray or black color will probably be objectionable, yet I imagine, if it can be shown, as can be done, that the magnetic oxide is more durable, more easily cleaned and much cheaper than even the common tinted article, a market will soon be created. Anyhow, the new combined processes are so far developed, and they have been so thoroughly examined by scientific and practical men both here and in Europe (whose testimony to the value and efficacy of them is voluminous), that they have passed from the region of theoretical investigation into that of practical application, and means have been taken for establishing works at different centers in Europe, as will also be done here, for the purpose of coating iron and steel as a trade operation. One firm alone in Scotland, Messrs. Walter Macfarlane & Co., have adopted the process, and their output of ornamental castings per day exceeds 100 tons. It is intended to apply the process to cast-iron gas and water pipes, and as the former have comparatively no pressure to bear, they may be made much lighter than they now are, if rendered incorrodible; while for water, it will be a great advantage to have both the main and service pipes rendered safe from rust, which not only discolors the water, but forms the nucleus of very troublesome deposits. There is no reason now why wrought-iron or mild-steel pipes should not be used for the same purposes, especially for the interior towns of distant countries, where the first cost of the pipes is but small as compared with the cost of carriage.

My father has himself used gas and water pipes where the cost on arrival at their destination has been five times greater than their first cost in England. If, then, light wrought-iron, or steel pipes could be used, not weighing one-third of those made of cast-iron, and rendered practically indestructible, what an enormous saving will be effected! Again, in the case of railway sleepers in iron and steel, which are now almost wholly used in Germany, the process is likely to prove of much advantage, so at least I am told by engineers, both in Belgium and in Germany; and if there, why not here? For fountains, railings and all architectural work the process is invaluable, and iron may now be used in many instances instead of bronze.

It will naturally be asked, what is the cost of the process? I cannot do better than answer the question by quoting from the report of Professor Flamache, the engineer-in-chief of the State railways in Belgium, who was sent over specially to England to report on the process by the Public Works Department of that country. His estimate of cost,

after a very careful examination and testing of the process, was 7½ francs per 1000 kg., or nearly $2 per ton, at, of course, the Belgian rate of expenses. He also gives the cost of coating a certain extent of surface, but this I consider to be completely valueless, as, for example, I have had a furnace full of 56-pound weights. and another time I have had it full of gas-governor tops, the surface in the latter case being perhaps one hundred times more in extent than in the former, while the actual cost of oxidizing would be the same in both cases. He also says that this cost may be reduced, as instead of one workman attending to one furnace he can attend to three or four; also by a better system of taking the articles out than existed in the experimental furnace that he saw.

Sir Joseph Whitworth, feeling much interest in Professor Barff's process, sent to him some steel to be oxidized, so that he might ascertain whether it did or did not lose in strength by the operation, and the result of Sir Joseph's testing was that there had been no alteration whatever. Theoretically, one would rather expect that iron and steel would be somewhat toughened, as the tendency of the process is to anneal, and would, no doubt, if continued long enough, render some classes of cast iron malleable. A very-thin article, if excessively coated, might probably be weakened, due to the fact that the coat of magnetic oxide would form an appreciable percentage of the bulk of the article; but this, of course, is a very extreme case, and one which is not likely ever to occur in practice.

The development of these processes has been a long and tedious business, and one requiring much faith and patience in the midst of most disheartening failures for months together; but to gentlemen connected with the iron and steel industries, and who know well that results are only obtained by patient and well-directed toil, I need not dwell on this, as almost every man who has had to reduce theory to practice has had abundant experience of the same kind.

A REVIEW OF PROF. S. W. ROBINSON'S LONG COLUMN FORMULA.

By WILLIAM H. BURR.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

Having lately had occasion to examine the column formula given by Prof. S. W. Robinson in this Magazine for June, 1882, I find myself compelled to take exception to them, both in regard to the analysis on which they are based and on the ground of their disagreement with actual tests.

In the first place, he employs the "common theory of flexure" as the instrument to establish his formula, recognizing, as is usual, the fact that the condition of stress at any section of the column is a resultant of pure compression combined with pure bending. At just what point the common theory of flexure becomes approximately applicable to pieces under combined compression and flexure is not yet precisely established by experiment; but a sufficient number of experiments exist to show that it is not below a length equal to two hundred radii of gyration of section. To demonstrate that a formula based simply upon the common theory of flexure (as this is) gives results agreeing closely with actual tests is proving altogether too much, unless the columns are excessively long. From the very nature of the case it is not possible to produce a formula so based which shall represent with tolerably close approximation the ultimate compressive resistance of such columns as are ordinarily used in bridge construction. All that ever has been done of a reliable character, is to produce a *form* of formula in which empirical coefficients may be introduced. Even such formulas are applicable only within restricted limits; yet they have had great practical value.

Again, any formula for rupture deduced by the aid of the common theory of flexure, carries a supposition of a perfectly

elastic condition, even to the point of failure. No such condition exists in ordinary long columns, including those by which Prof. Robinson tests his formulas. It may be urged, however, that his formulas *do* give tolerably close agreement with the experiments cited by him. But in reality those results are neither sufficiently continuous nor extended in range of length over radius of gyration to furnish anything approaching a critical test of his formula. And again, a close consideration of Prof. Robinson's analysis reveals some singular characteristics, that in one or two instances, at least, take the shape of positive error, and which lead to an explanation of discrepancies which will subsequently be shown to exist.

On pages 490 and 491 in the June, 1882, No. of this Magazine, he takes moments first about the center of gravity of the column section and then about some line parallel to the first axis and without the column, while he proceeds to combine them or treat them separately as if they involved independent conditions and as if one would give some result which the other would not. In reality either of the equations so formed is simply an equality between the bending moments of external forces and internal stresses, and either one can be derived from the other by adding or subtracting the same thing to or from each member of the latter. Such equations involve precisely the same general conditions, and give nothing except two forms of the common difference between the members, and in this case that is not needed.

At the top of the second column on page 491, Prof. Robinson shows by taking moments about an axis passing through the center of gravity of the column section, that "when a compressive force, T, is applied at the distance, k, (radius of gyration) from the center of the cross section of a prism, the displacement, EF, due to bending, is exactly equal to the displacement, FG, or BC, due to direct compression"; references belong to his Fig. 22. But his Fig. 22 shows that if FG=EF=BC, the distance BD of the neutral axis from the center of gravity of the section must be equal to BE, the distance of the line of action of the compressing force T from B, the center of gravity; *i.e.*, when $y_1=k$, BD $=y_1$. It is important to observe that BD is proved equal to y_1 *only when* T *is applied at* E.

After showing these results, Prof. Robinson then takes moments about a neutral axis which he supposes at a distance qk from B "while T is applied at E." But it has just been seen that his first moment equation proves that "while T is applied at E," qk must equal k, or $q=1$. However, let us follow his analysis.

The moment equation preceding his Eq. (92a) gives:

$$Tqk=\frac{\varepsilon K}{\rho}\,q^2k^2.$$

Or, since $T=\varepsilon K\lambda$, and since he has just proved that when T is applied at E, $k=\rho\lambda$, the above equation gives $q=1$, just what his first moment equation proves. The two moment equations, therefore, give the same result, as was to be expected. But immediately under the above equation, or value of Tqk, Prof. Robinson writes:

$$Tk=\frac{\varepsilon K}{\rho}qk^2 <= \text{or} > \frac{T}{\rho\lambda}k.qk.$$

He gives no warrant for these inequality signs. The equation, mathematically exact, stands:

$$Tk=\frac{\varepsilon K}{\rho}\,qk^2;$$

there is no possible variation in the value of the second member. None of the conditions of the problem permit such latitude; the equation is simply one between the external bending moment and that of the internal stresses. After introducing these inequality signs, without warrant or right, as has just been seen, Prof. Robinson proceeds to show that although he assumes q may vary, yet it cannot be greater or less than unity, and hence, must equal that quantity.

When he has thus shown for Fig. 22, that under the condition of T being applied at E, BD=BE$=k=y_1$, he at once leaps to the astonishing conclusion "we therefore follow the conditions that in Eqs. (90) to (92), BD$=y_1$." Having shown that for one special point of application of T, that BD$=y_1=k$, he generalizes to the effect that in a bent column the normal distance from the line o action of the applied force to the center

of gravity of the column section, at its point of greatest deflection, is equal to the normal distance from the latter point to the neutral axis. Nothing could be more erroneous; at least, he should prove it and not assume it. The logical process would require a *general* result to be established from which, by the introduction of particular given conditions, a special one would proceed.

By means of this erroneous operation Prof. Robinson finds a value of the greatest intensity of compression (t) which involves the radius of curvature of the column at the place of greatest bending; the same value also involves the greatest deflection y_1. He then writes the greatest bending moment in terms of the total compressive resistance (T) of the column.

At this point of his analysis we are surprised to observe that he is obliged to use the value of the compressive resistance of the column as given by Euler's formula in order to eliminate that force (T) from one of the equations just mentioned. He states half a page further on that he does not use Euler's formula as a "foundation" of his own. Whatever may be his intention, however, as a matter of fact he takes the compressive resistance of a long column as given by the oldest column formula and introduces it into his own equations. Now if T is given by Euler's formula, what is needed of his Eqs. (97), (98), (99) and (100)?

The signification of these operations will be noticed further on.

His long column formula as actually given are his Eqs. (97), (98), (99) and (100). As good a series of experiments as can be chosen, probably, for the purpose of testing the accuracy of a formula is that made on Phœnix columns in Nov. 1879, and given in *Trans. Am. Soc. Civil Engineers*, January, 1882. The range of this series even, is too limited for the purpose, but its continuity is eminently satisfactory. These columns had flat ends, hence:

$$T = \frac{tK}{1 + \frac{d_1^2}{2k^2}\left(\sqrt{1 + \frac{tl^2}{\pi^2 \varepsilon d_1^2}} - 1\right)} \quad . \quad (1)$$

But a difficulty at once confronts us in applying this formula, which indicates an error of some sort in its very form. A Phœnix column is equally liable to fail in any direction, if its ends are simply "flat," while d_1 (distance of most remote fibre from neutral axis) has different values in different directions. Eq. (1) indicates that T may vary with d_1. Now which value shall be taken in the present case, in which d_1 may vary from about 4.1 to 5.8 inches? For these columns $k^2=8.94$, and in consequence of the indetermination regarding d_1, there was taken about a mean value of $\frac{d_1^2}{2k^2}$, *i.e.*, that value was taken at 1.5. Taking $t=40000$ and $\varepsilon=27{,}000{,}000$, Eq. (1) may be put in the form:

$$\frac{T}{K} = p = \frac{40000}{\frac{3}{2}\sqrt{1+0.00005\frac{l^2}{k^2}} - 0.5} \quad . \quad (2)$$

The accompanying table gives some of the data of the columns together with the results of the tests; also the values in the column "p" found by the application of Prof. Robinson's formula, *i.e.*, Eq. (2).

Phœnix Columns.

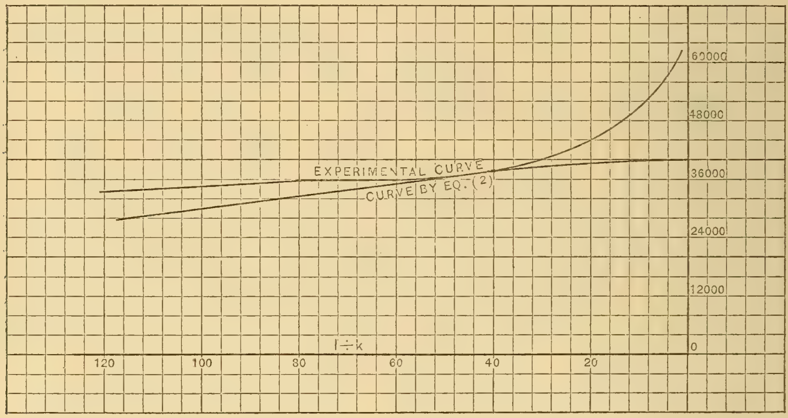
No.	Length.	$l \div k$	Exp.	p.
	Feet.		Pounds.	Pounds.
1	28	112	34650	28300
2	25	100	35150	30000
3	22	88	35000	31570
4	19	76	36130	33250
5	16	64	36580	34780
6	13	52	37000	36430
7	10	40	36440	37770
8	7	28	40700	38830
9	4	16	50400	39640
	Inches.			
10	8	2.7	57200	40000

The column "Exp." contains the means of two experiments on each length of post. Each way from Nos. 5, 6 and 7, there is a constant and rapid divergence of the formula from actual results of tests. In view of the fact that there are a number of formulas of both Euler's and Tredgold's forms which, with empirical coefficients, give far better results and through a wider range than Eq. (2), the above table shows Prof. Robinson's formula to be of little or no practical value even if its analytical basis were correct.

The adjacent figure gives a graphical representation of the results in the table and shows the formula to be yet more unsatisfactory, because its curve is convex, or concave, in the wrong direction. Again, the Eq. (2) and its curve show the greatest possible unit resistance of wrought iron in blocks to be 40,000 pounds per square inch, whereas that material has an ultimate resistance to compression of 55,000 to 60,000 pounds.

Euler's for very large values of $l \div k$. But Prof. Robinson does not show that there is more than one point of agreement, whereas there should be coincidence for all very large values.

Although not relevant to the matter under discussion, it is proper to observe in this place that Prof. Robinson seems to hold, as some others do, that Euler's form of formula contemplates only pure bending, and not combined bending and compression. As a matter of fact it belongs to a piece under combined compression and bending just as completely as Tredgold's form, *i.e.*, the many varities of Gordon's or Rankine's formula. Both forms involve the same conditions precisely, and are subject to the same limitations in respect to the common theory of flexure and elastic conditions of the material, and to no other.



But why, it may be plausibly urged, if the formula is so erroneous in its nature, are not the variations from actual test still greater?

With a little consideration the reason is obvious. Prof. Robinson's analysis really belongs to a block, or column, of material in which the resultant stress in the middle section, at least, is *explicitly* assumed to act at the distance k from the center of gravity, while he *implicitly* assumes the resistance of the same piece of material to be given by Euler's formula, after which he proceeds to establish his result in the ordinary manner when treating of combined compression and flexure.

Now such a *mélange* involves just enough of truth, and approximation thereto, not to give results at random, but which are far enough from expressions of a true law.

Besides the fact indicative of error that the formula involves d_i, as has already been noticed, if it were not incorrect its results ought to agree with those of Euler's for very large values of $l \div k$.

M. DE MOLINARI calculates that the municipal expenditure of Paris equals that of London, although it has not two-thirds of the population. The most expensive capital on the Continent from a ratepayer's point of view is Munich. But after Munich comes Paris, with an annual expenditure of £4 per head of its population. The expenditure of London, including loans, is about £2 16s. per head. Both London and Paris spend £10,000,000 on their local government. Berlin is much more economical, the rate per head in the Prussian capital being only £1 17s. 6d. per annum.

EXPERIMENTS ON IRON AND MILD STEEL.

From "Iron."

The Aix-la-Chapelle branch of the German Engineers' Union have lately had before them some valuable experiments on the use, as a material of construction, of what they now call Flusseisen, and we call either ingot iron or mild steel. The question was raised by a paper by Prof. Intze, who strongly recommended the material on the ground of a series of tensile tests. At a subsequent meeting a letter was read by Herr Offergeld, of the Duisburg Bridge Works, describing comparative experiments made at those works on riveted girders of mild steel and wrought iron. The steel was made specially for the experiments by Krupp, it being found that the steel bought in market was not sufficiently uniform for such tests; in fact, this want of uniformity, together with numerous failures in the works, had compelled that company to abandon the use of steel for bridge work altogether.

The girders, whether of wrought iron or mild steel, were about 6 meters in length (19.7 feet), 700 mm. in depth (2.3 feet), and of the same strength as those employed for railway bridge making. As is usual with such work, when riveted girders are erected, the plates were completely put together, and the holes made in that position, so that it was not necessary to rimer or drift the holes—a practice which was in this case indeed, very carefully avoided. In the tests the girders were secured against bending to a greater extent than is usual in practice. The web plates were strengthened with angle-iron stiffeners, placed 800 mm. apart (2.62 feet); towards the center they were as near as 400 mm. (1.31 foot). Various experiments in mild steel had given the following results:

Breaking strength (F). Kg. per sq. mm.	Tons per sq. in.	Elongation (D). Per cent.	Contraction of area (C). Per cent.
42	26.5	24	49
42	26.5	15	43
48	30.2	24	49

A load of F=28 kg. per square mm. (17.6 tons per square inch) produced in the girder a deformation of 2 mm., which, on removal of the load, proved to be permanent, and which subsequently increased; and here it may be observed, once for all, that all deformations of a local nature occurred in the compressed flange only. Increased pressure next produced a swelling in the web plate close to the stiffener, which continued to increase. The flange yielded correspondingly, and the horizontal angle-irons connected with it crumpled till at F=35 kg. per square mm. (22 tons per square inch) the resistance of the girder was so greatly diminished that a load of F=36 kg. per sq. mm. could not be attained.

A second set of mild steel girders gave the following tests:

Breaking strength. Kg. per sq. mm.	Tons per sq. in.	Elongation Per cent.	Contraction of area. Per cent.
46	29.0	24.0	47
43	26.1	24.5	46
48	30.2	24.5	50

At F=31 kg. per square mm. (19.5 tons per square inch) the lower flange began to bend out sideways. At F=35 kg. the swellings right and left of the center near the stiffeners began, one appearing in front, and the other on the opposite side; the compression flange became S-shaped in plan. The deflection was continued without difficulty from 30 to 94 mm., after which it became so rapid that the pressure could not be increased. The tension flange was bent by 2 mm. (0.08 inch).

A third set of mild steel girders gave the following tests:

Breaking strength (F). Kg. per sq. mm.	Tons per sq. in.	Elongation Per cent.	Contraction of area. Per cent.
49	39.9	23	42
48	30.2	22	45
48	30.2	21	47

The series of effects were exactly the same as before—F=25 kg. per square mm. proved the limit of elasticity, and beyond that the same deformations took place.

The behavior of three wrought iron girders was as follows:

Breaking strength. Kg. per sq. mm.	Tons per sq. in.	Elongation Per cent.	Contraction of area. Per cent.
39	24.6	24	35
40	25.2	21	28
10	6.3	14	24

The girder tests gave at F=35 kg. per sq. mm. (22 tons per square inch) the first signs of yielding in the compression flange. At F=37 kg. the central part of the compression flange was reduced from its original length of 800 mm. to 795 mm., and the tension flange increased to 809 mm., though no alteration in shape was perceptible. It was only under a pressure of F=38 kg. that the double swellings occurred in the web plates, but instead of attaining a depth of 25 mm. (1 inch), they amounted at the most to 10 mm. (0.4 inch). At F=39 there appeared at a row of rivets signs of crumpling in the compression flange. In the tension flange the rivet-heads at the center of the girder, both in the flanges and angle irons, were displaced, and showed small cracks in places.

A second set of wrought iron girders gave the following tests:

Breaking strength. Kg. per sq. mm.	Tons per sq. in.	Elongation Per cent.	Contraction of area. Per cent.
39	24.6	22.5	26
40	25.2	21.0	29
38	24.0	15.0	23

The girder was loaded to F=38 kg. per square mm. without any break becoming apparent; the amount of deformation was immaterial, and as looked at from above, equal, both upper and lower flange being bent out to 15 mm. No swellings appeared. The compression flange, however, both plates and angle irons, was buckled for 1 mm. in length and 3 mm. in depth. The rivet-heads of the tension flange were displaced by 1 mm.

Another set of wrought iron girders gave the following tests:

Breaking strength. Kg. per sq. mm.	Tons per sq. in.	Elongation Per cent.	Contraction of area. Per cent.
40	25.2	23	30
40	25.2	25	31
19	24.6	12	11

These experiments showed at F=35 kg., in the tension flange a small tear of 1 mm., starting from a rivet. At F=38 kg. this tear widened, and a slight yielding in the compression flange became apparent. At F=39 kg. indications of swellings in the web plates showed themselves. A length of 320 mm. diminished in the compression flange to 317 mm., and increased in the tension flange to 331 mm. At F=40 kg. the tear increased. At a deflection of 84 mm. the tear increased so that the horizontal plates appeared to be torn right over both riveting holes; this took place without any noise.

Increased load produced a frequent low cracking, arising, doubtless from the rivets, which were displaced to the amount of 1 mm. The compression flange became severely buckled at a deflection of the girder amounting to 88 mm. (3.5 inches). One of the angles of the compression flange split through. At 90 mm. the tension flange broke through entirely with a dull sound. The breakage of each bar showed both extension and contraction. The two center vertical strips, originally parallel at a distance of 840 mm., showed just below the compression girder a distance of only 822 mm., and just above the tension girder of 858 mm. These experiments show that mild steel is less effective for girders than wrought iron.

The greater tensile strength by 20 per cent. in mild steel is unavailable, because, long before such a tension is reached, the material developes wrinkles in the compressed portions, and this deformation destroys the resisting power of the girder. The resisting power of the wrought iron girder is efficient against even a greater load, and both the tension and compression flange begin to yield at about the same time to the pressure put upon them.

In mild steel also allowance must be made as yet, at least, for the inequalities existing in the material itself, which necessitate the most careful treatment in the working up, as the smallest tear or scratch sometimes causes the whole to go to pieces. The bad results obtained from steel bridge girders must not, however, be ascribed to the fact that 55 kg. per square mm. (41 tons per square inch) is too hard a steel, as in America good results have been obtained with steel of 80 kg. (50 tons per square inch), with an extension of 10 per cent., in bridge work. Steel or this quality was therefore obtained and experimented on. The girders behaved comparatively well. The first

showed a crack at 55 kg. (34.6 tons per square inch), but without losing its strength, which continued till a second crack took place at 68 kg. (42.8 tons per square inch). The second girder cracked first at 59 kg., and lost its resisting power at the same time. The third was not to be broken, but at 69 kg. showed such serious local deformation (of exactly the same quality and degree as in mild steel) that the resisting power was thereby destroyed. That such exceptionally hard steel can be so deflected shows that previous bad results must not be laid down to the score of hardness. The fault lies principally in the inequalities, which cannot possibly be avoided in the making, and which only become the more marked when the material is made in large quantities. Both the hard and mild steel used were specially manufactured for these experiments. Whether the excellence could have been retained had 100 tons been ordered is very doubtful, and the experiments would certainly have turned out less brilliantly. Careful working of bridge material cannot be counted on, as it is impossible to station a controller behind each workman.

Herr Intze explained that, in spite of the experience and experiments of Herren Petersen and Offergeld, his views (previously stated) remained unchanged, and that they had, indeed, received fresh confirmation from some experiments which, by the courtesy of the management, he had been enabled to make officially for two new bridges recently erected at Königsberg. These experiments confirmed the excellent qualities of many kinds of steel, which were, further, maintained throughout the construction, as well as in the test pieces—a result which it was well known had not been attained in the hard steel experimented on by Harkor for the Dutch bridges.

The Königsberg bridges were constructed mainly of mild steel. About sixty experiments with test pieces were made with plates, angle iron I iron, and rivet iron, yielding an average breaking strength of 49.5 kg. per square mm. (31.2 tons per square inch), a limit of elasticity 32.4 kg. per square mm. (20.4 tons per square inch), an extension of 26.9 per cent. and a contraction of 46.5 per cent.

Some I irons, which, from the arrangement of the rolling mills, could not be easily made in mild steel, had been made in puddled steel, and gave similar results, though in somewhat lower figures. Out of four experiments they showed a breaking strength of 4.43 kg. per square mm. (27.9 tons per square inch), a limit of elasticity 28.5 (18.0 tons per square inch), extension 21.8 per cent., and contraction 23.7 per cent. An interesting comparison was made of the results of experiments for these same bridges, with other materials used in constructing them, viz., cast steel, wrought and unwrought, cast iron and wrought iron. These experiments all show the superiority of mild steel. A girder made out of plates and angle iron sustained, after the highest test load, a calculated maximum bending strain of 45 kg. per square mm. (28.3 tons per square inch); a second, rolled in one piece, 48 kg. per square mm. (30.2 tons per square inch). The further increase of the load was prevented by the side bulging of the girder (against which it had not been secured), producing tiny cracks in the rivets on which the chief strain fell. The test pieces taken from these girders had shown a breaking strength of 48 kg. per square mm. in the mean. Two test girders of puddled steel, made by riveting plates to a rolled iron girder, yielded each time a maximum bending strain of 40 kg. per square mm. (25.2 tons per square inch) at the heaviest load, which, on account of the side bulging, could not be increased, although no cracks were visible. Test pieces from these girders showed a mean breaking strength of 47.3 kg. per square mm. (29.8 tons per square inch). This shows that the agreement in the experiments made between built-up girders and test pieces of the same material is satisfactory for practical purposes, especially in the case of mild steel.

In order to ascertain how their different qualities affect wrought iron and hard and soft steel, when variously worked, especially with regard to breaking point. Herr Intze recently made about 80 experiments with seven varieties of iron and steel. Dr. Forchheimer was good enough to take upon himself the greater part of the superintendence, and to note down the results, from which Herr Intze draws the following conclusions:

1. Drilling the bars does not diminish the breaking strength, but usually increases it slightly.

2. Punching diminishes the strength in almost all kinds of material.

3. Drilling considerably diminishes the work done up to breaking point (*i.e.*, the product of the stress and extension) compared with a solid bar, especially in hard materials.

4. Punching diminishes the work done considerably more than drilling.

5. Punching succeeded by annealing again raises the work done to nearly the same point as drilling without annealing.

6. With a drilled and riveted joint, after removing the rivet heads, a marked diminution of the work done up to breaking point, as compared with a joint simply drilled, is apparent, and this diminution increases with steel as the material becomes harder. With steel of about 65 kg. per square mm. breaking point (41 tons per square inch) where the section is weakened by about 46 per cent. in a test bar 150 mm. long—*i.e.*, the holes being about 150 mm. (5.9 inches) apart in the direction of the pull—with holes of about 15 mm. diameter, and after riveting up the work done was only $\frac{1}{200}$ part of that in a similar solid bar. With a similar weakening of section in a bar 1,500 mm. long—*i.e.*, the rivets being 1.500 mm. (5 feet apart) in the direction of the pull—the work done up to breaking was only $\frac{1}{2000}$ part that of a solid bar.

7. In wrought iron, with a section reduced by 46 per cent., no diminution of the work done worth heeding takes place through riveting, beyond what is caused by drilling alone. The work done is, under equal conditions, usually five times as great as in steel, with a breaking strength of 65 square mm.

8. Reductions of section, which are caused simply by drilling, show, when varying from 20 to 50 per cent., a diminution of the work done up to breaking point, according to the amount of the reduction; and this work is much smaller with hard than with mild steel.

Herr Intze considers that in these results an explanation may be found of the fact that with very great reductions of area, such as those occurring in the joints of a steam boiler, sudden failures have been observed in quite new steel boilers. On account of the high limit of elasticity, and of strains caused in the working, it might be that deformation, when combined with unequal heating, would prove, when the weakened line of rivets was tested, to have already reached the breaking point, although in the solid plate the limit of elasticity would not have been surpassed. The very low value of work done in the much weakened lines of rivets in steel plates could only admit of a very slight deformation in the boiler, and a breakage might therefore easily occur. To meet these cases it would be very desirable to find a material whose limit of elasticity, as compared with the breaking point, was small, unless the weakness of the line of rivets could be so greatly diminished that the deformation in the whole structure must considerably overpass the limit of elasticity before the breaking point of the line of rivets was reached. The speaker suggested that, having regard to the above results, those parts of structures which were both exposed to severe tension, and weakened by rivets, might be strengthened against shocks and deformation by making as many holes as possible in the line of rivets, provided the proper net section was preserved. It would of course really be best if the same net section could be maintained throughout in any such portion, as the same strength would then be preserved throughout the whole length to resist shocks. Herr Intze concluded by expressing a hope that the union would form a committee to investigate the matter, and decide on the relative merits of iron and steel.

Herr André thanked Herr Intze for his valuable paper, although he differed from his conclusions, and then said:—Two years ago we replaced the iron shaft of a high speed steam engine by a Bessemer steel one, manufactured at a works which, for such articles, stands almost, if not quite, equal to Krupp and Bochum. Atfer two years' work the shaft broke suddenly when moving very slowly, and without any load attached (the train of rolls was not coupled). We were about to replace it with another Bessemer shaft of mild steel, when the manager of the steel works himself advised us to return to the old material if there was any risk of the shaft becoming occasionally heated while working, for, he said, strong cooling appliances were then brought at once

to bear on the shaft, which produced microscopic cracks on the surface, and these in a short time, might produce an unexpected breakage. In order to confirm this, he (the speaker) telegraphed to ten wireworks where similar engines were in use, and found that the majority employed and preferred iron. It appears, therefore, that iron shafts are again replacing the steel ones which had superseded them, for quick and heavy work, where there is any risk of heating. This was further confirmed by the foreman of an engine works on the Rhine, who stated that in two years they had had five breakages of heavy steel shafts, and had in each case replaced them with iron. In one of these steel shafts, of 400 mm. diameter (15.75 inches) a hole as large as the fist was found, and in it were two small steel balls, which had ground themselves quite round and smooth in the two years. As mild steel is even more inclined to form bubbles than hard, it seems to be, at present, even less adapted for heavy shafts and pieces.

It appeared to be as little adapted for steam boilers. The same engineer, for example, had informed him that a boiler he had constructed cracked right across the bottom when removed from the workshop to the survey yard, although great care had been taken in riveting and drilling the holes.

The constructor should remember also that "mild steel" is a very vague term; and it is necessary to ascertain where and by what process it is made. Nothing can be determined by the amount of carbon it contains, for, although one can estimate approximately, according to the carbon contained, the materials produced by a certain process at a certain works, it is impossible to compare the productions of different works, especially where different processes are adopted. For example, Thomas steel, containing 1 to 1.5 of carbon may be exceedingly soft, while Bessemer steel, containing the same amount, may be so hard that a knob occurring on a thin wire rod can hardly be rolled away.

The differences in Thomas steel itself are very great, and although he could speak very favorably of it from personal experience, there were many complaints of inequalities. If an engineer, therefore, resolves to have no mild steel, but only ingot iron, he will find the border line between the two so confused that the following definition may be looked upon as a necessary expedient: "No material capable of considerable hardening shall be called iron," and, if narrowly examined, it will be seen that a great deal of the ingot iron specified as "incapable of considerable hardening" is, nevertheless, capable of very considerable hardening under certain circumstances, such as the sudden cooling of a heated shaft. This "inconsiderable hardening" is just sufficient to shrink the surface, produce tension, small cracks, and finally breakages.

This shows that the puddling furnaces are not so entirely superseded as it has been the fashion to assert, and it has become clear that such furnaces have too rapidly diminished, as the demand for puddled bar has, of late years, frequently exceeded the supply.

One may consider that ingot iron is now preferred to cast steel for the following purposes: Rails, railway axles and shafts of similar dimensions, railway sleepers, and, above all, tires. For heavy quick running shafts, iron is in request, except for very heavy marine shafts, for which crucible steel is frequently employed. For shipbuilding, the Simens-Martin ingot iron-plates are increasingly employed, while for boiler-plates wrought iron holds undisputed sway; for wire rods, cast steel and wrought iron are about equally in demand. There is no doubt that with improvements in the management of steel processes, especially in soft steel and ignot iron, the demand for it will increase; but it is probable that, for some time to come, the existing processes will continue rivals in the same fields.

As the refining process still exists side by side with its successor, the puddling process, so will the puddling process itself survive in company with its younger rivals the Bessemer, Martin, and Thomas systems, and, thanks to its malleability, wrought iron will long find manufacturers glad to employ it.

Herr Krohn added the following observations: We find from Herr Offergeld's experiments that several girders were spoiled by bending, which occurred either in the upper girder or in the upper portion of the web in those parts most exposed to pressure. The compression

portions of an ingot iron girder showed flaws before the breaking strength of the loaded parts had been fully attained, whilst with a girder of wrought iron a uniform stress of the whole section was attained, and therefore a relatively higher breaking-point was reached. This circumstance may be explained by the properties of both materials. As the breaking-point is reached the relation between deformation and strain becomes greater with mild steel than with wrought iron. As it is well known that with a breaking load the amount of the moment of rupture increases with the deformation, it follows that the risk of breakage is greater with ingot than with wrought iron. Therefore, in order to maintain a uniform stress on both the compression and tension portions of an ingot iron girder, the compression portion must be strengthened to a far greater extent than is necessary with wrought iron. It seems doubtful, and on this point Herr Offergeld's experiments offer no conclusion, whether, in spite of the extra material necessary to strengthen the compression flange of an ingot iron girder in order to attain uniform stress on the whole cross section, a saving is not effected on the whole as against wrought iron by the attainment of a higher breaking point, and on this account we must hesitate before agreeing with Herr Offergeld's unfavorable opinion of ingot iron derived from his experiments."

Herr Petersen supplemented the remarks of Herr André as follows: "Ingot iron and cast steel are both cast, have a similar granular structure and fracture, and in these respects possess unquestionable advantages over wrought iron, which is composed of successive layers, and consequently shows defects of welding, rendering it a bad material for many purposes. When these kinds break, the difference in behavior corresponds to their difference in structure. The homogeneous material breaks quick and short, whereas wrought iron, owing to its laminated structure, breaks slowly layer by layer under the bending strain. In spite, therefore, of its lower breaking strain, wrought iron offers in many cases greater security than steel, because the breakage, instead of occurring quickly and suddenly, takes place gradually, and generally gives warning of the danger by a harmless tear in the outside layer."

He had previously shown that unequal heating and careless working were apt to produce small cracks and flaws, as well as internal tension, in homogeneous materials, and these diminished the strength and prepared the way for breakages. Striking examples of this occurred in a large rail rolling mills with some cast steel crank shafts, which, after being only a short time in use, showed such serious flaws and cracks on the journals, brought on by one-sided heating while working, that they were changed for wrought iron.

It is well known that various difficulties occur in casting thick steel ingots, and it is even more frequent to have a porous casting in mild than in hard steel. If steel ingots have incomplete, hollow, or porous spots, these do not become welded together by further heating and working, but after being rolled thin they retain their porosity, as unwelded spots are retained in wrought iron. As these porous places are generally in the center of the ingot, the round bars, piston rods, and axles made of it have also usually an internal weakness, which it is difficult to set right in the working, and which may cause breakages in the future. Thus the plunger rod of a subterranean pumping engine, on being tested with water up to 20 atm., was shown to be hollow in its whole length, and water burst out of the cleft at the end of the rod. The following accident with the piston-rod of a pumping engine occurred at a Westphalian coalpit. The piston-rod in question belonged to a single-acting Cornish beam engine, with a cylinder diameter of 2 meters (6.56 feet) and a stroke of 4 meters (13.12) feet. The effective steam pressure was 4.5 atm., the vacuum about 600 mm. (23.6 inches), so that the net effective pressure on the piston was 5.3 atm., amounting altogether to 164,000 kg. (160 tons). The diameter of the piston rod was 250 mm. (9.84 inches). The break occurred at the beginning of the descent, so that 3½ meters of the stroke (11.5 feet) had yet to be accomplished. Falling from this height, the piston struck the bottom of the cylinder and smashed with it the valve boxes and all the principal parts of the engine. The

breaking strain of the piston amounted in the full transverse section only to about 3.1 kg. per square mm. (2.2 tons per square inch), and in the hollow place, where about 12 square mm. were lost, to about 4.5 kg. per square mm. (9.8 tons per sq. inch). A lead casting the size of the fist was taken out of this hole. These examples are not meant to detract from the valuable qualities of cast steel, but merely to point out those weaknesses in it which must be guarded against in practice.

VARIOUS METHODS OF DETERMINING DIMENSIONS.

By DR. JAMES WEYRAUCH, Professor at the Polytechnic of Stuttgart.

Selected Papers of the Institution of Civil Engineers.

II.

VII. Lippold's Method.

Whereas the methods hitherto mentioned leave it unexplained why by variable loads fracture is produced more easily than by a static load, engineer Lippold of Wiesbaden gives Wöhler's laws the following form:* "In order to fracture a piece, a certain amount of work is necessary, and this may be accumulated in the material at once, as well as by repeated intermittent loads. These loads, however, must recur instantaneously, or within so short a time that vibrations are produced." It follows that only through the occurrence of vibrations can extension, and with it stress and work, equal to what will overcome the statical breaking strength, be produced by a load lower than the statical breaking load.

Importance is attached to repetitions of the load only in so far as by these the limit of elasticity can be brought up nearly to the limit of fracture, in which case the relations depending on the proportionality of stress and extension are approximately applicable up to the point of fracture. In conformity with the preceding interpretation of Wöhler's law, Lippold, in determining the admissible stress, starts from the following rule: "On no member of a structure shall more work be performed by the fixed and live load than would be done by a weight slowly increasing from nothing up to the amount of the static load considered admissible." If for a piece subjected to tension or compression l denote the original length, F the sectional area, x the momentary extension or compression, E the modulus of elasticity, the piece tries to regain its original condition with a force

$$(a) \qquad X=\frac{FE}{l}\,x=\frac{x}{a}.$$

A further extension by the amount dx may be produced by the work $X\,dx$, and the extension of the piece from 0 to x requires the work

$$(b) \qquad \int_0^x X\,dx=\frac{x^2}{2a}=\frac{Xx}{2}=\frac{aX^2}{2}.$$

If the load increases gradually from nil up to the admissible static load $R=Fb_r$ the work done is spent only in overcoming the elastic force, and the work accumulated in the piece is by (b)

$$(c) \qquad A=\frac{aR^2}{2}=\frac{a}{2}\,(Fb_r)^2.$$

According to the above rule more work than this must not be put into the piece (in other words, the potential energy corresponding to the elastic force must not exceed this value.)

Let there be equilibrium at a given moment between the load P acting on the piece and the elastic force. Then the piece* contains the work $\frac{aP^2}{2}$, the alteration of length according to (a) is $\lambda=a$ P. Now let a new load Q, acting in the same sense as P, be suddenly added; then the load exceeds the resistance, a part of the work of Q is transformed into kinetic energy (*vis viva*), which,

* H. Lippold. "Die Inanspruchnahme von Eisen und Stahl mit Rucksicht auf bewegte Last."

*P may also have been suddenly applied. In this case the alteration of length would, in the first instance, have been greater than λ, vibrations about the position of equilibrium λ take place; but when it is attained only the energy $\frac{aP^2}{2}$ remains behind in the piece, the rest has been principally converted into heat.

however, is gradually expended in overcoming the increasing resistance. At that moment, when no more kinetic energy remains and the greatest alteration of length $\lambda+\lambda'$ has been attained, the work done by the loads—apart from other applications (heat)—must be equal to the work of the elastic force overcome. The latter work by (b) amounts to $\frac{(\lambda'+\lambda)^2}{2\alpha}$, so that

$$(d) \qquad \frac{\alpha P^2}{2}+(P+Q)\lambda'=\frac{(\lambda'+\lambda)^2}{2\alpha}$$

and hence with reference to $\lambda=\alpha P$

$$(e) \qquad \lambda'=2\alpha Q.$$

The new alteration of length is accordingly independent of that previously existing, and twice as great as with a gradually applied load. If now the total work spent on the piece is not to exceed the value (c), there follows from (d)

$$(f) \qquad \frac{\alpha P^2}{2}+(P+Q)2\alpha Q=\frac{\alpha}{2}(Fb_r)^2,$$

and hence the requisite sectional area

$$(f) \qquad F=\frac{P+2Q}{b_r}.$$

Suppose that equilibrium exists again between a load P acting on the piece and the elastic force. Momentary alteration of length λ, work accumulated in piece $\frac{\alpha P^2}{2}$. Suddenly a load $Q>P$, acting in the opposite sense to the latter, is applied, so that the alteration of length λ ceases, and one in the opposite sense $\lambda'-\lambda$ is produced. In neutralizing λ the force $Q-P$ and the elastic force act in the same sense, resistance only beginning with the alteration of length $\lambda'-\lambda$ in the opposite direction; and neglecting other applications of the work

$$(g) \qquad \frac{\alpha P^2}{2}+(Q-P)\lambda'=\frac{(\lambda'-\lambda)^2}{2\alpha}.$$

(As the original length of zero of the elastic force is approached, the potential energy $\frac{\alpha P^2}{2}$ is converted into kinetic, and this, as well as the additional work $(Q-P)\lambda'$ coming from an external source, are, when the zero is passed, again transformed into potential energy, the value of which at the distance $\lambda'-\lambda$ from the zero is $\frac{(\lambda'-\lambda)^2}{2\alpha}$).

As in the first case, equation (g), taken in conjunction with $\lambda=\alpha P$, gives

$$(e) \qquad \lambda'=2\alpha Q.$$

Here also the total alteration of length resulting from Q is independent of that previously existing, and twice as great as with a gradually applied load.

If, again, the work spent on the piece is not to exceed the value (c), then follows with regard to (e)

$$(h) \qquad \frac{\alpha P^2}{2}+(Q-P)2\alpha Q=\frac{\alpha}{2}(Fb_r)^2,$$

and hence the requisite sectional area

$$(i) \qquad F=\frac{2Q-P}{b_r}.$$

If, now, a member of a bridge has to be calculated, equilibrium may be supposed established, not only after the action of the fixed load, but after every straining action. If, then, a new load be suddenly applied, the preceding formulæ are applicable. In calculating the sectional area, however, that case must be selected which gives the greatest value of F. Accordingly we obtain the necessary section and the admissible stress per unit of area:

For tension or compression only from (f), $P+Q=\max B$, $P=\min B$

$$F=\frac{2\max B-\min B}{b_r} \quad . \quad . \quad . \quad . \quad (51)$$

for alternate tension and compression from (i) where $Q-P=\max B$, $P=\max B'$

$$F=\frac{2\max B+\max B'}{b_r} \quad . \quad . \quad . \quad (52)$$

and with regard to (1) and (2) in both cases

$$b=\frac{\max B}{F}=\frac{b_r}{2-\varphi} \quad . \quad . \quad . \quad (53)$$

in these formulæ Lippold substitutes for wrought iron the value $b_r=1{,}300$, for unhardened cast steel $b_r=2{,}400$ per square centimeter.

Formula (53) agrees for tension only or compression only with Ritter's (45), in which $K=2$

In Lippold's very remarkable work views are put forward and developed which, previous to Wöhler's labors, had many supporters, and the correctness of which cannot be disputed. The question

is, however, whether this view of the subject is not one-sided, whether the destruction of the material is solely, or even mainly, due to the causes indicated.

If that be the case, then, Wöhler's law and results would lose much of their significance, as they are unnecessary for the development of the formulæ and numerical values given.

It is certainly not creditable that Lippold's work has remained almost unnoticed in Germany, especially on the part of those to whom the management of technical experiments is entrusted. The experiments hitherto made leave no doubt as to the general law, but only as to the causes of it.

Although every one is at present free to choose whether he will accept a theoretical explanation or simply remain contented with the experimental results, it is most certain from Lippold's theory, that the use of a constant coefficient of strength for determining the dimensions of engineering structures is totally indefensible.

VIII.—Clericetti's Equations.

The latest paper on the application of Wöhler's experiments is by Professor Clericetti of the Technical Institute of Milan.* Clericetti arrives in the first instance at the same conclusion as Lippold, but subsequently investigates separately the influence of repetitions of the load.

A sudden application of a load to a piece causes temporarily twice the extension (VII.), and therefore twice the increase of stress due to the same load gradually applied. This applies in the first instance to stresses within the limit of elasticity, as, however, the latter, by being exceeded, may be brought up nearly to the limit of fracture, Clericetti considers that a sudden load should be treated even until fracture occurs, as a quiescent load of twice the magnitude. As a further test, for a piece having a unit of sectional area, and to which the stress due to fixed load c, the difference of stress in the same sense d, and a statical breaking strength, t, apply, the formula is given

$$(a) \qquad c+2d=t$$

*C. Clericetti, "Sulla determinazione dei coefficienti di sferzo specifico dietro le esperienze di Wöhler," Politecnico, 1881.

The stress at the moment of fracture by the ordinary method would be

$$(b) \qquad c+d=a$$

so that the ultimate working strength for a load quiescent only as to one portion c is

$$(c) \qquad a=\frac{c+t}{2}.$$

This formula agrees very well with Wöhler's results, as the following comparison shows:

For t=1,100 (compare IV.) and					
$c=$	0	250	400	600	1,100
by Wöhler $a=$	500	700	800	900	1,100
" formula (c) $a=$	550	675	750	850	1,100

It must, however, be remembered that in these experiments fracture occurred, not after a few applications of load, but only after 40,000,000 and more; while Kirkaldy found that with a sudden application the breaking weight varied for different kinds of iron from 75.2 to 90.4 per cent., on an average 81 per cent.* of that required with a gradual application.

Lippold ascribes this deviation of 50 per cent. to the circumstance that the loads had not sufficient time to produce the full extension and with it the full stress, the energy not used in overcoming the resistance of the piece being converted into heat.

Formula (a) is distinguished from that with which Gerber starts (I (a))

$$c+\tau d=t$$

only by the choice of the coefficient τ, which Gerber determines from experimental results. In fixing the admissible stress Clercietti proceeds as follows:

Repeated Loads in one sense.—Let B_0, B_1, denote respectively the fixed and live loads max $B=B_0+B_1$, m a factor of safety, u the primitive strength, then Clericetti writes

$$F=\frac{B_0+2B_1}{u}m=\frac{2\max B-B_0}{u}m \quad . \quad (54)$$

According to the usual method the stress per unit of area, and with regard to (1) is

$$b=\frac{\max b}{F}=\frac{B_0+2B_1}{B_0+B_1}\,\frac{u}{m}=\frac{1}{2-\varphi_0}\,\frac{u}{m} \, . \quad (55)$$

If the load is in the same sense on the whole, moving loads in opposite senses

* Von Kaven's "Collectaneen," Zeitschrift des hannöv. Arch. u. Ing. Vereins' 1868.

may still occur. Clericetti would apply the preceding formulæ to this case also.

Particulars are not given; but it appears that then φ should be substituted for φ_0 (which is correct when $B_0 = \min B$), whence generally

$$b = \frac{1}{2-\varphi}\frac{u}{m} \quad . \quad . \quad . \quad (56)$$

The formula differs from Lippold's (53), in so far as in the latter $\frac{t}{m} = b_r$ takes the place of $\frac{u}{m}$. With a suitable choice of constants, both these formulæ, as well as Ritter's (45) lead to equal values of b.

Repeated Loads in opposite senses.—Let B_0, B_1, B_2, denote the numerical values of the fixed load and the extreme live load. On the assumption of the sudden application of the live load the actual stress varies according to Clericetti —when B_0 B_1 act in the same sense—between

$$\pm\frac{B_0+2B_1}{F} \text{ and} \mp \frac{2B_2}{F},$$

when B_0, B_2 denote loads in the same sense

$$\pm\frac{B_0+2B_2}{F} \text{ and} \mp \frac{2B_1}{F};$$

hence in both cases the difference of stress would be

$$(d) \qquad d = \frac{B_0+2B_1+2B_2}{F};$$

and s being the vibration strength, Clericetti makes the sectional area

$$F = \frac{B_0+2B_1+2B_2}{2s} m \quad . \quad . \quad . \quad (57)$$

where $m\, d = 2\, s$ has been introduced. As by (19) and (20) $B_1 + B_2 = \max\, B + \max\, B_1$ there follows also

$$F = \frac{\max B + \max B_1 + \frac{1}{2}B_0}{s} m \quad . \quad . \quad (58)$$

and having regard to (2) the stress per unit of area is

$$b = \frac{\max B}{B_1+B_2+\frac{1}{2}B_0}\frac{s}{m} = \frac{1}{1-\varphi \pm \frac{1}{2}\varphi_0}\frac{s}{m} \quad (59)$$

Herein the upper or lower sign is applicable to φ_0, according as the fixed load is of the same or opposite kind as the upper limiting load, that is, always that sign which will make the quantity $\pm\frac{\varphi_0}{2}$ positive.

Numerical Values.—Clericetti takes the following values of t, u, s, as averages from Wöhler's experiments, for iron 3,800, 2,450, 1,385, for steel 6,800, 3,400, 2,000, adopts the factor of safety $m=2$, and thus obtains, in the case of iron, for tension only or compression only

$$F = \frac{B_0+2B_1}{1{,}250} \quad . \quad . \quad . \quad . \quad . \quad (60)$$

$$b = 1{,}250\frac{B_0+B_1}{B_1+2B_1} = \frac{1{,}250}{2-\varphi_0} \quad . \quad . \quad . \quad (61)$$

For alternate tension and compression

$$F = \frac{B_1+B_2+\frac{1}{2}B_0}{650} \quad . \quad . \quad (62)$$

$$b = 650\frac{\max B}{\frac{1}{2}B_0+B_1+B_2} = \frac{650}{1-\varphi\pm\frac{1}{2}\varphi_0} \quad (63)$$

According to the principles of the preceding method itself, for alternate tension and compression, the actual stress should not be assumed as above, but as varying between

$$\pm\frac{B_0+2B_1}{F} \text{ and } \pm \frac{B_0-2B_2}{F},$$

and $\quad \pm\frac{B_0+2B_2}{F} \text{ and} \pm \frac{B_0-2B_1}{F},$

respectively. (*Vide* also X).

Hence would follow, in a way similar to the preceding

$$(e) \qquad d = \frac{B_1+B_2}{F}2$$

$$b = \frac{1}{1-\varphi}\frac{s}{m} \quad . \quad . \quad . \quad . \quad (64)$$

Formulæ (56) (64) give, then, for iron, with tension or compression only,

$$b = \frac{1{,}250}{2-\varphi} \quad . \quad . \quad . \quad . \quad (65)$$

and with alternations of tension and compression, on account of $s=1{,}385$ and $m=2$,

$$b = \frac{700}{1-\varphi} \quad . \quad . \quad . \quad . \quad . \quad (66)$$

For $\varphi=0$, $\varphi_0=0$, equal values of b should result from the formulas for tension and compression only, and from those for alternate tension and compres-

sion. The way in which u and s are introduced into the above formulas can, however, scarcely be justified, and, after what has been said of a at the commencement, is surprising.

Equation (64), with a suitable choice of constants, gives the same results as a rule followed by American engineers previous to Wöhler's experiments, according to which for alternate tension and compression.

$$F=\frac{\max B+\max B_1}{c} \quad . \quad . \quad . \quad (67)$$

where c denotes the ordinary stress for tension alone. The stress per unit of area would be

$$b=\frac{\max B}{F}=\frac{c}{1-\varphi} \quad . \quad . \quad . \quad . \quad . \quad (68)$$

this formula coincides with (64) for $c=\frac{u}{m}$.

IX.—Examples.

For a better comprehension of the various methods of determining dimensions some examples shall be calculated.

As almost all authors have left liability to buckling unnoticed, it shall remain excluded here. It may be taken into account in a way which is for all methods analogous to that described in the author's previous communication for the Launhardt and Weyrauch system.* The latter gave for tension or compression only without liability to buckling

$$b=v(1+m\varphi);\quad F=\frac{\max B}{v(1+m\varphi)}; \quad . \quad . \quad (69)$$

and for alternate tension and compression

$$b=v(1+n\varphi);\quad F=\frac{\max B}{v(1+n\varphi)} \quad . \quad . \quad (70)$$

On the authority of Wöhler's experiments for iron the values were adopted $v=700$, $m=n=\frac{1}{2}$, whence generally

$$b=350(2+\varphi);\quad F=\frac{\max B}{350(2+\varphi)} \quad . \quad . \quad (71)$$

As, however, the choice of constants depends on the constructive material, the object of the structure and experience of the designer, no great weight was attached to the numerical values, and, for example, the less favorable data

$$b=320(2+\varphi);\quad F=\frac{\max B}{320(2+\varphi)} \quad . \quad . \quad (72)$$

were also given.

* Minutes of Proceedings Inst. C.E., vol. lxiii., p. 290.

Example 1.—For the boom of a girder-bridge which is always in tension (or always in compression), the load varies between the maximum value max B=30,000 and the minimum value, due to the fixed load only, min B=10,000 kilograms. The requisite net sectional area is to be determined.

(1) By the old method with $b=700$ there follows:

$$F=\frac{30{,}000}{700}=42.86 \text{ square centimeters.}$$

(2) By Gerber's method with $B_c=10{,}000$, $B_v=20{,}000$ from (8)–(10).

$$\varphi=\frac{10{,}000}{1.5\times 20{,}000}=\tfrac{1}{3}.$$

$$\delta=\tfrac{1}{4}(3+\sqrt{16\times\tfrac{1}{9}+16\times\tfrac{1}{3}+13})\times 1.871.$$

$$F=\frac{1.5\times 1.871\times 20{,}000}{1{,}600}$$

$$=35.08 \text{ square centimeters.}$$

(3) By Schaffer's method with $B_0=10{,}000$, $B_1=20{,}000$, $B_2=0$ from (23)–(25).

$$\xi=\frac{1.5\times 20{,}000}{1.5\times 20{,}000+10{,}000}=\tfrac{3}{4}.$$

$$\frac{a}{m}=\frac{-3\times\frac{3}{4}+\sqrt{13\times\frac{9}{16}-16\times\frac{3}{4}+16}}{(2-\frac{3}{4})^2}1{,}600$$

$$=1{,}140.$$

$$F=\frac{1.5\times 20{,}000+10{,}000}{1{,}140}$$

$$=35.08 \text{ square centimeters.}$$

(4) By Winkler's method with $B_0=10{,}000$, $B_1=20{,}000$, $B_2=0$ from (32).

$$F=\frac{10{,}000}{1{,}400}+\frac{20{,}000}{590}$$

$$=41.04 \text{ square centimeters.}$$

(5) By Seefehlner's method with $\varphi=10{,}000 : 30{,}000=\frac{1}{3}$ from (44).

$$F=\frac{4-\frac{1.6}{3}}{2+\frac{1}{3}}\cdot\frac{30{,}000}{1{,}200}$$

$$=37.14 \text{ square centimeters.}$$

(6) By Launhardt and Weyrauch's method with $\varphi=\frac{1}{3}$ from (71).

$$F=\frac{30{,}000}{350(1+\frac{1}{3})}=36.73 \text{ square centimeters.}$$

(7) By Ritter's method with $\varphi=\frac{1}{3}$ from (48).

$$F=\frac{30{,}000(2-\frac{1}{3})}{1{,}200}$$

$$=41.67 \text{ square centimeters.}$$

(8) By Lippold's method from (51).

$$F=\frac{2\times 30{,}000-10{,}000}{1{,}300}$$

$=38.46$ square centimeters.

(9) By Clericetti's method, with $B_0=10{,}000$, $B_1=20{,}000$ from (60).

$$F=\frac{10{,}000+2\times 20{,}000}{1{,}250}$$

$=40.00$ square centimeters.

Example 2.—For a member of a bridge always in tension (or always in compression) the greatest load max $B=30{,}000$, the smallest min $B=10{,}000$, as above, but the fixed load $B_0=20{,}000$ kilograms. The net sectional area is to be determined.

(1) By the old method as above, $F=42.86$ square centimeters.

(2) According to Gerber with $B_c=20{,}000$, and $B_v=10{,}000$.

$$\varphi_1=\frac{20{,}000}{1.5\times 10{,}000}=\frac{4}{3}.$$

$$\delta_1=\tfrac{1}{4}\left(3+\sqrt{16\times\frac{16}{9}+16\times\frac{4}{3}+13}\right)=2.731.$$

$$F_1=\frac{1.5\times 2.731\times 10{,}000}{1{,}600}=25.60,$$

then with $B_c=20{,}000$, $B_v=-10{,}000$.

$$\varphi_2=\frac{20{,}000}{1.5\times 10{,}000}=-\frac{4}{3}.$$

$$\delta_2=\tfrac{1}{4}\left(3+\sqrt{16\times\frac{16}{9}-16\times\frac{4}{3}+13}\right)=1.871.$$

$$F_2=\frac{1.5\times 1.871=10{,}000}{1{,}600}=17.54,$$

and together

$F=25.60+17.54=43.14$ square centimeters.

(3) According to Schaffer with $B_0=20{,}000$, $B_1=10{,}000$, $B^2=10{,}000$ from (23)—(25).

$$\xi=\frac{1.5(10{,}000+10{,}1000)}{1.5\times 10{,}000+20{,}000}=\frac{6}{7}.$$

$$\frac{a}{m}=\frac{-3\times\frac{6}{7}+\sqrt{13\times\frac{36}{49}-16\times\frac{6}{7}+16}}{(2-\frac{6}{7})^2}1{,}600$$

$=1{,}055.$

$$F=\frac{1.5\times 10{,}000+20{,}000}{1{,}055}$$

$=33.18$ square centimeters.

(4) According to Winkler with $B_0=20{,}000$, $B_1=10{,}000$, $B_2=10{,}000$, from (32).

$$F=\frac{20{,}000}{1{,}400}+\frac{10{,}000}{590}+\frac{10{,}000}{1{,}300}$$

$=38.93$ square centimeters.

(5) According to Seefehlner as above $F=37.14$ sq. cent.

(6) " " Launhardt and Weyrauch $F=36.73$ sq. cent.

(7) " " Ritter as above $F=41.67$ "

(8) " " Lippold as above $F=38.46$ "

(9) " " Clericetti with $\varphi=\frac{1}{3}$ $F=40.00$ sq. cent.

Example 3.—A piece sustains a maximum tensile load of max $B=40{,}000$ and a maximum compression of max $B_1=20{,}000$, while the fixed load in tension is $B^0=10{,}000$ kilograms. The net sectional area is to be determined

(1) According to the old method, with $b=700$

$$F=\frac{40{,}000}{700}=57.14 \text{ square centimeters.}$$

(2) According to Gerber from (8)—(10) with $B_c=10{,}000$, $B_v=30{,}000$.

$$\varphi_1=\frac{10{,}000}{1.5\times 30{,}000}=\frac{2}{9}.$$

$$\delta_1=\tfrac{1}{4}(3+\sqrt{16\times\tfrac{4}{81}+16\times\tfrac{2}{9}+13})=1.791.$$

$$F_1=\frac{1.5\times 1.791\times 30{,}000}{1{,}600}=50.37.$$

Then with $B_c=10{,}000$, $B_v=-30{,}000$.

$$\varphi_2=-\frac{10{,}000}{1.5\times 30{,}000}=-\frac{2}{9}.$$

$$\delta_2=\tfrac{1}{4}(3+\sqrt{16\times\tfrac{4}{81}-16\times\tfrac{2}{9}+13})=1.550.$$

$$F_2=\frac{1.5\times 1.550\times 30{,}000}{1{,}600}=43.59;$$

and together

$F=50.37+43.59=93.96$ square centimeters.

(3) According to Schaffer with $B_0=10{,}000$, $B_1=30{,}000$, $B_2=30{,}000$ from (23)-(25).

$$\xi=\frac{1.5(30{,}000+30{,}000)}{1.5\times 30{,}000+10{,}000}=\frac{18}{11}.$$

$$\frac{a}{m}=\frac{-3\times\frac{18}{11}+\sqrt{13\times\frac{324}{121}-16\times\frac{18}{11}+16}}{\left(2-\frac{18}{11}\right)^2}$$

1,6 CC=C4

$$F=\frac{1.5\times 30,000+10,000}{648}$$
$=84.88$ square centimeters.

(4) According to Winkler with $B_0=10,000$, $B_1=30,000$, $B_2=30,000$ from (32)

$$F=\frac{10,000}{1,400}+\frac{30,000}{590}+\frac{30,000}{1,300}$$
$=81.07$ square centimeters.

(5) According to Seefehlner with $\varphi=-20,000:40,000=-\frac{1}{2}$ from (44)

$$F=\frac{4+\frac{1.6}{2}}{2-\frac{1}{2}}\,\frac{40,000}{1,200}$$
$=106.67$ square centimeters.

(6) According to Launhardt and Weyrauch with $\varphi=-\frac{1}{2}$ from (71)

$$F=\frac{40,000}{350(1-\frac{1}{2})}=76.19 \text{ square centimeters.}$$

(7) According to Ritter with $\varphi=-\frac{1}{2}$ from (50)

$$F=\frac{40,000\times 0.5}{(1+0.5-\sqrt{1+0.5^2})600}$$
$=87.26$ square centimeters.

(8) According to Lippold from (52)

$$F=\frac{2\times 40,000+20,000}{1,300}$$
$=76.92$ square centimeters.

(9) According to Clericetti with $B_0=10,000$, $B_1=30,000$, $B_2=30,000$ from (62).

$$F=\frac{30,000+30,000+5,000}{650}$$
$=100.00$ square centimeters.

Example 4.—A piece sustains a maximum tensile load max $B=40,000$, and a maximum compressive load max $B'=20,000$, which latter, however, at the same time, represents the fixed load. The net sectional area is to be determined.

(1) According to the old method as above, $F=57.14$ square centimeters.

(2) According to Gerber with $B_e=-20,000$, $B_s=60,000$ from (8)–(10)

$$\varphi=-\frac{20,000}{1.5\times 60,000}=-\frac{2}{9}.$$

$$\delta=\tfrac{1}{4}\left(3+\sqrt{16\times\frac{4}{81}-16\times\frac{2}{9}+13}\right)=1.550.$$

$$F=\frac{1.5\times 1.550\times 60,000}{1,600}$$
$=87.19$ square centimeters.

(3) According to Schaffer with $B_0=20,000$, $B_1=60,000$, $B^2=0$ from (23)–(25)

$$\xi=\frac{1.5\times 60,000}{1.5\times 60,000-20,000}=\frac{9}{7}.$$

$$\frac{a}{m}=\frac{-3\times\frac{9}{7}+\sqrt{13\times\frac{81}{49}-16\frac{9}{7}+16}}{\left(2-\frac{9}{7}\right)^2}\,1,600$$
$=803.$

$$F=\frac{1.5\times 60,000-20,000}{803}$$
$=87.18$ square centimeters.

(4) According to Winkler with $B_0=20,000$, $B_1=60,000$, $B_2=0$ from (32).

$$F=-\frac{20,000}{1,400}+\frac{60,000}{590}$$
$=87.41$ square centimeters.

(5) According to Seefehlner as above
$F=106.67$ square centimeters.

(6) According to Launhardt and Weyrauch as above
$F=76.19$ square centimeters.

(7) According to Ritter as above
$F=87.26$ square centimeters.

(8) According to Lippold as above
$F=76.92$ square centimeters.

(9) According to Clericetti with $B_0=20,000$, $B_1=60,000$, $B_2=0$ from (62).

$$F=\frac{60,000\times 10,000}{650}$$
107.69 square centimeters.

X. Tables.

The following tables are intended to show in a concise form how the admissible stress per square centimeter for iron bridges varies according to the various methods of determining dimensions previously demonstrated. As hitherto, for tension or compression only

$$\varphi=\frac{\min B}{\max B}\qquad \varphi_0=\frac{B_0}{\max B},$$

and for alternate tension and compression

$$\varphi=-\frac{\max B'}{\max B}\qquad \varphi_0=\pm\frac{B_0}{\max B}.$$

In the latter case the upper or lower sign is applicable according as the fixed load (numerical value B_0) is in the same or opposite sense as the higher limiting load (numerical value max B).

TABLE A.—FOR TENSION OR COMPRESSION ONLY.

Method.	Formula.	φ_0	$\varphi=0.$	$\varphi=\frac{1}{4}.$	$\varphi=\frac{1}{2}.$	$\varphi=\frac{3}{4}.$	$\varphi=1.$
Old............	$b=c$	*ad lib.*	700	700	700	700	700
Mainz Bridge...	(74)	φ	533	640	800	1,067	1,600
Gerber........	(13)	φ	646	794	998	1,271	1,600
	(12)	$\frac{\varphi+1}{2}$	584	670	741	1,333	1,600
	(13)	1	703	891	1,140	1,402	1,600
Schäffer......	(29)	φ	646	794	998	1,271	1,600
	(29)	$\frac{\varphi+1}{2}$	680	845	1,067	1,333	1,600
	(29)	1	704	892	1,140	1,402	1,600
Winkler......	(38)	φ	592	692	832	1,044	1,400
	(38)	$\frac{\varphi+1}{2}$	632	733	871	1,074	1,400
	(38)	1	678	779	914	1,106	1,400
Seefehlner......	(43)	*ad lib.*	680	750	937	1,178	1,500
Launhardt & Weyrauch..	(71)	*ad lib.*	700	787	875	962	1,050
	(72)	"	640	720	800	880	960
Ritter..........	(47)	*ad lib.*	600	686	800	960	1,200
Lippold........	(53)	*ad lib.*	650	743	867	1,040	1,300
Clericetti......	(61)	φ	625	714	833	1,000	1,250

In table A, φ_0 may vary from φ ($B_0=$ min B) to 1 ($B_0=$max B), for instance for the booms of girder bridges

$$\varphi=\varphi_0=\frac{\min B}{\max B}=\frac{p}{q} \quad . \quad . \quad . \quad . \quad (73)$$

where p is the weight of the girder itself, and $q=p+z$ the total load on the latter per unit of length. Up to spans of 100 meters φ may attain about the value $\frac{1}{2}$. For comparison the values are also added of

$$b=\frac{p+z}{p+3z}1{,}600=\frac{1{,}600}{3-2\varphi} \quad . \quad . \quad (74)$$

which are those assumed to be admissible, as early as the year 1863, by Gerber, in calculating the Mainz railway bridge.*

In Table B, φ_0 may vary: (1) when the fixed load is of the same kind as the upper limiting load, from 0 to 1, and (2) when the fixed load is of the opposite kind to the upper limiting load from 0 to φ.

*Gerber in Zeitschrift des Vereins deutscher Ingenieure, 1865.

From both the preceding tables the inaccuracy of Gerber's division of the difference of stress (I) will be apparent, as it is impossible that the admissible stress b should vary with φ_0 in the irregular manner shown.

The remarkable values obtained by Clericetti in Table B are due to the assump ion mentioned towards the end of VIII. with regard to the actual stresses with a suddenly applied load. If this assumption is corrected aud formula (66) accepted, the values given in Table B as "American" are the result (compare VIII.).

If two pieces, subjected only to tension or only to compression, sustain equal maximum loads, max B, and equal minimum loads, min B, according to Table A and examples (1), (2), (IX.), the formulæ of Schaffer and Winkler give the mallest sectional area for that piece which sustains the greatest fixed load. The same remark applies to Gerber, in so far as the influence of the division of the

TABLE B.—ALTERNATE TENSION AND COMPRESSION.

Method.	Formula.	φ_0.	$\varphi=0$.	$\varphi=-\frac{1}{4}$.	$\varphi=-\frac{1}{2}$.	$\varphi=-\frac{3}{4}$.	$\varphi=-1$.
Old	$b=c$	*ad lib.*	700	700	700	700	700
Gerber........	(15)	φ	646	538	459	399	351
	(14)	$\frac{\varphi}{2}$	646	513	424	361	315
	(14)	0	646	517	431	369	323
	(14)	$\frac{1}{2}$	584	486	412	358	315
	(15)	1	703	569	474	404	351
Schäffer......	(29)	φ	646	538	459	399	351
	(29)	$\frac{\varphi}{2}$	646	544	464	403	354
	(29)	0	646	550	469	406	356
	(29)	$\frac{1}{2}$	680	563	473	405	354
	(29)	1	704	573	474	404	351
Winkler......	(38)	φ	592	518	460	413	376
	(38)	$\frac{\varphi}{2}$	592	525	471	428	391
	(38)	0	592	556	523	495	408
	(38)	$\frac{1}{2}$	632	591	555	522	427
	(38)	1	678	636	590	553	448
Seefehlner......	(43)	*ad lib.*	600	477	375	289	214
Launhardt & Weyrauch..	(71)	*ad lib.*	700	612	525	437	350
	(72)	"	640	560	480	400	320
Ritter..........	(49)	*ad lib.*	600	526	458	400	321
Lippold........	(53)	*ad lib.*	650	578	520	473	433
Clericetti.....	(63)	φ	650	473	371	306	260
	(63)	$\frac{\varphi}{2}$	650	495	400	335	289
	(63)	0	650	520	433	371	325
	(63)	$\frac{1}{2}$	520	433	371	325	289
	(63)	1	433	371	325	289	260
American.......	(66)	*ad lib.*	700	560	467	400	350

difference of the load previously referred to is not predominant. This is due to the fact that the live load is introduced into the calculation by the authors named, multiplied with a factor n, for the purpose of taking separately into account the influence of impact.

Of course none of the methods of calculation reviewed are intended to give a rigid law for determining dimensions, but merely as guides from which the practical engineer will deviate in one direction or another if there are reasons for so doing.

As formerly the value $b=$constant was taken, but with reference to particularly unfavorable conditions lower values also were adopted, so the methods reviewed assume $b=f(x, y \ldots)$, but leave free play for the recognition of any special circumstances. For calculating the parts of machinery it has latterly been thought desirable to systematically utilize Wöhler's results,* experience having led to the adoption of perfectly different admissible stresses for different ways of applying loads, which, however, in the cases of static tension, tension alternating with zero, and alternations of equal tension and compression confirmed Wöhler's ratio of 3:2:1 given by formulæ (71) (72). Undoubtedly all the new methods have their faults, but the method hitherto in use is quite indefensible.

* C. Bach: "Die Maschinenelemente, ihre Berechnung und Construction mit Rücksicht auf die neueren Versuche." Stuttgart, 1881.

SOME POINTS IN ELECTRIC LIGHTING.

By DR. JOHN HOPKINSON, F. R. S., M. Inst. C. E.

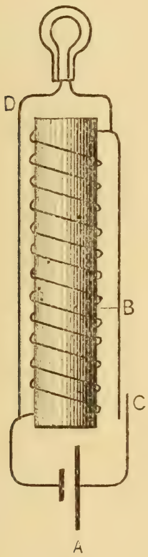
A Lecture delivered before the Institution of Civil Engineers.

Artificial light is generally produced by raising some body to a high temperature. If the temperature of a body be greater than that of surrounding bodies it parts with some of its energy in the form of radiation. Whilst the temperature is low these radiations are not of a kind to which the eye is sensitive; they are exclusively radiations less refrangible and of greater wave-length than red light, and may be called infra red. As the temperature is increased the infra red radiations increase, but presently there are added radiations which the eye perceives as red light. As the temperature is further increased, the red light increases, and yellow, green and blue rays are successively thrown off in addition. On pushing the temperature to a still higher point, radiations of a wave-length, shorter even than violet light, are produced, to which the eye is insensitive, but which act strongly on certain chemical substances; these may be called ultra violet rays. It is thus seen that a very hot body in general throws out rays of various wave-lengths, our eyes, it so happens, being only sensitive to certain of these, viz., those not very long and not very short, and that the hotter the body the more of every kind of radiation will it throw out; but the proportion of short waves to long waves becomes vastly greater as the temperature is increased. The problem of the artificial production of light with economy of energy is the same as that of raising some body to such a temperature that it shall give as large a proportion as possible of those rays which the eye happens to be capable of feeling. For practical purposes this temperature is the highest temperature we can produce. Owing to the high temperature at which it remains solid, and to its great emissive power the radiant body used for artificial illumination is nearly always some form of carbon. In the electric current we have an agent whereby we can convert more energy of other forms into heat in a small space than in any other way; and fortunately carbon is a conductor of electricity as well as a very refractory substance.

The science of lighting by electricity very naturally divides itself into two principal parts—the methods of production of electric currents, and of conversion of the energy of those currents into heat at such a temperature as to be given off in radiations to which our eyes are sensible. There are other subordinate branches of the subject, such as the consideration of the conductors through which the electric energy is transmitted, and the measurement of the quantity of electricity passing and its potential or electric pressure. Although I shall have a word or two to say on the other branches of the subject, I propose to occupy most of the time at my disposal this evening with certain points concerning the conversion of mechanical energy into electrical energy. We know nothing as to what electricity is, and its appeals to our senses are in general less direct than those of the mechanical phenomena of matter. The laws, however, which we know to connect together those phenomena which we call electrical, are essentially mechanical in form, are closely correlated with mechanical laws, and may be most aptly illustrated by mechanical analogues. For example, the terms "potential," "current," and "resistance," with which we are becoming familiar in electricity have close analogues respectively in "head," "rate of flow," and "coefficient of friction" in the hydraulic transmission of power. Exactly as in hydraulics, head multiplied by velocity of flow is power measured in foot-pounds per second or in horse-power, so potential multiplied by current is power and is measurable in the same units. The horse-power not being a convenient electrical unit, Dr. Siemens has suggested that the electrical unit of power or volt-ampere should be called a watt: 746 watts are equal to one horse-power. Again, just as water flowing in a pipe has

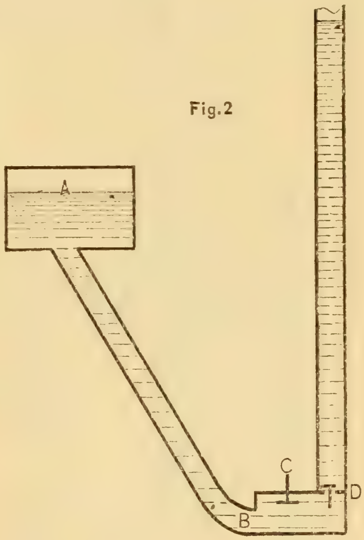
inertia and requires an expenditure of work to set it in motion, and is capable of producing disruptive effects if its motion is too suddenly arrested—as, for example, when a plug-tap is suddenly closed in a pipe through which water is flowing rapidly, so a current of electricity in a wire has inertia; to set it moving electro-motive force must work for a finite time, and if we attempt to arrest it suddenly by breaking the circuit, the electricity forces its way across the interval as a spark. Corresponding to mass and moments of inertia in mechanics we have in electricity coefficients of self-induction.

Fig. 1



We will now show that an electric circuit behaves as though it had inertia. The apparatus we shall use is shown diagrammatically in Fig. 1. A current from a Sellon battery A circulates round an electro-magnet B; it can be made and broken at pleasure at C. Connected to the two extremities of the wire on the magnet is a small incandescent lamp D, lent me by Mr. Crompton, of many times the resistance of the coil. On breaking the circuit, the current in the coil, in virtue of its momentum, forces its way through the lamp, and renders it momentarily incandescent, although all con nection with the battery, which in any case would be too feeble to send sufficient current through the lamp, has ceased. Let us try the experiment, make contact, break contact. You observe the lamp lights up. Compare with the diagram Fig. 2 of the hydraulic analogue the hydraulic ram. There a current of water suddenly arrested forces a way for a portion of its quantity to a greater height than that from which it fell. A B corresponds to the electro magnet, the valve C to the contact-breaker, and D E to the lamp. There is, however, this difference between the inertia of water in a pipe and the inertia of an electric current—the inertia of the water is confined to the water, whereas the inertia of the electric

Fig. 2

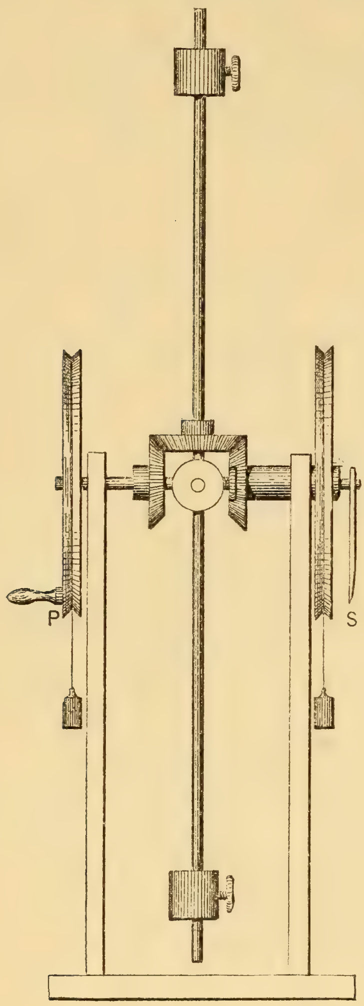


current resides in the surrounding medium. Hence arise the phenomena of induction of currents upon currents, and of magnets upon moving conductors—phenomena which have no immediate analogues in hydraulics. There is thus little difficulty to any one accustomed to the laws of rational mechanics in adapting the expression of those laws to fit electrical phenomena; indeed we may go so far as to say that the part of electrical science with which we have to deal this evening is essentially a branch of mechanics, and as such I shall endeavor to treat it.

This is neither the time nor the place for setting forth the fundamental laws of electricity, but I cannot forbear from showing you a mechanical illustration, or

set of mechanical illustrations, of the laws of electrical induction, first discovered by Faraday. I have here a model, Fig. 3, which was made to the instructions of the late Professor Clerk Maxwell, to illustrate the laws of induction. It consists of a pulley P, which I now

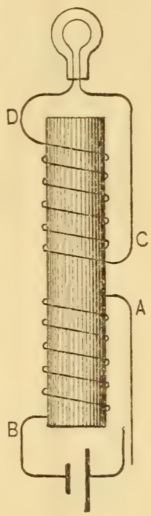
Fig. 3



turn with my hand, and which represents one electric circuit, its motion the current therein. Here is a second pulley S, representing a second electric circuit. These two pulleys are geared together by a simple differential train, such as is sometimes used for a dynamometer. The intermediate wheel of the train, however, is attached to a balanced fly-wheel, the moment of inertia of which can be varied by moving inwards or outwards these four brass weights. The resistances of the two electric circuits are represented by the friction on the pulleys of two strings, the tension of which can be varied by tightening these elastic bands. The differential train, with its fly-wheel, represents the medium, whatever it may be, between the two electric conductors. The mechanical properties of this model are of course obvious enough. Although the mathematical equations which represent the relation between one electric conductor and another in its neighborhood are the same in form as the mathematical equations which represent the mechanical connection between these two pulleys, it must not be assumed that the magnetic mechanism is completely represented by the model. We shall now see how the model illustrates the action of one electric circuit upon another. You know that Faraday discovered that if you have two closed conductors arranged near to and parallel to each other, and if you cause a current of electricity to begin to flow in the first, there will arise a temporary current in the opposite direction in the second. This pulley, marked P on the diagram, represents the primary circuit, and the pulley marked S on the diagram the secondary circuit. We cause a current to begin to flow in the primary, or turn the pulley P; an opposite current is induced in the secondary circuit, or the pulley S turns in the opposite direction to that in which we began to move the pulley P. The effect is only temporary, resistance speedily stops the current in the secondary circuit, or in the mechanical model friction the rotation of the pulley S. I now gradually stop the motion of P; the pulley S moves in the direction in which P was previously moving, just as Faraday found that the cessation of the primary current induced in the secondary circuit a current in the same direction as that which had existed in the primary. If there were a large number of convolutions or coils in the secondary circuit, but that circuit were not completed, but had an air space interrupting its continuity, an experiment with the well-known Ruhmkorff coil would show you that when the current was *suddenly*

made to cease to flow in the primary circuit, so great an electromotive force would be exerted in the secondary circuit that the electricity would leap across the space as a spark. I will now show you what corresponds to a spark with this mechanical model. The secondary pulley S shall be held by passing a thread several times round it. I gradually produce the current in the primary circuit: I will now suddenly stop this primary current: you observe that the electromotive force is sufficient to break the thread. The inductive effects of one electric circuit upon another depend not alone on the dimensions and form of the two circuits, but on the nature of the material between them. For example, if we had two parallel circular coils, their inductive effects would be very considerably enhanced by introducing a bar of iron in their common axis. We can imitate this effect by moving outwards or inwards these brass weights. In the experiment I have shown you the weights have been some distance from the axis in order to obtain considerable effect, just as in the Ruhmkorff coil an iron core is introduced within the primary circuit. I will now do what is equivalent to removing the core: I will bring the weights nearer to the axis, so that my fly-wheel shall have less moment of inertia. You observe that the inductive effects are very much less marked than they were before. With the same electro-magnet which we used before, but differently arranged, we will show what we have just illustrated—the induction of one circuit on another. Referring to Fig. 4, coil A B corresponds to wheel P; C D to wheel S; and the iron core to the fly-wheel and differential gear. The resistance of a lamp takes the place of the friction of the string on S. As we make and break the circuit you see the effect of the induced current in rendering the lamp incandescent. So far I have been illustrating the phenomena of the induction of one current upon another. I will now show on the model that a current in a single electric circuit has momentum. The secondary wheel shall be firmly held; it shall have no conductivity at all; that is, its electrical effect shall be as though it were not there. I now cause a current to begin to flow in the primary circuit, and it is obvious enough that a certain amount of work must be done to bring it up to a certain speed. The angular velocity of the fly-wheel is half that of the pulley representing the primary circuit. Now suppose that the two pulleys were connected together in such a way that they must have the same angular velocity in the same direction. This represents the coil having twice as many convolutions as it had before. A little consideration will show that I must do four times as much work to give the primary pulley the same velocity that it attained before; that is to say, that the coefficient of self-induction of a coil of wire is proportional to the square of the number of convolutions. Again, suppose that these two wheels were so geared together that they must always have equal

Fig. 4

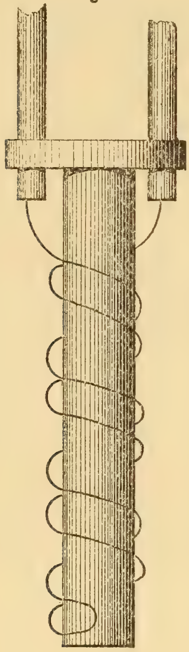


and opposite velocities, you can see that a very small amount of work must be done in order to give the primary wheel the velocity which we gave to it before. Such an arrangement of the model represents an electric circuit, the coefficient of induction of which is exceedingly small, such as the coils that are wound for standard resistances; the wire is there wound double, and the current returns upon itself, as shown in Fig. 5.

In the widest sense, the dynamo-electric machine may be defined as an apparatus for converting mechanical energy into the energy of electro-static charge,

or mechanical power into its equivalent electric current through a conductor. Under this definition would be included the electrophorus and all frictional machines; but the term is used, in a more restricted sense, for those machines which produce electric currents by the motion of conductors in a magnetic field, or by the motion of a magnetic field in the neighborhood of a conductor. The laws on which the action of such machines is based have been the subject of a series of discoveries. Oersted discovered that an electric current in a conductor exerted

Fig. 5



force upon a magnet; Ampère that two conductors conveying currents generally exerted a mechanical force upon each other: Faraday discovered—what Helmholtz and Thomson subsequently proved to be the necessary consequence of the mechanical reactions between conductors conveying currents and magnets—that if a closed conductor move in a magnetic field, there will be a current induced in that conductor in one direction, if the number of lines of magnetic force passing through the conductor was increased by the movement; in the other direction if diminished. Now all dynamo-electric machines are based upon Faraday's discovery. Not only so; but however elaborate we may wish to make the analysis of the action of a dynamo-machine, Faraday's way of presenting the phenomena of electro-magnetism to the mind is in general our best point of departure. The dynamo-machine, then, essentially consists of a conductor made to move in a magnetic field. This conductor, with the external circuit, forms a closed circuit in which electric currents are induced as the number of lines of magnetic force passing through the closed circuit varies. Since, then, if the current in a closed circuit be in one direction when the number of lines of force is increasing, and in the opposite direction when they are diminishing, it is clear that the current in each part of such circuit which passes through the magnetic field must be alternating in direction, unless indeed the circuit be such that it is continually cutting more and more lines of force, always in the same direction. Since the current in the wire of the machine is alternating, so also must be the current outside the machine, unless something in the nature of a commutator be employed to reverse the connections of the internal wires in which the current is induced, and of the external circuit. We have then broadly two classes of dynamo-electric machines; the simplest, the alternating current machine, where no commutator is used; and the continuous-current machine, in which a commutator is used to change the connection of the external circuit just at the moment when the direction of the current would change. The mathematical theory of the alternate-current machine is comparatively simple. To fix ideas, I will ask you to think of the alternate-current Siemens machine, which Dr. Siemens exhibited here three weeks ago. We have there a series of magnetic fields of alternate polarity, and through these fields we have coils of wire moving; these coils constitute what is called the armature; in them are induced the currents which give a useful effect outside the machine. Now, I am going, to trouble you to go through the mathematical equations, simple though they are, by which the following formulæ are obtained:

$$I = A \sin \frac{2\pi t}{T} \quad \text{(I.)}$$

$$E=\frac{2\pi A}{T}\cos\frac{2\pi t}{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

$$x=\frac{2\pi A}{T}\frac{\cos 2\pi\frac{t-\tau}{T}}{\sqrt{\left(\frac{2\pi\gamma}{T}\right)^2+R^2}} \quad . \quad . \quad . \quad (III.)$$

$$\operatorname{Tan}\frac{2\pi\tau}{T}=\frac{2\pi\gamma}{RT} \quad . \quad . \quad . \quad . \quad . \quad . \quad (IV.)$$

$$\varphi R\frac{2\pi^2A^2}{T^2}\frac{I}{\left(\frac{2\pi\gamma}{T}\right)^2+R^2} \quad . \quad . \quad . \quad (V.)$$

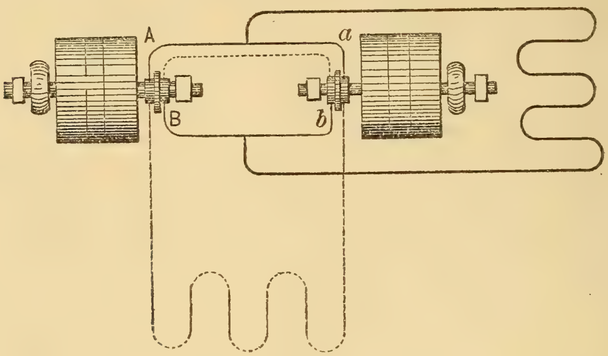
$$R=\frac{2\pi\gamma}{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (VI.)$$

T represents the periodic time of the machine; that is, in the case of a Siemens machine having eight magnets on each side of the armature, T represents the time of one-fourth of a revolution. I represents the number of lines of force embraced by the coils of the armature at the time t. I must be a periodic function of t, in the simplest form represented by Equation I. Equation II. gives E the electromotive force acting at time t upon the circuit. Having given the electromotive force acting at any time, it would appear at first sight that we had nothing to do but to divide that electromotive force by the resistance R of the whole circuit, to obtain the current flowing at that time. But if we were to do so we should be landed in error, for the conducting circuit has other properties besides resistance. I pointed out to you that it had a property of momentum represented by its coefficient of self-induction called γ in the formula; and when we are dealing with rapid changes of current, it plays as important a part as the resistance. Formula III. gives the current x, flowing at any time, and you will observe that it shows two things: first the maximum current is less than it would be if there were no self-induction; secondly, it attains its maximum at a later time. This retardation is represented by the letter τ, and its amount is determined by the Formula IV. At a given speed of rotation, the amount of electrical work developed in the machine in any time φ is given by Formula V. It is greatest when $R=\frac{2\pi\gamma}{T}$. From these formulæ we see that the current is diminished either by increasing γ or increasing R; also the moment of reversal of current is not co-incident with the moment of reversal of electromotive force, but occurs later. by an amount depending on the relative magnitudes of γ and R. They show us that although by doubling the velocity of the machine we really double the electromotive force at any time, we do not double the current passing, nor the work done by the machine; but we may see that if we double the velocity of the machine, we may work through double the external resistance, and still obtain the same current. In what precedes, it has been assumed that the copper wires are the only conducting bodies moving in the magnetic field. In many cases the moving wire-coils of these machines have iron cores, the iron being in some cases solid, in others more or less divided. It is found that if such machines are run on open circuit, that is, so that no current circulates in the armatures; the iron becomes hot, very much hotter, than when the circuit of the copper wire is closed. In some cases this phenomenon is so marked that the machine actually takes more to drive it, when the machine is on open circuit, than when it is short-circuited. The explanation is that on open circuit currents are induced in the iron cores, but that when the copper coils are closed, the current in them diminishes by induction the current in the iron. The effect of currents in the iron cores is not alone to waste energy and heat the machine; but for a given intensity of field and speed of revolution, the external current produced is diminished. The cure of the evil is to subdivide the moving iron as much as possible, in directions perpendicular to those in which the current tends to circulate.

There remains one point of great practical interest in connection with alternate-current machines. How will they behave when two or more are coupled together, to aid each other in doing the same work? With galvanic batteries, we know very well how to couple them, either in parallel circuit or in series, so that they shall aid, and not oppose, the effects of each other; but with alternate current machines, independently driven, it is not quite obvious what the result will be, for the polarity of each machine

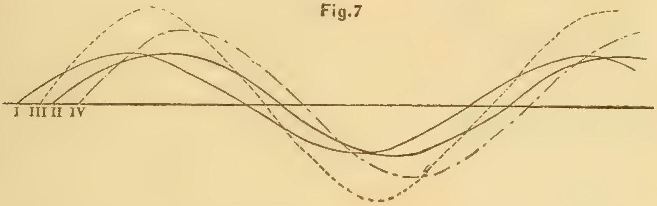
is constantly changing. Will two machines, coupled together, run independently of each other, or will one control the movement of the other in such wise that they settle down to conspire to produce the same effect, or will it be into mutual opposition? It is obvious that a great deal turns upon the answer to this question, for in the general distribution of electric light, it will be desirable to be able to supply the system of conductors from which the consumers draw by separate machines, which can be thrown in and out at pleasure. Now I know it is a common impression that alternate-current machines cannot be worked together, and that it is almost a necessity to have one enormous machine to supply all the consumers drawing from one system of conductors. Let us see how the matter stands. Consider two machines independently driven, so as to have approximately the same periodic time and the same electromotive force. If these two machines are to be worked together, they may be connected in one of two ways; they may be in parallel circuit with regard to the external conductor, as shown by the full line in Fig. 6, that is, their currents may be added algebraically and sent to the external circuit, or they may be coupled in series, as shown by the dotted line, that is, the whole current may pass successively through the two machines, and the electromotive force of the two machines may be added, instead of their currents. The latter case is simpler. Let us consider it first. I am going to show that if you couple two such alternate current machines in series, they will so control each other's phase as to nulify each other, and that you will get no effect from them; and, as a corollary from that, I am going to show that if you couple them in parallel circuit, they will work perfectly well together, and the currents they produce will be added; in fact, that you cannot drive alternate-current machines tandem, but that you may drive them as a pair, or, indeed, any number abreast. In diagram, Fig. 7, the horizontal line of abscissæ represents the time advancing from left to right; the full curves represent the electromotive forces of the two machines not supposed to be in the same phase. We want to see whether they will tend to get into the same phase or to get into opposite phases. Now, if the machines are coupled in series, the resultant electromotive force on the circuit will be the sum of the electromotive forces of the two machines. This resultant electromotive force is represented by the broken curve III; by what we have already seen in Formula IV., the phase of the current must lag behind the phase of the electromotive force, as is shown in the diagram by curve IV., thus——.——.——. Now, the work done in any machine is represented by the sum of the products of the currents and of the electromotive forces, and it is clear that as the phase of the current is more near to the phase of the lagging machine II than to that of the leading machine I, the lagging machine must do more work in producing electricity than the leading machine; consequently its velocity will be retarded, and its retardation will go on until the two

Fig. 6



machines settle down into exactly opposite phases, when no current will pass; The moral, therefore, is, do not attempt to couple two independently driven alternate-current machines in series. Now for the corollary, AB, Fig. 6, represent the two terminals of an alternate-current machine; *ab* the two terminals of another machine independently driven. A and *a* are connected together, and B and *b*. So regarded, the two machines are in series, and we have just proved that they will exactly oppose each other's effects, that is, when A is positive, *a* will be positive also; when A is negative, *a* is also negative. Now, connecting A and *a* through the comparatively high resistance of the external circuit with B and *b*, the current passing through that circuit will not much disturb, if at all, the relations of the two machines. Hence, when A is positive, *a* will be positive, and when A is negative, *a* will be negative also; precisely the condition required that the two machines may work together to send a current into the external circuit. You may, therefore, with confidence, attempt to run alternate-current machines in parallel circuit for the purpose of producing any external effect. I might easily show that the same applies to a larger number: hence, there is no more difficulty in feeding a system of conductors from a number of alternate-current machines, than there is in feeding it from a number of continuous-current machines. A little care only is required that the machine shall be thrown in when it has attained something like its proper velocity. A further corollary is that alternate currents with alternate current machines as motors may theoretically be used for the transmission of power.*

Fig. 7

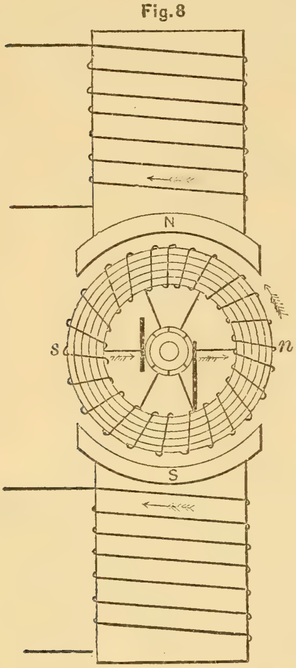


It is easy to see that, by introducing a commutator revolving with the armature, in an alternate-current machine, and so arranged as to reverse the connection between the armature and the external circuit just at the time when the current would reverse, it is possible to obtain a current always constant in direction; but such a current would be far from constant in intensity, and would certainly not accomplish all the results that are obtained in modern continuous-current machines. This irregularity may, however, be reduced to any extent by multiplying the wires of the armature, giving each its own connection to the outer circuit, and so placing them that the electromotive force attains a maximum successively in the several coils. A practically uniform electric current was first commercially produced with the ring armature of Pacinotti, as perfected by Gramme. The Gramme machine is represented diagrammatically in Fig. 8. The armature consists of an anchor ring of iron wire, the strands more or less insulated from each other. Round this anchor ring is wound a continuous endless coil of copper wire; the armature moves in a magnetic field, produced by permanent or electro-magnets with diametrically opposite poles, marked N and S. The lines of magnetic force may be regarded as passing into the ring from N, dividing, passing round the ring and across to S. Thus the coils of wire, both near to N and near to S, are cutting through a very strong magnetic field; consequently there will be an intense inductive action; the inductive action of the coils near N being equal and opposite to the inductive action of the coils near S, it results that there will be strong positive and negative electric potential at the extremities of a diameter perpendicular to the line N S. The electromotive force produced, is made

* Of course in applying these conclusions it is necessary to remember that the machines only *tend* to control each other, and that the control of the motive power may be predominant, and *compel* the two or more machines to run at different speeds.

use of to produce a current external to the machine thus, the endless coil of the armature is divided into any number of sections, in the diagram into six for convenience, usually into sixty or eighty, and the junction of each pair of sections is connected by a wire to a plate of the commutator fixed upon the shaft which carries the armature; collecting brushes make contact with the commutator as shown in the diagram. If the external resistance were enormously high, so that very little current, or none at all passed

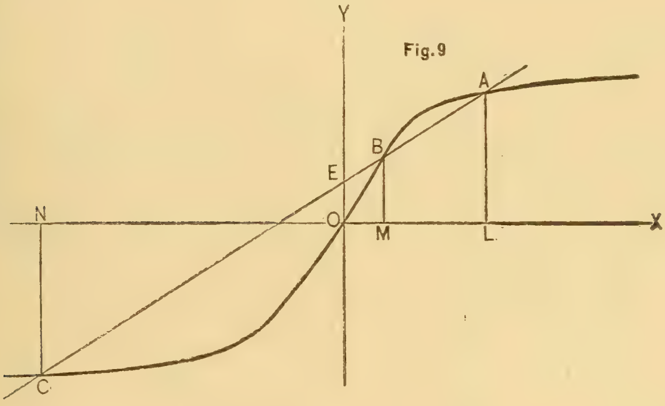
Fig. 8



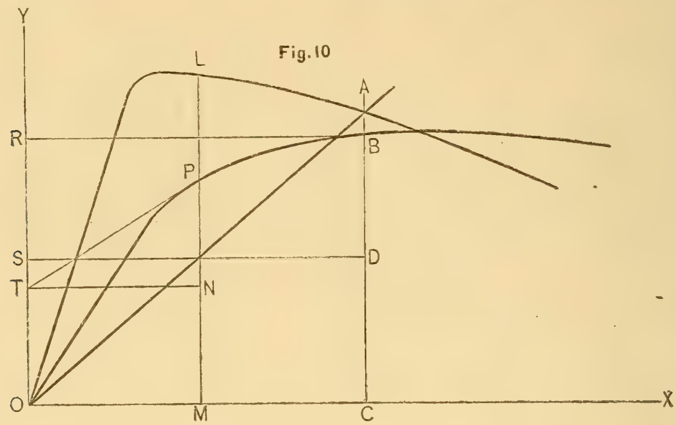
through the armature, the greatest difference of potential between the two brushes would be found when they made contact at points at right angles to the line between the magnets; but when a current passes in the armature, this current causes a disturbing effect upon the magnetic field. Every time the contact of the brushes changes from one contact-plate to the next, the current in a section of the copper coil is reversed, and this reversal has an inductive effect upon all the other coils of the armature. You may take it from me that the net result on any one coil is approximately the same as if that coil alone were moved, and all the other coils were fixed, and there were no reversals of current in them. Now you can easily see that the magnetic effect of the current circulating in the coils of the armature, will be to produce a north pole at *n*, and a south pole at *s*. This will displace the magnetic field in the direction of rotation. If, then, we were to keep the contact points the same as when no current was passing, we should short circuit the sections of the armature at a time when they were cutting through the lines of magnetic force, with a result that there would be vigorous sparks between the collecting brushes and the commutator. To avoid this, the brushes must follow the magnetic field, and also be displaced in the direction of rotation, this displacement being greater as the current in the armature is greater in proportion to the magnetic field. The net effect of this disturbing effect of the current in the armature reacting upon itself is then to displace the neutral points upon the commutator, and consequently somewhat to diminish the effective electromotive force. It is best to adjust the brushes to make contact at a point such that, with the current then passing, flashing is reduced to a minimum, but this point does not necessarily coincide with the point which gives maximum difference of potential. The magnetic field in the Gramme and other continuous dynamo-electric machines, may be produced in several ways. Permanent magnets of steel may be used, as in some of the smaller machines now made, and in all the earlier machines; these are frequently called magneto-machines. Electro-magnets excited by a current from a small dynamo-electric machine, were introduced by Wilde; these may be described shortly as dynamos with separate exciters. The plan of using the whole current from the armature of the machine itself, for exciting the magnets, was proposed almost simultaneously by Siemens, Wheatstone and S. A. Varley. A dynamo, so excited, is now called a series dynamo. Another method is to divide the current from the armature, sending the greater part into the external circuit, and a smaller portion through the electro-magnet, which is then of very much higher resistance; such an arrang ment is called a shunt dynamo. A combination of the

two last methods has been recently introduced, for the purpose of maintaining constant potential. The magnet is partly excited by a circuit of high resistance, a shunt to the external circuit, and partly by coils conveying the whole current from the armature. All but the first two arrangements named depend on residual magnetism to initiate the current, and below a certain speed of rotation give no practically useful electromotive force. A dynamo machine is, of course, not a perfect instrument for converting mechanical energy into the energy of electric current. Certain losses inevitably occur. There is, of course, the loss due to friction of bearings, and of the collecting brushes upon the commutator; there is also the loss due to the production of electric currents in the iron of the machine. When these are accounted for, we have the actual electrical effect of the machine in the conducting wire; but all of this is not available for external work. The current has to circulate through the armature, which inevitably has electrical resistance; electrical energy must, therefore, be converted into heat in the armature of the machine. Energy must also be expended in the wire of the electro-magnet which produces the field, for the resistance of this also cannot be reduced beyond a certain limit. The loss by the resistance of the wires of the armature and of the magnets greatly depends on the dimensions of the machine. About this I shall have to say a word or two presently. To know the properties of any machine thoroughly it is not enough to know its efficiency and the amount of work it is capable of doing; we need to know what it will do under all circumstances of varying resistance or varying electromotive force. We must know, under any given conditions, what will be the electromotive force of the armature. Now, this electromotive force depends on the intensity of the magnetic field, and the intensity of the magnetic field depends on the current passing around the electro-magnet and the current in the armature. The current then in the machine is the proper independent variable in terms of which to express the electromotive force. The simplest case is that of the series dynamo, in which the current in the electro-magnet and in the armature is the same, for then we have only one independent variable. The relation between the electromotive force and current is represented by such a curve as is shown in the diagram, Fig. 9. The abscissæ, measured along OX, represent the current, and the ordinates represent the electromotive force in the armiture. When four years ago I first used this curve, for the purpose of expressing the results of my experiments on the Siemens dynamo machine, I pointed out that it was capable of solving almost any problem relating to a particular machine, and that it was also capable of giving good indications of the results of changes in the winding of the magnets, or of the armatures of such machines. Since then M. Marcel Deprez has happily named such curves "characteristic curves." I will give you one or two illustrations of their use. A complete characteristic of a series dynamo

Fig. 9



does not terminate at the origin, but has a negative branch, as shown in the diagram; for it is clear that by reversing the current through the whole machine, the electromotive force is also reversed. Suppose a series dynamo is used for charging an accumulator, and is driven at a given speed, what current will pass through it? The problem is easily solved. Along OY, Fig. 9, set off OE to represent the electromotive force of the accumulator, and through E draw the line CEBA, making an angle with OX, such that its tangent is equal to the resistance of the whole circuit, and cutting the characteristic curve as it in general will do, in three points A, B and C. We have then three answers to the question.

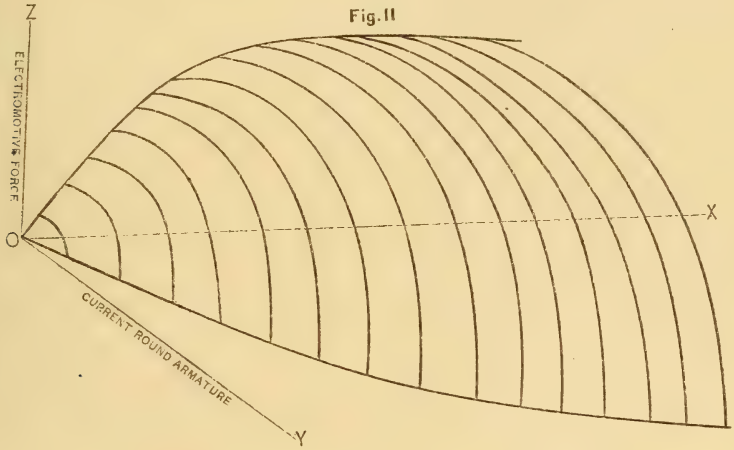


The current passing through the dynamo will be either OL, OM, or ON the abscissæ of the points where the line cuts the curve. OL represents the current when the dynamo is actually charging the accumulator. OM represents a current which could exist for an instant, but which would be unstable, for the least variation would tend to increase. ON is the current which passes, if the current in the dynamo should get reversed, as it is very apt to do when used for this purpose. The next illustration is rather outside my subject, but shows another method of using the characteristic curve. Many of you have heard of Jacobi's law of maximum effect of transmitting work by dynamo machines. It is this: Supposing that the two dynamo machines were perfect instruments for converting mechanical energy into electrical energy, and that the generating machine were run at constant velocity, whilst the receiving machine had a variable velocity, the greatest amount of work would be developed in the receiving machine when its electromotive force was one-half that of the generating machine, then the efficiency would be one-half, and the electrical work done by the generating machine would be just one-half of what it would be if the receiving machine were forcibly held at rest. Now this law is strictly true if, and only if, the electromotive force of the generating machine is independent of the current passing through its armature. What I am now going to do is to give you a construction for determining the maximum work which can be transmitted when the electromotive force of the generating machine depends on the current passing through the armature, as, indeed, it nearly always does, referring to Fig. 10. OPB is the characteristic curve of the generating machine; construct a derived curve thus, at successive points P of the characteristic curve, draw tangents PT, draw TN parallel to OX, cutting PM in N, produce MP to L, making LP equal PN; the point L gives the derived curve, which I want. Now, to find the maximum work which can be transmitted, draw OA at such angle with OX that its tangent is equal to twice the resistance of the whole circuit, cutting the derived curve in A. Draw the ordinate AC, cutting the characteristic curve in B; bisect AC at D. The work expended upon the generating machine would be represented

by the parallelogram OCBR, the work wasted in resistance by OCDS, and the work developed in the receiving machine by the parallelogram SDBR.

When the dynamo-machine is not a series dynamo, but the currents in the armature and in the electro-magnet, though possibly dependent upon each other are not necessarily equal, the problem is not quite so simple. We have, then, two variables, the current in the electro-magnet and the current in the armature; and the proper representation of the properties of the machine will be by a characteristic surface such as that illustrated by this model, Fig. 11. Of the three co-ordinate axes, OX represents the current in the magnet; OY represents the current in the armature not necessarily to the same scale, and OZ the electromotive force. By the aid of such a surface as this one may deal with any problem relating to a dynamo-machine, no matter how its electro-magnets and its armature are connected together. Let us apply the model to find the characteristic of a series dynamo. Take a plane through OZ, the axis of electromotive force, and making such an angle with the plane OX, OZ, that its tangent is equal to current unity on axis OY, divided by current unity on axis OX. This plane cuts the surface in a curve. The projection of this curve on the plane OX, OZ is the characteristic curve of the series dynamo. This model only shows an eighth part of the complete surface. If any of you should interest yourselves about the other seven parts, which are not without interest, remember that it is assumed that the brushes always make contact with the commutator at the point of no flashing, if there is one. Of course in actual practice one would not use the model of the surface, but the projections of its sections. While I am speaking of characteristic curves there is one point I will just take this opportunity of mentioning. Three years ago, Mr. Shoolbred exhibited the characteristic curve of a Gramme machine, in which, after the current attained to a certain amount, the electromotive force began to fall. I then said that I thought there must be some mistake in the experiment. However, subsequent experiments have verified the fact; and when one considers it, it is not very difficult to see the explanation. It lies in this: after the current attains to a certain amount the iron in the machines becomes magnetically nearly saturated, and consequently an increase in the current does not produce a corresponding increase in the magnetic field. The reaction, however, between the different sections of the wire on the armature goes on increasing indefinitely, and its effect is to diminish the electromotive force.

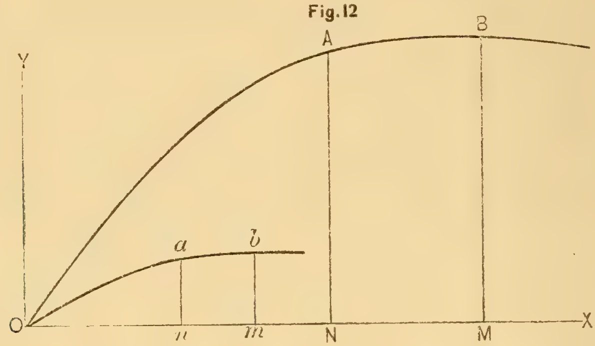
Fig. 11



A little while ago I said that the dimensions of the machine had a good deal to do with its efficiency. Let us see how the properties of a machine depend upon its dimensions. Suppose two machines alike in every particular excepting that the one has all its linear dimensions

double that of the other; obviously enough all the surfaces in the larger would be four times the corresponding surfaces in the smaller, and the weights and volumes of the larger would be eight times the corresponding weights in the smaller machines. The electrical resistances in the larger machine would be one-half of the smaller. The current required to produce a given intensity of magnetic field would be twice as great in the larger machine as in the smaller. In the diagram, Fig. 12, are shown the comparative characteristic curves of the two machines, when driven at the same speed. You will observe that the two curves are one the projection of the other, having corresponding points with abscissæ in the ratio of one to two, and the ordinates in can carry in the armature is limited by the rate at which we can get rid of the heat generated in the armature. This we may consider as proportional to its surface, consequently we must only waste four times as much energy in the armature of the larger machine as in the smaller one, instead of eight times, as would be the case if we carried the current in proportion to the section of the wire. Again, the larger machine cannot run at so great an angular velocity as the smaller one. And lastly, since in the larger machine the current in the armature is greater in proportion to the saturated magnetic field than it is in the small one, the displacement of the point of contact of the brushes with the commutator will be greater. However, to cut

Fig. 12



the ratio of one to four. Now at first sight it would seem as though, since the wire on the magnet and armature of the larger machine has four times the section of that of the smaller, that four times the current could be carried, that consequently the intensity of the magnetic field would be twice as great, and its area would be four times as great, and hence the electromotive force eight times as great; and since the current in the armature also is supposed to be four times as great, that the work done by the larger machine would be thirty-two times as much as that which would be done by the smaller. Practically, however, no such result can possibly be attained, for a whole series of reasons. First of all, the iron of the magnets becomes saturated, and consequently instead of getting eight times the electromotive force, we should only get four times the electromotive force. Secondly, the current which we the matter, about which one might say a great deal, short, one may say that the capacity of similar dynamo-machines is pretty much proportionate to their weight, that is, to the cube of their linear dimensions; that the work wasted in producing the magnetic field will be directly as the linear dimensions; and that the work wasted in heating the wires of the armature will be as the square of the linear dimensions. Now let us see how this would practically apply. Suppose we had a small machine capable of producing an electric current of 4 HP., that of this 4 HP. 1 was wasted in heating the wires of the armature, and 1 in heating the wires of the magnet, 2 would be usefully applied outside. Now, if we doubled the linear dimensions we should have a capacity of 32 HP., of which 2 only, if suitably applied, would be required to produce the magnetic field and 4 would be wasted in heating the wires of the

armature, leaving 26 HP. available for useful work outside the machine—a very different economy from that of the smaller machines. But if we again doubled the linear dimensions of our machine, we should by no means obtain a similar increase of effect. A consideration of the properties of similar machines has another very important practical use. As you all know, Mr. Froude was able to control the design of ironclad ships by experiments upon models made in paraffin wax. Now, it is a very much easier thing to predict what the performance of a large dynamo-machine will be, from laboratory experiments made upon a model of a very small fraction of its dimensions. As a proof of the practical utility of such methods, I may say that by laboratory experiments I have succeeded in increasing the capacity of the Edison machines without increasing their cost, and with a small increase of their percentage of efficiency, remarkably high as that efficiency already was.

I might occupy your time with considerations as to the proper proportion of conductors, and explain Sir W. Thomson's law, that the most economical size of a copper conductor is such that the annual charge for interest and depreciation of the copper of which it is made, shall be equal to the cost of producing the power which is wasted by its resistance. But the remaining time will, perhaps, be best spent in considering the production of light from the energy of electric currents. You all know that this is done commercially in two ways, by the electric arc, and by the incandescent lamp; as the arc lamp preceded the incandescent lamp historically, we will examine one or two points connected with it first.

I have here all that is necessary to illustrate the electric arc, viz., two rods of carbon supported in line with each other, and so mounted that they can be approached or withdrawn. Each carbon is connected with one of the poles of the Edison dynamo-machine, which is supplying electricity to the incandescent lamps which illuminate the whole of this building. A resistance is interposed in the circuit of the lamp, because the electromotive force of the machine is much in excess of what the lamp requires. I now approach the carbons, bring them into contact, and again separate them slightly; you observe that the break does not stop the current which forces its way across the space. I increase the distance between the carbons, and you observe the electric arc between their extremities; at last it breaks, having attained a length of about one inch. Now the current has hard work to cross this air-space between the carbons, and the energy there developed is converted into heat, which raises the temperature of the ends of the carbon beyond any other terrestrial temperature. There are several points of interest I wish to notice in the electric arc. Both carbons burn away in the air, but there is also a transference of carbon from the positive to the negative carbon; therefore, although both waste away, the positive carbon wastes about twice as fast as the negative. With a continuous current, such as we are using now, the negative carbon becomes pointed, whilst the positive carbon forms a crater or hollow; it is this crater which becomes most intensely hot and radiates most of the light, hence the light is not by any means uniformly distributed in all directions, but is mainly thrown forward from the crater in the positive carbon. This peculiarity is of great advantage for some purposes, such, for example, as military or naval search lights, but it necessitates, in describing the illuminating power of an arc light, some statement of the direction in which the measurement was made. On account of its very high temperature the arc light sends forth a very large amount of visible radiation, and is therefore very economical of electrical energy. For the same reason its light contains a very large proportion of rays of high refrangibility, blue and ultra-violet. I have measured the red light of an electric arc against the red of a candle, and have found it to be 4,700 times as great, and I have measured the blue of the same arc light against the blue of the same candle, and found it to 11,380 times as great. The properties of an electric arc are not those of an ordinary conductor. Ohm's law does not apply. The electromotive force and the current do not by any means bear to each other a constant ratio. Strictly speaking, an electric arc cannot be said to have an electric resistance measureable in ohms. We will now examine the

electrical properties of the arc experimentally. In the circuit with the lamp is a Thomson graded current galvanometer for measuring the current passing in amperes; connected to the two carbons is a Thomson graded potential galvanometer, for measuring the difference of potential between them in volts. We have the means of varying the current by varying the resistance, which I have already told you is introduced into the circuit. We will first put in circuit the whole resistance available, and will adjust the carbons so that the distance between them is, so near as I can judge, $\frac{1}{8}$ inch. We will afterwards increase the current, and repeat the readings. The results are given in the following table:

Current galvanometer.	Potential galvanometer.	Ampères	Volts.	Watts.	HP.
6.2	12.0	9.9	35	346	0.46
9.3	12.0	14.9	35	521	0.70
11 5	11.8	18.4	34	626	0.84

If the electrical properties of the arc were the same as those of a continuous conductor, the volts would be in proportion to the amperes, if correction were made for change of temperature; you observe that instead of that the potential is nearly the same in the two cases. We may say, with some approach to accuracy, that with a given length of arc the arc opposes to the current an electromotive force nearly constant, almost independent of the current. This was first pointed out by Edlund. If you will speak of the resistance of the electric arc, you may say that the resistance varies inversely as the current. Take the last experiment: by burning 4 cubic feet of gas per hour we should produce heat-energy at about the same rate. I leave any of you to judge of the comparative illuminating effects. It is not my purpose to describe the mechanisms which have been invented for controlling the feeding of the carbons as they waste away. Several lamps lent by Messrs. Siemens Brothers—to whom I am indebted for the lamp and resistance I have just been using — lie upon the table for inspection. An electric arc can also be produced by an alternate current. Its theory may be treated mathematically, and is very interesting, but time will not allow us to go into it. I will merely point out this: there is some theoretical reason to suppose that an alternate-current arc is in some measure less efficient than one produced by a continuous current. The efficiency of a source of light is greater, as the mean temperature of the radiating surface is greater. The maximum temperature in an arc is limited probably by the temperature of volatilization of carbon; in an alternate-current arc the current is not constant, therefore the mean temperature is less than the maximum temperature; in a continuous-current arc, the current being constant, the mean and maximum temperatures are equal, therefore in a continuous-current arc the mean temperature is likely to be somewhat higher than in an alternate-current arc.

We will now pass to the simpler incandescent light. When a current of electricity passes through a continuous conductor, it encounters resistance, and heat is generated, as was shown by Joule, at a rate represented by the resistance multiplied by the square of the current. If the current is sufficiently great the heat will be generated at such a rate that the conductor rises in temperature so far that it becomes incandescent and radiates light. Attempts have been made to use platinum and platinum-iridium as the incandescent conductor, but these bodies are too expensive for general use, and besides, refractory though they are, they are not refractory enough to stand the high temperature required for economical incandescent lighting. Commercial success was not realized until very thin and very uniform threads or filaments of carbon were produced and enclosed in reservoirs of glass, from which the air was exhausted to the utmost possible limit. Such are the lamps made by Mr. Edison with which this building is lighted to-night. Let us examine the electrical properties of such a lamp. Here is a lamp intended to carry the same current as those overhead, but of half the resistance, selected because it leaves us a margin of electromotive force wherewith to vary our experiment. Into its circuit I am able to introduce a resistance for checking the current, composed of other incandescent lamps for convenience, but which I shall cover over that they may

not distract your attention. As before, we have two galvanometers, one to measure the current passing through the lamp, the other the difference of potential at its terminals. First of all we will introduce a considerable resistance; you observe that, although the lamp gives some light, it is feeble and red, indicating a low temperature. We take our galvanometer readings. We now diminish the resistance, the lamp is now a little short of its standard intensity; with this current it would last 1000 hours without giving way. We again read the galvanometers. The resistance is diminished still further. You observe a great increase of brightness, and the light is much whiter than before. With this current the lamp would not last very long. The results are given in the following table:

Current galvano-meter.	Potential galvano-meter.	Am-pères.	Volts.	Watts	Resist-ance ohms.
5.2	12.8	0.38	37	14	97
6.0	14.3	0.44	41	18	93
11.5	23.4	0 84	68	57	81

There are three things I want you to notice in these experiments: first, the light is whiter as the current increases; second, the quantity of light increases very much faster than the power expended increases; and thirdly, the resistance of the carbon filament diminishes as its temperature increases, which is just the opposite of what we should find with a metallic conducter. This resistance is given in ohms in the last column. To the second point, which has been very clearly put by Dr. Siemens in his British Association address, I shall return in a minute or two.

The building is this evening lighted by about 200 lamps, each giving sixteen candles' light, when 75 watts of power are developed in the lamp. To produce the same sixteen candles' light in ordinary flat-flame gas-burners, would require between seven and eight cubic feet of gas per hour, contributing heat to the atmosphere at the rate of 3,400,000 foot-pounds per hour, equivalent to 1250 watts, that is to say, equivalent gas lighting would heat the air nearly seventeen times as much as the incandescent lamps.

Look at it another way. Practically, about eight of these lamps take one indicated horse-power in the engine to supply them. If the steam engine were replaced by a large gas engine this 1 HP. would be supplied by 25 cubic feet of gas per hour, or by rather less; therefore by burning gas in a gas engine driving a dynamo, and using the electricity in the ordinary way in incandescent lamps, we can obtain more than 5 candles per cubic foot of gas, a result you would be puzzled to obtain in 16-candle gas burners. With arc lights instead of incandescent lamps many times as much light could be obtained.

At the present time, lighting by electricity in London must cost something more than lighting by gas. Let us see what are the prospects of reduction of this cost. Beginning with the engine and boiler, the electrician has no right to look forward to any marked and exceptional advance in their economy. Next comes the dynamo. the best of these are so good, converting 80 per cent. of the work done in driving the machine into electrical work outside the machine, that there is little room for economy in the conversion of mechanical into electrical energy; but the prime cost of the dynamo-machine is sure to be greatly reduced. Our hope of greatly increased economy must be mainly based upon probable improvements in the incandescent lamp, and to this the greatest attention ought to be directed. You have seen that a great economy of power can be obtained by working the lamps at high-pressure, but then they soon break down. In ordinary practice, from 140 to 200 candles are obtained from a horse power developed in the lamps, but for a short time I have seen over 1000 candles per horse-power from incandescent lamps. The problem, then, is so to improve the lamp in details, that it will last a reasonable time when pressed to that degree of efficiency. There is no theoretical bar to such improvements, and it must be remembered that incandescent lamps have only been articles of commerce for about three years, and already much has been done. If such an improvement were realized, it would mean that you would get five times as much light for a sovereign, as you can now. As things now stand, so soon as those who supply elec-

tricity have reasonable facilities for reaching their customers, electric lighting will succeed commercially where other considerations than cost have weight. We are sure of some considerable improvement in the lamps, and there is a probability that these improvements may go so far as to reduce the cost to one-fifth of what it now is. I leave you to judge whether or not it is probable, nay, almost certain, that lighting by electricity is the lighting of the future.

WATER SUPPLY OF SAN FRANCISCO FROM LAKE MERCED.

By P. J. FLYNN, Civil Engineer.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE following is a brief report submitted on the water supply of San Francisco, California, from Lake Merced. Lake Merced is situated about six miles in a south-westerly direction from a central point the new City Hall of San Francisco, and it is within about a quarter of a mile of the Pacific coast. Part of the lake is within the municipal boundary of San Francisco.

Omitting all consideration of cost, the points requiring investigation are:

1st. The *quantity* of water available from the lake to supply the city.

2d. The *quality* of the water for domestic purposes.

In order to make a satisfactory report the flow from the outlet to the lake should be gauged daily for several years, *including a dry season*, the rainfall noted for the same time and the water analyzed frequently—about once a week—for the space of one year. I fail to find that this has been done in the case of Lake Merced. I, however, avail myself of the information contained in the reports of the engineers heretofore employed to report on the water supply of the city.

As being of little use, for the purpose of this report, I omit a detailed description of the lake and its surrounding watershed.

The area of the lake is given as 331 acres, and the area of its watershed seven and one-half square miles. The water supply of the lake is derived from the rainfall. The watershed, composed chiefly of sand, acts as an immense storage reservoir and the uniformity of flow from it to the lake is such that works of an inexpensive nature will prevent any loss by waste from the lake should it be used as a service reservoir. According to the gaugings then made, I find that the watershed stored in 1874 a supply sufficient to fill a reservoir one mile long, 1,041 feet wide and 50 feet deep.

The only supply that there is any proof of, is rainfall, and this alone will be estimated for in this report. Engineers differ in opinion as to whether there is an artesian supply in addition to the rainfall, but, as *proof is wanting* that such a supply exists, the safest plan is to make no allowance for it. If such a supply exists it must be small, as Mr. Schussler at one time found the flow from the lake to be only 1,500,000 gallons per day.

The gaugings taken at the outlet show considerable variation in the flow from the lake: Mr. Scowden gauged 5,680,434 gallons per day; Mr. Allardt, 5,500,000 gallons; Messrs. Schussler and Elliott, 2,500,000 gallons, and 1,750,000 gallons; Colonel Von Schmidt, 10,000,000 gallons; Mr. Schussler, 1,500,000 gallons, and 55,000,000 gallons.

Mr. Scowden gives the area of the watershed as $7\frac{1}{2}$ square miles, and he estimates the *average* of 25 years rainfall as 23.9 inches per annum, the same as San Francisco. The total average daily rainfall will therefore be equal to 8,534,630 gallons per day. In no case, however, does all the rain that falls on a watershed flow off. A certain portion is lost in evaporation and infiltration. Even a steep surface of granite does not shed all the rainfall. After gauging the outflow of the lake, Mr. Scowden estimated that 66 per cent. of a total rainfall of 23.9 inches per annum could be utilized. The loss, therefore, was 34 per cent., equal to 8.12 inches rainfall. This I consider a fair estimate, and I adopt it. A yearly rainfall of 23.9 inches gives an

average daily supply of 8,534,630 gallons, and 66 per cent. of this amounts to 5,632,855 gallons per day available for a water supply.

Mr. Scowden, in his estimates, adopted a supply of 100 gallons of water per individual per day, and Colonel Mendell adopted 50 gallons as a *minimum*. No doubt 30 gallons is an ample supply for all purposes if the water is used carefully, but the experience of almost all cities in Europe and America, having a constant supply of water, is that more is allowed run to waste than is actually used, and no means have yet been found to effectually check this waste. New York was sometime since expending at the rate of over 100 gallons per individual per day. For this report I adopt Colonel Mendell's *minimum* allowance of 50 gallons per day. At this rate 5,632,855 gallons per day will suffice for a population of 112,657 only, being probably not more than one-third of the population of the city at the present time.

As the strength of a bridge is measured by its weakest part, so is the utility of a watershed measured by its driest season, except where there is a storage reservoir of sufficient capacity to retain enough of water to compensate for loss during a dry season.

A small rainfall does not, especially in sandy ground, give a supply proportionate to a large rainfall. For instance, a rainfall of seven inches will not give a supply one-fourth of that given by a rainfall of 28 inches. The reason of this is that the quantity lost through evaporation and infiltration may, practically, be taken as *constant*. I will give a familiar illustration. Place a large sponge in an empty vessel and pour a measured quantity of water over it. After it is saturated take it out and press, with your hands, as much of the water out of it and into the vessel as you can do. The vessel has lost say, one-half a pint of water. This loss is a *constant quantity*, for, if you increased the quantity of water poured over the sponge, the quantity retained by it will be the same. Something similar is the case with the water sheds. A *constant quantity* of water is lost, irrespective of the rainfall. From what has been already said it is evident that the constant yearly loss on the Lake Merced watershed is 34 per cent. of 23.9 inches of rainfall, that is, 8.12 inches of rainfall, the available supply being 66 per cent.

Colonel Mendell, in his report, gives a table of the rainfall of San Francisco, as recorded by Mr. Thomas Tennant, from July to July of the years 1849 to 1877 inclusive. In this statement the rainfall is given for 1850–51 equal to 7.4 inches, 1851–52 equal to 18.44 inches, 1862–63 equal to 13.62 inches, 1863–64 equal to 10.08 inches, 1869–70 equal to 19.31 inches, 1870–71 equal to 14.10 inches. We have here on an average, one dry year in about every three. Then again we have four dry years in the space of nine years, and on three occasions two consecutive dry years.

In the following table it will be seen that the water that can be utilized in dry years is sufficient to supply only a small percentage of the present population of the city:

Years.	Rainfall in inches	Available rainfall*	Gallons per day.	Population at 50 gallons per day.
1850–51.	7.40	0.72	—	—
1851–52.	18.44	10.32	3,684,983	73,699
1862–63.	13 62	5.50	1,963,896	39,277
1863–64.	10.08	1.96	699,861	13,997
1869–70.	19.31	11.19	3,995,635	79.912
1870–71.	14.10	5.98	2,135,290	42,706

It is very likely that there was little, if any, flow from the lake in 1850–51, especially during the dry months, as the supply was only 7.4 inches and the *constant loss* 8.12 inches. Then comes another dry year with a supply for a population of 39,277. The above table shows that, taken in the most favorable light, the supply for six out of twenty years falls far short of that required for one-third of the present population. What has happened before in the occurrence of dry years, at intervals, is almost certain to happen again. The only safe course, therefore, is to estimate on the basis of the least known supply. The least known supply of Lake Merced would cause a *complete water famine*. A small watershed like that of Lake Merced is

* The total rainfall less 8.12 inches, the constant loss.

more liable to great variation in its rainfall than a large one.

The following has been published in favor of Lake Merced supply.

"It may be objected that 8,000,000 gallons per day would not be sufficient to supply San Francisco. Admit that fact, and what does it prove? Does it prove that we should reject that much cheap water because we cannot have it all cheap? The city of London is said to be supplied by some eight different companies." The great disadvantages of having so many companies have been long felt by the London public, and a great deal has been written on the subject. Several eminent engineers have reported at length on a single supply to replace all the other supplies. The question took such hold of the public attention some years since, that the British Government appointed a Royal Commission to investigate the subject of water supply. This Commission took evidence for nearly two years, from February, 1867, to December, 1868, the bulk of the evidence, which fills a large volume, being plans and suggestions from engineers and other scientific men, for a single supply for London. Water supply is at the present time receiving a great deal of attention, and it is probable that at no distant day a single supply will be introduced to supply all London.

A growing city like San Francisco demands such a supply as will meet all its present and future requirements, until the city reaches a population of at least a million. The supply should be equal to all the wants of the city, including domestic purposes, manufactories, street sprinkling, irrigation of gardens, and sewer flushing.

The next point for consideration is the *quality* of the water. What is required is a supply that not only is pure now, but that also, so far as is known, is likely to remain pure so long as that supply is required.

Colonel Mendell estimates, from the former growth of the city, that it will have a population of 500,000 in 1887, and 800,000 in 1897. As part of the lake is within the municipal boundary of San Francisco, it is reasonable to believe that, long before the population reaches the latter number, a large portion of the watershed of Lake Merced will be built over and occupied by several thousand inhabitants.

Before sewers are constructed cesspools will be one of the means adopted to provide for the disposal of the sewage. This sewage will pollute the subsoil. At the same time the surface of unpaved streets and thoroughfares will be formed of a mass of accumulated filth. Even after sewers are constructed the leakage from them into the sandy subsoil will contaminate the water supply of the lake. The rain, before reaching the lake, will have to pass through the surface deposit and subsoil. It will, very likely, be said that this water will be filtered by passing through the sand before reaching the lake. In the year 1875 a commission appointed by the British Government, to report on the pollution of rivers, stated with reference to water polluted with sewage:

"The only safe course is to avoid altogether the use, for domestic purposes, of water which has been polluted with excrementitious matters." This is very explicit. The large cemetery which it is proposed to locate within the watershed will be another cause of pollution to the waters of the lake.

As streets and sewers spread over the watershed, the *supply of water* will steadily *decrease*, whilst, at the same time, *sewage pollution* will steadily *increase.* All the rain falling on the inhabited district will not be intercepted from the lake, but the quantity that reaches the lake from it will be polluted.

During heavy rains the surface flow from the inhabited district will be strongly impregnated with sewage, and this can be prevented from reaching the lake only by the construction of costly intercepting sewers or open channels. This will be, however, at the expense of the supply of water to the lake. It will thus be seen that as the watershed is built over the supply of water to the lake will steadily decrease, and pollution increase, and eventually the *watershed will become a sewage shed*, the supply that it will shed to the lake being, in fact, *diluted sewage.* The poisoning of the water of the lake will be cumulative. It may be objected to this, that as there will be a constant flow to and from the

lake, keeping its water always free from stagnation, therefore the sewage can do no material damage. On this subject, and with reference to flowing rivers polluted with sewage, the Rivers Pollution Commissioners above mentioned state:

"All the processes that have yet been offered for the purpose of cleansing such polluted water have proved ineffective to produce a resulting effluent fit for drinking and domestic purposes after such a contamination with sewage or other animal refuse.

Contrast Lake Merced with a flowing river under the above circumstances, and the verdict will not be in its favor. Chicago constructed a tunnel *two miles long under the bed of Lake Michigan* in order to place the inlet for its water supply beyond the reach of the contaminated waters along its margin.

Even if it is admitted that the waters of Lake Merced will remain pure, still the quantity of water, for all useful purposes of water supply to a large city, will eventually fail. In addition to the quantity of water carried off by the sewers a further quantity will be taken up by wells, which will be sunk as the district becomes inhabited, and the supply will gradually diminish, until the quantity of water reaching the lake will scarcely suffice to compensate for the infiltration from the lake and evaporation from its surface. No flow will then take place from the lake, and its water will be almost stagnant.

Long before the land around Lake Merced is fully built over, the most stringent measures will have to be adopted, and costly intercepting sewers constructed, to prevent this becoming a hot-bed of disease. I do not mean from the use of its water, but from the poisonous exhalations and deadly malaria that are likely to arise from it in hot weather, if the surface storm flow of the streets be not prevented from reaching it and converting it into a sewage reservoir.

Lake Merced, as a source of water supply for the city, has in its favor, its location near the city, and consequently, causing a minimum of expense, in cost of works, pumping and repairs.

Its disadvantages are:

1. That its supply is totally inadequate to the present and future wants of the city.

2. That its water, admitting its purity at the present time, can be kept pure only by the prevention of the extension of the city over its watershed.

THE EFFECT ON ARCHITECTURE OF LIABILITY TO EARTHQUAKES.

From "The Builder."

The fatal and destructive earthquake in the Island of Ischia, of the magnitude of which each day brings further accounts, recalls, to those who had any experience of its effects, the yet more fatal catastrophe which desolated Calabria early in 1858. We are writing with personal experience only of the fringe of this terrible earthquake, which is said to have cost the lives of 30,000 Italians. Potenza, in the province of Basilicata, which is called the home and cradle of earthquakes, was the central point of disturbance in 1858, and the speed at which the shock traveled was estimated, after careful inquiry, at 775 ft. per second, or nearly half the velocity of sound in the air. At Naples itself but little damage was done. But this was due rather to the solid excellence of the work of the Italian architects than to the feeble nature of the shocks, which continued through an entire night. The first was the most severe of these shocks, being followed almost immediately by the *replica*, or return shock, which is always the most dreaded part of the disturbance. Some idea of the intensity of the action at Naples may be formed from the following brief account. The writer was seated in a large saloon in a palace in Naples, which had formerly been that of the Spanish viceroys, and which is close on the shore of the bay. About nine o'clock in the evening, when some of the citizens were at the opera, the first shock came, without any premonitory symptoms. Its violence was, to some extent,

to be measured by the amplitude of the oscillations of a large chandelier, depending from the ceiling, which swung through an arc of more than 99°. And as to duration, there was enough time to rise, to cross the room, and to go out on to a terrace overlooking the bay, while the noise and vibration continued. On arriving at the edge of the terrace, the observer had to cling to the railing for support, as the whole building rocked like a ship at sea; and the walls of the lowest story of the palace in question are 17 ft. thick.

The houses of Naples were emptied by the shock, the entire population, in every condition of dress and undress, pouring out into the streets, where they remained for the remainder of the night, the nobles and wealthier inhabitants sleeping in their carriages. And yet it was said that only one stone was shaken from its place in Naples. There was one point to which the natives flocked with interest on the next morning to see how far art had withstood the fierce anger of nature. It was the gate of a *porte cochere*—that of the Palais of Justice, if we rightly remember—where a wide and delicate hood of masonry stretched over the gateway almost like a piece of textile work. The stone hood was uninjured.

In these regions, built on tufa, and thus in almost organic connection with the sources of volcanic energy, the architect has to gird up his loins in order to take his part in a very serious struggle. A house in Naples is estimated to last for 100 years, undergoing in that time two pretty complete renovations Of the solidity of the work an idea can be formed from the thickness of those 17 ft. walls of which we have spoken. They, indeed, are exceptional, but not so exceptional as might be imagined. Walls of a thickness of 3 ft. and 4 ft. in any building of considerable size, of fifty or sixty years old, are rarely undisfigured by seams which tell of past earthquakes. The Royal Palace at Caserta, a masterpiece of Vanvitelli, and built with little regard to expense, is disfigured by not a few vertical seams, which bear witness to the violence of the shocks which, at different dates, rent but could not overthrow the noble structure.

We are not aware that any such local differences in the effects of the shock were witnessed in Naples in 1858, as was the case in Lisbon in 1755, owing to the different conductive powers of different geological formations. But for the whole circle of the bay, which measures some twelve miles straight across from Naples to Sorrento, the movement of that night elevated the ground by about 8 in., a level which it maintained at all events for five or six years thereafter.

It is quite easy to understand how in a locality subject to such disturbances, the normal proportions of houses are of quite another strength from those with which the English architect is familiar. And there are two or three peculiarities to which it was well to direct attention The responsibility of the architect, or of the builder is of a much more serious nature in Naples than it is with us. It extends, by common law, so as to cover maintenance for a definite time; and thus the builder, for his own sake, builds strongly. Thus, brickwork, as we have it, is unknown to Naples. Most of the building are of stone or tufa, and when *mattone*, or brick, is used, it is in the form of the flat Roman bricks, or rather tiles, of which we have some instances in the remains of Roman work in this country, and of which the strength and durability are extraordinary. *Mattone* is more costly than the tufa generally used for internal work in Naples, and it is also considered more durable. In the third place, there is no doubt that the mortar used by the Italian builder is far better than any commonly used in England. And this is the more worthy of attention, because, as has been before noticed in the *Builder*, the principle of making mortar is contrary to that generally adopted by English engineers and architects. Probably no one in our day has studied the question of masonry more carefully than did I. K. Brunel. His designs for the bridges and other works on the Great Western, the South Wales, and other railways to which he was engineer, were exceptionally light and bold; his aim being to employ a small bulk of the very finest work, in preference to a larger bulk of ordinary work. To this end his specifications were drawn with a care that was never wearied; his great energies continually bringing out fresh editions of his normal masonry specification. The mortar was prescribed to be made with fresh slaked lime, mixed while yet hot,

and any mortar not used in the course of the day in which it was mixed was to be removed from the work by the contractor. The lime was also the subject of very careful provisions.

In Southern Italy, on the contray, the lime is slaked a year, or by preference two years, before it is made into mortar. The first thing done on commencing a building is to dig a pit into which as much lime as is thought likely to be required for the work is cast, and covered with water. It is so kept, as far as possible, under water during the whole progress of the woik—being dug out as a damp paste for mixing with sand prior to use. As to the excellence of the result there can be no manner of doubt—any more than as to the apt and ready skill of the Italian masons, who have all the facilities at their command that we usually regard as in the province of the carpenter. The centers of arches, for example, instead of being built of wood, and lagged, are roughly and rapidly built of stone, and smoothed over with earth or clay, to receive the permanent arch stones. We have seen in a week or two after one of those local earthquakes at the foot of Mount Vesuvius—which did much more damage in that part of Campania than did the wasted energy of the Potenza earthquake of 1858—the cracks in the houses soldered up, the broken lintels replaced, and a large building that looked a hopeless ruin restored to a habitable condition in a few weeks, by the industry of the Italian masons. And of the confidence which this craftsman puts in his work, the excellence of the mortar no doubt is not the least determining condition.

Professor Palmieri, the seismologist, or earthquake doctor, has been writing to give his opinion that the catastrophe at Ischia was due to subterranean subsidence rather than to earthquake proper. And Mr. Mallet, M. Ins. C.E., who made a special study of the phenomena of the earthquake of 1858, of which we have spoken, maintains that all earthquakes are the effect of such subsidence. We cannot ourselves, having witnessed repeated earthquakes of more or less activity, subscribe to any doctrine that attributes these alarming phenomena to so simple a physical cause—a cause, moreover, which is not only hypothetical, but not very easy to explain on its own account. That there is something of the nature of the electric shock in the earthquake, we think most physicists who have any experience of the sensation caused will hold. We desire to speak with all modesty, and that the more so from our only very recent acquaintance with the fact that electricity, as a source either of light or of motion, is now known to be convertible into heat or other forms of motion. Thus, that an internal shock, such as that produced by the fall of the roof of a great cavern, might arrive at the surface of the earth as an electric disturbance, is quite in harmony with what we know of what is now called dynamo-motor power. But then our experience of mines, tunnels, salt works and the like, is rather opposed to the notion of any such internal cavities forming themselves under the action of water. It is possible that the different conductivity of different geological strata, of which we have witnessed with our own eyes the very evident proof that is to this day presented in the streets of Lisbon, may, if carefully studied, throw some light on this question. Subsidence is not a new phenomenon; and in cases like those of the thick coal-seam of Staffordshire, and the salt-veins and brine-pumpings of Cheshire, a much greater amount of subterranean erosion is at work than we can in any way attribute to the percolation of water in any part of the globe. But though houses are ruined, districts made bare and waste, and lakes formed by subsidence in Staffordshire and in Cheshire, we have no earthquakes there. On the other hand, to witness, after a series of shocks, whether occurring in a few minutes or extending for some days, a displacement such as that of the Bay of Naples, or the effects of the more violent local shocks which rent nearly every house at Torre del Greco, Torre Annunziata, and Resina, and that threw out a spring of hot water that ran for three weeks, betokens, we imagine, some far more violent energy than the subsidence of the roof of an unknown cavern. The terror that the earthquake causes, not in mankind alone, but in the whole animal kingdom, is a special feature of these terrific visitations. The idea that this terror is caused by any process of reasoning is one not to be entertained by any who have experienced it. That

there is something akin to electricity in the shock of the earthquake is, we think, proved, among other things, by the nature of the panic instinctively caused in man and beast by even the lightest shock. In the case of an earthquake slightly felt in Wiltshire, some ten years ago, our own attention was roused by the violent terrors evinced by a pair of Australian grass parakeets. We had not noticed the shock, but we did note the time thus fixed; and on the following day the newspapers gave accounts of a shock of earthquake felt in that part of England at the precise moment indicated by the terror of the birds.

WATER POWER OF THE SOUTHERN ATLANTIC SLOPE.

By MR. GEORGE F. SWAIN.

Proceedings of the Society of Arts.

The 282d meeting of the Society of Arts was held on Thursday, March 23d, at 7.30 P.M., President Francis A. Walker in the chair. After the transaction of some matters of business, the President introduced Mr. Geo. F. Swain, of the Institute, who spoke of the Water Power of the Southern Atlantic Slope of the United States.

Mr. Swain gave a brief description of the general structure of the Appalachian Mountain system, and a comparison between the character of the streams in the northern and southern portions. His remarks had reference only to that part south of the James River. The total area of the region studied was about 117,350 square miles, with an average width of about 240 miles. The rivers are navigable from the ocean in many cases nearly up to the point where falls are found, but are obstructed by sand bars and snags to a considerable extent. Upon the maps exhibited a line was drawn through the lowest points at which falls occurred on the streams. Between this "fall line" and the sea there is no water power, and the streams are sluggish. From this line to the sources of the streams are located all the powers.

In the discussion of the amount and value of the various powers it is necessary to know something of the amount of water flowing in the stream. As few measurements of this sort have been made on these southern rivers, considerations as to the amount of rainfall and the nature of the soil must be used in making these estimates. Charts showthe amount of rainfall in various parts of the region were shown. The rainfall is often greater in this region in winter than in summer, but at that time the evaporation is less rapid, and hence the streams would receive the greatest addition when least needed, and would thus be more variable than the rivers of the northern region. The absence of lakes and the rapid removal of the forests also contribute to render these streams more irregular in flow. As there are no considerable falls of snow and no great formations of ice, the southern streams would be comparatively free from spring freshets.

The facilities for artificial storage basins in this region are not particularly great, as in many cases the valleys which must be overflowed compose the best farming lands.

The convenience of transportation is an important consideration in the estimate of the value of a power, and the location of the railroads at present is somewhat unfavorable. The topography of the region is such as to render it easier to secure a good location for the railroads upon the divides or ridges separating the streams than along the valleys. Hence many of the falls are at a distance from the roads.

The following brief list of some of the most notable powers, most of them undeveloped, will show, however, that there is a very large amount of available power upon these streams. The figures are given only for the most important powers, and are computed from approximate figures for the dryest season of a year dryer than the average:

The Roanoke River has at one place a fall of about 84 feet in nine miles, which could furnish about 18,500 horse-power.

The Dan River, at Danville, Va., has a

fall of 22 feet, capable of furnishing 1200 horse-power.

The Tar, at Rocky Mount, has a fall of 20 feet, with 350 horse-power.

The Cape Fear has, at Smiley's Falls, 27 feet fall in 3½ miles, with 2850 horse-power, and at Buckhorn Falls 20 feet fall, with 2000 horse-power. The Deep and Haw rivers are the most utilized of the streams of North Carolina. The former has eight cotton factories, and has a fall of 24 feet at Lockville, giving 800 horse-power, which is partially utilized. The Haw carries seven cotton factories. Both have several powers not utilized.

A very remarkable gorge exists at the Narrows of the Yadkin River, where there is a fall of 150 feet in 3½ miles, which could furnish 15,000 horse-power, but owing to the precipitous nature of the banks cannot be made available. At Bean's Shoal is a fall of 39 feet in four miles.

Upon the tributaries of the Santee are several fine powers. Of these the Wateree has falls of 42 feet, 173 feet, 35 feet, and 25 feet; the Congaree, at Columbia, S. C., one of 22 feet; the Broad and Lockhart shoals one of 46 feet in 1½ miles, and at another point one of 102 feet in 6¼ miles; the latter is, however, enclosed by rather steep banks. Upon the tributaries of the Broad are many more smaller powers.

At Augusta, Ga., is one of the best powers in the South. The Savannah River there has a fall of 50 feet from the Augusta Canal, seven miles long. Of this fall 33 feet are utilized, capable of furnishing 9000 horse-power, of which but a portion is now used. At Trotter's Shoals is a fall of 75 feet in seven miles, and in the upper tributaries are many smaller falls.

The Altamaha River has on its tributaries many powers but little used.

ON THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS.

AN INVESTIGATION OF THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS USED IN ENGINEERING CONSTRUCTION, CONDUCTED IN THE MECHANICAL LABORATORY OF THE DEPARTMENT OF ENGINEERING OF THE STEVENS INSTITUTE OF TECHNOLOGY.

By ESTEBAN DUQUE ESTRADA, M. E.

Contributed to Van Nostrand's Engineering Magazine.

I.

Introduction.

The timber trees of Cuba, like those of every tropical country, are remarkable for their great size, both in diameter and height, their almost perfect indestructibility by those agents which ordinarily injuriously affect timber, such as insects, dry-rot and decay generally, their hardness and homogeneousness, the beauty of their texture and grain, and the fragrance of their wood.

As a class, they are but little known in the United States, a few varieties only being imported, such as lignum-vitae, rosewood, ebony, various kinds of cedar, and the several varieties of mahogany. The extended use of hardwoods in interior and exterior house-decoration will, it is thought, greatly increase the demand for certain valuable Cuban woods. The whole subject has a special interest and value to the writer, these being woods with which he is familiar, and he has ventured to take the subject of the strength of certain Cuban woods as a subject for investigation. It was decided to confine the investigation to comparatively few of the more important and representative types of timber, on the natural supposition that the slight differences between the several varieties would not seriously affect the general conclusions to which such experiments would generally lead.

In procuring these woods the writer had the good fortune to secure the co-operation of Mr. Alfredo Leblanc, of Cienfuegos (Cuba), who carefully se-

lected such pieces as, in his judgment, would best represent the useful, rather than the merely ornamental, "timber trees." These carefully-selected specimens probably represent as good an average as could be obtained by proper inspection.

The writer regrets very much that lack of time would not permit him to make these series of experiments complete in every detail.

The experiments by transverse and torsional stress are, it is hoped, as complete as they can be made; not so complete, however, are the experiments by compression, and especially by tensile stress. In studying up the subject the writer found little or nothing of value bearing upon any experimental determinations of the mechanical properties of the woods of Cuba.

DESCRIPTION OF WOODS.

BARIA (*Cordia gerascanthoides*) attains a height of about 60 feet (18.3 meters), and a diameter of 18 inches (46 centimeters) and often more. It is found in nearly all parts of the island, being more abundant towards the central part.

The wood has a dark greenish brown color in the heart, and is lighter in the sap-wood. It is highly prized on account of its strength, durability and lightness. The wood when varnished has a very handsome appearance. It is extensively used in framing, carriage making, and general house-fitting. Its specific gravity is 0.78.

CAOBILLA (*Crotos lucidam*) is said to be a variety of the Caoba (*Swientenia mahogani*). It grows on black soils, and near the coast. It attains a height of 40 feet (12.1 meters), and is quite abundant. The wood is light red in color in the heart and brown white in the sap wood; is fine grained, and, as a general rule, is inferior in strength and durability to mahogany. It is chiefly used for furniture, boarding and framing. Its specific gravity is 0.80.

COCULLO (*Bumelia nigra*) is found on hills and rocky lands. It attains a height of 50 feet (15.2 meters). It has many branches, has but little sap-wood, and can be obtained in considerable quantities.

The wood is strong, heavy and elastic, and is yellowish white in color. It has a different appearance when seen across the grain, being dotted with white spots. It is used for cart constructions and buildings, is very durable, and can be worked moderately easy. It can be obtained in logs 12 inches (30.5 centimeters) square. Its specific gravity is 1.15.

DAGAME (*Colycophyllum candidissimum*) is one of the most plentiful trees of the forests of Cuba, being generally found near mountains, and in reddish soils. A common height is from 40 to 50 feet (12 to 15.2 meters). Its trunk is straight and quite free from branches.

The wood is of a pale yellow color, very fibrous, is close grained, thus resembling box-wood; is moderately heavy, and very strong and elastic. It is very easily worked, either across or with the grain. It turns remarkably well, is entirely free from knots, and susceptible of good polish; it is very durable.

It is used very extensively in general carpentry, for the wood-work of plows, cart axles, spokes and spikes; is an excellent material for house-framing for its strength and durability, and joiners prefer it for their work to most other woods. It is also very extensively employed by carriage manufacturers, for ships' yardarms, and other similar purposes. The largest section that can be obtained after squaring is 12 inches (30.5 centimeters). Its speciffc gravity is 0.90.

GUAYACANCILLO (*Guaiacum verticale*), is very much like the Guayacan (*Guaiacum officinale*), or what is known in this country as lignum-vitæ, and appears to be one of its varieties, its dimensions being smaller than those of the latter, and its leaves fewer in number. It is found quite abundantly along the road to Puentes Grandes, which furnishes the main supply. The largest logs that can be obtained from this tree are from 10 to 20 feet (3 to 6.1 meters) long, and 6 to 10 inches (3 to 6 centimeters) square.

The wood is light yellow in the sapwood, and dark brown in the heart, sometimes with dark green shades. The other properties and its uses are exactly those of the lignum-vitæ, with this exception that the guayacancillo has a higher modulus of rupture and coefficient of elasticity. It hardens upon exposure to the air. In Cuba it is chiefly employed for sheaves of pulley-blocks, or water-wheels, and for all the uses to which lignum-vitæ is put. Its specific gravity is 1.08.

JIQUI COMUN (*Bumelia nigra*) is found in large supply, especially in the center part of the island, where it abounds in all kinds of soil. It attains the height of from 50 to 70 feet (15.2 to 21.3 meters). The largest section obtainable after squaring is 18 inches (46 centimeters).

The wood is one of the hardest known, is exceedingly heavy, fine grained, and very compact. It has a rosewood color, and becomes harder and darker with age, and when very old it is almost black, and is very difficult to work. It is principally used for posts, in consequence of its great durability and strength, and is a very fine material for dock-piles, owing to its non-liability to decay; is not attacked by insects, and it is said that it petrifies under water. It is also used for telegraph poles, piles for foundations, scaffolding, railings, and main posts for gateways. It turns easy, and with good tools it can be made of any desired shape. Its specific gravity is 1.20.

JOCUMA AMARILLA (*Sideroxilon solicifolia*) grows preferably on rocky soils, and is largely found near the coast; it reaches a height of 60 feet (18.2 meters), and furnishes logs of 16 (40.6 centimeters) after squaring.

The wood is of a light yellow color, heavy, fine grained and strong; is entirely free from knots, cup or star shakes, and works well. It is largely used in framing, for beams, tiles and posts, cabinet work, and general building constructions. Its specific gravity is 1.04.

JUCARO PRIETO (*Bucida*) is found in large supply near the southern coast, and attains a height of from 60 to 80 feet (18.2 to 24.3 meters), for which it requires 50 to 55 years growth; it has lateral roots, and yields gum by incision.

The wood has a dark brown color, much resembling black walnut, is very strong, tough and elastic, is heavy, fine grained, and free from knots. It stands the weather remarkably well, is worked easily, and is susceptible of good polish, thus producing a handsome effect.

It is largely employed in naval constructions, for purposes where strength and durability are required. It is also very extensively used by millwrighters, and is an excellent material for posts, piles, and general dock constructions. It can be obtained in logs of 36 feet (10.9 meters) length, and 16 inches (45.7 centimeters) square. Its specific gravity is 1.08.

JUCARO MASTELERO (*Bucida*) is found on low lands and valleys. It attains a height often exceeding that of the jucaro prieto, which is one of its varieties. Its fruit is of great value for cattle food. It is very abundant on the low lands of Camaguey. The wood has a light yellowish brown color, is very strong, hard and elastic; it has very little sap-wood, and is readily worked. It is used very much in naval constructions, for railroad ties, framing, millwrights' work, and for cart axles it is given the preference. It can be obtained in logs 16 inches (45.7 centimeters) square. Its specific gravity is 0.89.

MAJAGUA AZUL (*Paritium elatum*) is very abundant, reaching a height of 50 feet (15.2 meters), and often more. It attains a great age. The forests of Camagüey and Vuelta Abajo abound with this tree. Logs of from 16 to 20 inches (45.7 to 50.8 centimeters) square are very common. The trees growing in dry and rocky soils give the best timber, so far as strength and elasticity are concerned.

The wood is used for ships' ribs and framing, planks and boards, carriage work, furniture and gun stocks. The bark of this tree furnishes an excellent material for ropes and hawsers, being particularly valuable because so little affected by dryness or dampness.

The wood is very easily worked, closely resembing the white pine of this country in this respect. In color it is of a deep greenish blue from which property it derives its name. Its specific gravity is 0.72.

MAJAGUILLA (*Carpodiptera cubensis*) is found in Buelta-Abago; attains a height of from 40 to 50 feet (12.1 to 15.2 meters), and its bark is of great value for the manufacture of rope.

The wood is hard, heavy and strong; it is quite resinous, and consequently difficult to work. Its color is yellowish red. It is principally used for posts, carts and other constructions where finish is not of great importance. The largest section obtainable is 14 inches (35.5 centimeters). Its specific gravity is 1.11.

OCUJE MACHO (*Calophyllum calaba*) is of straight growth, attaining a height of from 50 to 60 feet (15.2 to 18.2 meters); its trunk is long and straight. It is very plentiful in the forests of Camagüey.

The wood is said to be indestructible, comparatively light, of a fibrous texture, and of a reddish color. This wood is highly valued in carpentry and rural constructions; is an excellent material for piles, for which purpose it is very extensively used; it is also used for spars and general ship construction, cart axles, and millwrights' work, Its specific gravity is 0.84.

Quiebra-hacha (*Copaifera humenaefolia*) is found in great abundance along the southern coast of the island, where it attains a height of upwards of 50 feet (15.2 meters) and considerable diameter; it has very little sap-wood, and makes an excellent timber.

The wood is of a dark red color, very much resembling the darker kinds of mahogany. It is exceedingly hard and heavy, and yet is not very difficult to work. It has no equal for use under ground and under water; is perfectly safe against the attacks of insects, of decay, and petrifies under water. Its hardness increases with age, and its color also becomes darker. It is used for all kinds of posts, railroad ties, piles, dock beams, engine foundations, and framing for heavy machinery. It can be obtained in logs as large as 24 inches (61 centimeters) square. Its specific gravity is 1.30.

Sabicu (*Mimosa adorantisima, Vel Acacia formosa*) is plentiful throughout the island, and attains colossal dimensions. It abounds chiefly on rocky and sandy soils. Its growth is somewhat crooked and irregular, but it yields an excellent timber of from 30 to 40 feet (9.1 to 12.1 meters) long, and up to 36 inches (92 centimeters) square. It has very little sap-wood, and its bark is an excellent material for tanning. The wood has a dark chestnut color, and is often twisted or curled in the fibers, sometimes very much resembling rosewood. It is hard, heavy and strong. It is very durable, and when entirely free from the sap-wood it is not affected by insects, even when left unprotected by paint. It works remarkably well across or with the grain.

The wood is used in shipbuilding, not only in Cuba, but also in England and Spain, for beams, keelsons, engine-bearers, and stern-posts, pillars, cleats, and for various other similar uses. In Cuba it is principally used for posts, dock constructions, millwrighting, and to nearly all the uses to which the quiebra-hacha is put. Its specific gravity is 0.95.

Yava (*Andira inermis*) is very common, especially in reddish soils; it attains a height of 60 feet (18.2 meters), and reaches a great age.

The wood varies very much in color, which leads to the belief that there are different species, and yet they belong to the same family. Some specimens of it resemble very much in their color the yellow pine of this country. It is very hard, strong and durable. It is used in millwrighting, for wheel segments, spokes, &c. It is very valuable for ship constructions, especially for keels, rudders and boarding. The largest section obtainable after squaring is 16 inches (40 centimeters). Its specific gravity is 0.88

Description of Testing-Machines.

The testing-machines used in these experiments were the standard machines used of the Mechanical Laboratory of the Department of Engineering of the Stevens Institute of Technology, and are fully described by Prof. R. H. Thurston, in his paper on the "Strength of American Timber," as follows:

The Tension and Compression Machine consists of two strong cast iron columns, secured to a massive bed-frame of the same material; above these columns is fastened a heavy cross-piece, also of cast iron, containing two sockets, in which rest the knife-edges of a large scale beam. The upper chuck is suspended by two eye-rods from two knife-edges $\frac{1}{2}$ in. to one side of center of a heavy wrought iron block, which is hung by two links from two pairs of knife-edges projecting from the scale beam on opposite sides of the knife-edges of the latter, and at equal distances from them, the whole making a very powerful differential beam combination. All the knife-edges are of tempered steel and the sockets and eyes are lined with the same material, thus reducing friction to a minimum. The stress is applied by means of a hydraulic press, with a fixed plunger and movable cylinder; to the latter the lower chuck is fastened by means of an adjustable staple and link. The stress to which the test-piece is subjected is measured by means of suspended weights and a sliding poise not seen in the figure. The specimen is secured in the chucks

either by wedge-jaws or cord chucks, according to the specimens to be tested. The capacity of the machine (Fig. 1) is twenty tons.

The extensions are measured by means of an instrument in which contact is indicated by means of an "electric contact apparatus." The instrument consists essentially of two very accurately made micrometer screws, working snugly in nuts is made between the two insulated points and one pole of a voltaic cell, and also between the micrometer screws and the other pole. As soon as one of the micrometer screws is brought in contact with the opposite insulated point, a current is established, which fact is immediately revealed by the stroke of an electric bell placed in the circuit. The pitch of the screws is 0.02 of an inch, and

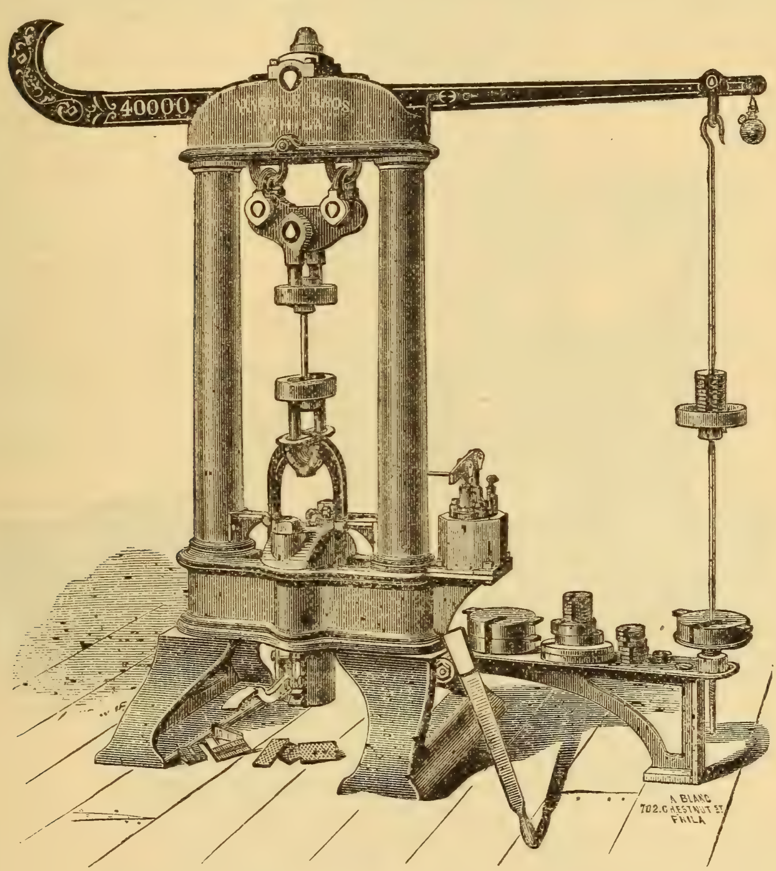


FIG. 1.—TENSION AND COMPRESSION MACHINE.

secured in a frame which is fastened to the head of the specimen by a screw clamp. It is so shaped that the micrometer screws run parallel to and equidistant from the neck of the specimen on opposite sides. A similar frame is clamped to the lower head of the specimen, and from it project two insulated metallic points, each opposite one of the micrometer screws. Electric connection their heads are divided into 200 equal parts; hence a rotatory advance of one division on the screw head produces a linear advance of one ten thousandth (0.0001) of an inch. A vertical scale divided into fiftieths of an inch is fastened to the frame of the iustrument, and set very close to each screw head and parallel to the axis of the screw; these serve to mark the starting point of the former,

and also to indicate the number of revolutions made. By means of this double instrument the extensions can be measured with great certainty and precision, and irregularities in the structure of the material, causing one side of the specimen to stretch more rapidly than the other, do not diminish the accuracy of the measurements, since half the sum of the extensions indicated by the two screws is always the true extension caused by the respective loads.

ment for measuring the deflections is not shown in the cut; it consists of an accurately cut micrometer screw of steel, having a pitch of 0.025 of an inch, working in a nut of the same material mounted in a brass frame. This instrument is supported by a rod of considerable rigidity and of sufficient length, which is secured to the beam C, close to the tension rods FF, in such a manner that the micrometer is directly under the cross-head, in the same vertical plane with the test-

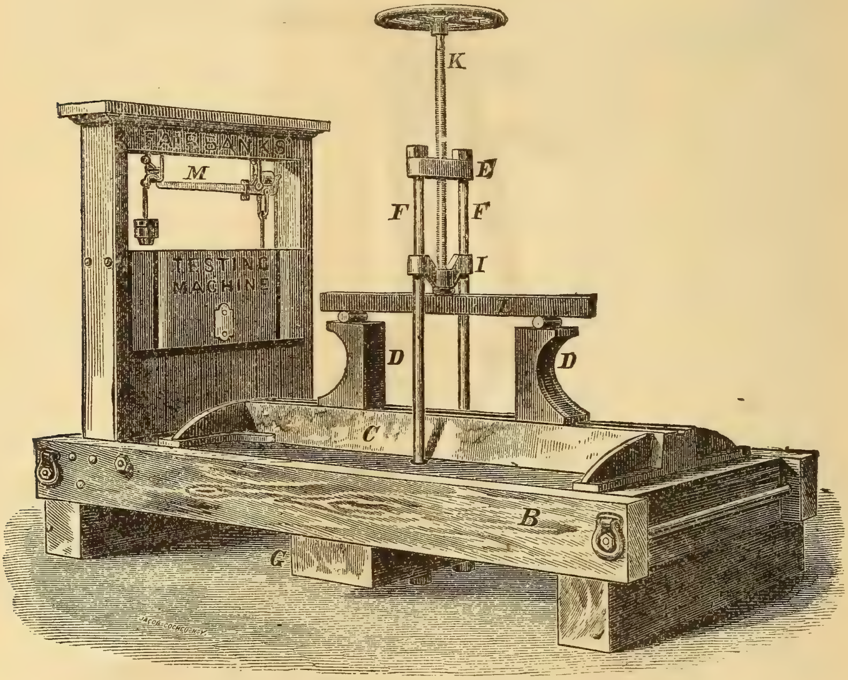


Fig. 2.—Transverse Testing Machine.

The Transverse Testing-Machine (Fig. 2) consists of a Fairbanks scale, on the platform of which rests a heavy cast-iron beam C, to which are fastened the supports DD at the required distance apart. The pressure is applied by means of the band wheel on the upper end of the screw K, which screw passes through the nut E, and terminates in the sliding cross-head I. This cross-head serves both as a guide and as a pressure block. The test-piece L rests upon mandrels mounted upon the supports DD at the required distance apart. The loads are weighed in the usual manner at M. The instru-

piece, and very near and parallel with the axis of the large screw K. The micrometer screw is provided with a head which is divided into 250 equal parts. Thus a rotatory motion of one division produces an advance in the direction of the axis of the micrometer screw of 0.0001 of an inch. A scale divided into fortieths of an inch is fastened to the frame of the instrument, in close proximity to the head and parallel to the axis of the screw; it serves to mark the starting point, and indicates the number of revolutions made in taking a measurement with the screw. To insure accurate

readings of the deflections the principle of the electric contact is also employed here. The capacity of the machine is 7,000 pounds.

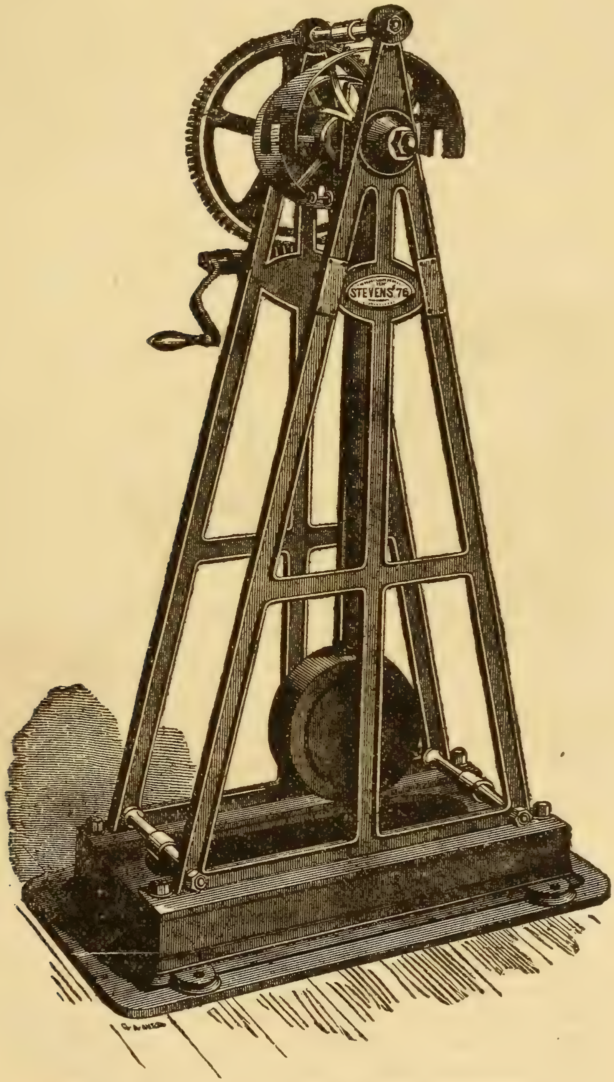


FIG. 3.—AUTOGRAPHIC MACHINE.

Prof. Thurston's Autographic Machine (Fig. 3) consists of two strong cast-iron wrenches facing each other with a space of $1\frac{1}{4}$ inches between their jaws. They rotate in independent journals placed in the same lines in the frames; the latter are bolted to a heavy bed-plate, which gives it the required stability. One of the wrenches is provided with an arm 4.5 feet in length, at the lower end of which is attached a heavy weight B; the other wrench has keyed to it a worm-wheel engaging with the worm which is set in motion by means of a crank. In this manner a very slow and quite uniform motion can be obtained.

Both wrenches are provided with lathe-centers directly opposite each other and in the common axis of rotation. The specimen to be tested is placed upon

the lathe-centers, which hold it in line while it is being secured in the jaws of the wrenches by means of steel wedges inserted from opposite sides. On the shaft of the wrench carrying the worm-wheel there is fastened a brass drum which rotates with it, while to the other wrench is fastened a pencil-holder which allows the point of the pencil to move on the surface of the drum, and is guided by the stationary curve of brass, in such a manner that its position on the drum indicates the number of foot-pounds of moment exerted by the arm and weight at any instant.

Supposing a test-piece to be placed in the machine the operator turns the crank with a uniform velocity, which gives a slow and a very steady motion to the wrench connected with the worm-wheel, which is transmitted through the test-piece through an arc which is a measure of the resistance to torsion offered by the test-piece, and is recorded simultaneously with the angle of torsion by the pencil upon a diagram-sheet fastened upon the drum for the purpose. The drum is of such a diameter that the circumference is 36 inches, which, when divided into tenths, makes 360 divisions, each of which is representative of one degree. The guide curve is a curve of sines, which insures the position of the pencil on the drum always such that it marks an ordinate proportional to the moment of the arm and weight at every instant during the test. The friction of the machine is not recorded by the machine but is added in calculating the results.

Dimensions and Form of Test-Pieces.

In preparing the test-pieces it was endeavored to make them conform as nearly as possible to the standard shapes and sizes of the Mechanical Laboratory. This could only be done in the case of the torsion specimens. Since the size or length of the test-specimen ought not theoretically to affect the strength of the wood, it was deemed safe to make the transverse specimens as large as possible.

In preparing the test-pieces for the tensile tests it was found necessary to provide against the shearing or crushing of the pieces in the chucks of the testing-machine. The test-pieces were prepared, viz., 1.5 inches (3.81 centimeters) wide, 1 inch (2.54 centimeters) thick, and 14 inches (35.5 centimeters) long, a hole 1 inch (2.54 centimeters) in diameter was then drilled at a distance of three inches (7.62 centimeters) from each end, or as much as the yoke of the machine would permit; it was found, however, that this distance of three inches thus available at the upper end was not sufficient to prevent shearing. Thus the experiments for tension are incomplete, and but few are reliable.

The compression specimens were made as large as the capacity of the machine would allow, always preserving the proper ratio of length to diameter.

Experiments to Obtain the Modulus of Elasticity by Transverse Stress.

The coefficient of elasticity for transverse resistance was obtained in the following manner: Each specimen was planed to a square section and to the largest size which its original dimensions permittted; then the breadth and depth were accurately measured by means of a micrometer screw reading to the 0.001 of an inch, after which it was placed on the transverse testing-machine, fixing the supports so as to allow 2 inches (5.08 centimeters of the test-piece to project beyond them. The beam being carefully balanced, the weight of the piece included, a load of 50 pounds (226.8 kilogrammes) was applied each time up to 200 pounds (907.18 kilogrames), and the corresponding deflections measured by means of the apparatus previously described. In most cases the deflections were directly proportional to the loads, and where any difference occurred it was exceedingly small. Several pieces were tried of different lengths, and the results obtained were accordant.

The formula used in these determinations was $E = \frac{Pl^3}{48 \Delta I}$, where P represents any load within the elastic limits, l the distance between the supports, Δ the corresponding deflection, and I the moment of inertia which, for a rectangular section, equals $\frac{1}{12} bd^3$.

Modulus of Elasticity by Transverse Stress.

Woods.	Length in inches.	Breadth in inches.	Depth in inches.	Loads in pounds.	Deflections in inches.	Difference in inches.	Modulus of Elasticity per formula $E=\frac{Pl^3}{4Dbd^3}$
Baria I_1.	36	1.78	2.00	50	.023	.023	1700000
				100	.046	.023	
				150	.069	.023	
				200	.092	.023	
Baria I_2.	36	1.50	2.00	50	.026	.026	1790000
				100	.052	.026	
				150	.078	.026	
				200	.104	.026	
Baria I_3.	36	2.10	2.12	50	.018	.018	1620000
				100	.036	.018	
				150	.054	.018	
				200	.072	018	
Caoba.	28	1.72	2.65	50	.006	.006	1429166
				100	.012	.006	
				150	.020	.008	
				200	.026	.006	
Caobilla.	30	1.46	2.50	50	.008	.008	1849315
				100	.016	.008	
				150	.024	.008	
				200	.032	.008	
Cocuyo K_1.	40	2.15	2.04	50	.019	.019	2310469
				100	.038	.019	
				150	.057	.019	
				200	.078	.019	
Cocuyo K_2.	40	1.97	2.01	50	.021	.021	2200000
				100	.042	.021	
				150	.063	.021	
				200	.084	.021	
Cocuyo K_3.	40	2.16	2.00	50	.017	.017	2130000
				100	.034	.017	
				150	.051	.017	
				200	.068	.017	
Dagame J_1.	40	2.06	2.18	50	.015	.015	2500000
				100	.030	.015	
				150	.045	.015	
				200	.060	.015	
Dagame J_2.	40	1.94	2.28	50	015	.015	2280820
				100	.030	.015	
				150	045	.015	
				200	.060	.015	
Dagame J_3.	40	1.50	2.03	50	.027	.027	2361623
				100	.054	.027	
				150	.082	.028	
				200	.110	.028	
Dagame J_3.	40	2.03	1.50	50	.048	.048	2433460
				100	.096	048	
				150	.143	.047	
				200	.192	.049	

Woods.	Length in inches.	Breadth in inches.	Depth in inches.	Loads in pounds.	Deflections in inches.	Difference in inches.	Modulus of Elasticity per formula $E=\frac{Pl^3}{4Dbd^3}$
Guaya-cancillo.	82½	1.50	2.59	50	.007	007	1503561
				100	.014	.007	
				150	.021	.007	
				200	.028	.007	
Jocuma Amarilla G_1.	36	1.54	1.55	50	.043	.043	2250000
				100	.086	.043	
				150	.133	.047	
				200	.177	.044	
Jocuma Amarilla G_2.	36	1.47	1.61	50	.062	.062	2300000
				100	.119	.057	
				150	.186	.067	
				200	.243	.057	
Jiqui Comun E_1.	28	1.82	2.00	50	.0075	.0075	2520952
				100	.0150	.0075	
				150	.0225	.0075	
				200	.0300	.0075	
Jiqui Comun E_2.	28	1 90	2.35	50	.000	.005	2490272
				100	.015	.000	
				150	.010	.005	
				200	.025	.000	
Jiqui Comun E_2.	36	1.90	2.35	50	.010	.010	2490000
				100	.020	.010	
				150	.030	.010	
				200	040	.010	
Jiqui Comun E_2.	40	1.90	2.35	50	.013	.013	2490000
				100	.026	.013	
				150	.039	.013	
				200	.052	.013	
Jiqui Comun E_3.	28	1.87	1.87	50	.009	.009	2490000
				100	.008	.009	
				150	.027	.009	
				200	.036	.009	
Jucaro Mastelero B_4.	40	1.81	1.83	50	.044	.044	2000000
				100	.088	.044	
				150	.132	.044	
				200	.176	.044	
Jucaro Mastelero B_5.	40	1.56	1.50	50	.056	.056	2500000
				100	.118	.052	
				150	.183	.065	
				200	.248	.065	
Jucaro Prieto B_6.	28	1.29	1.68	50	.019	.019	2300000
				100	.038	.019	
				150	.056	.018	
				200	.074	.018	
Jucaro Prieto B_1.	28	1.50	1.81	50	.014	.014	2200000
				100	.028	.014	
				150	.042	.014	
				200	.056	.014	

Modulus of Elasticity of Transverse Stress—*Continued.*

Woods.	Length in inches.	Breadth in inches.	Depth in inches.	Loads in pounds.	Deflections in inches,	Difference in inches.	Modulus of Elasticity per formula $E=\frac{Pl^3}{4Dbd^3}$	Woods.	Length in inches.	Breadth in inches.	Depth in inches.	Loads in pounds.	Deflections in inches.	Difference in inches.	Modulus of Elasticity per formula $E=\frac{Pl^3}{4Dbd^3}$
Jucaro Prieto B_2.	28	1.45	1.75	50 100 150 200	.016 032 .050 .068	.016 .016 .018 .018	2195200	Sabicu A_4.	28	1.21	1.59	50 100 150 200	.025 .050 .075 .100	.025 .025 .025 .025	2300000
Jucaro Prieto B_3.	28	1.50	1.87	50 100 150 200	.014 .028 .042 .056	.014 .014 .014 .014	1995636	Sabicu A_5.	28	1.25	1.76	50 100 150 200	.012 .024 .036 .048	.012 .012 .012 .012	2400000
Majagua Azul D.	28	1.65	1.88	50 100 150 200	.0135 .0260 0390 .0525	.0135 .0125 .0130 .0135	1939393	Quiebra-hacha C_1.	28	1.43	1.78	50 100 150 200	.016 .032 .048 .064	.016 .016 .016 .016	2100000
Majagua Azul D_3.	28	1 98	2.12	50 100 150 200	.007 .014 .021 .028	.007 .007 .007 .007	2000000	Quiebra-hacha C_3.	28	1.73	1.76	50 100 150 200	.014 .028 .042 .056	.014 .014 .014 .014	2100000
Majaguil-la L_2.	40	1.66	2.12	50 100 150 200	.020 .040 .060 .080	.020 .020 .020 .020	2000000	Yava H_1.	40	1.51	1.54	50 100 150 200	.063 .125 .187 .249	.063 .062 .062 .062	2322463
Majaguil-la L_3.	36	2.25	2.31	50 100 150 200	.010 .019 .028 .037	.010 .009 .009 .009	2257548	Yava H_2.	40	1.50	1.51	50 100 150 200	.065 .130 195 .264	.065 .065 .065 .069	2393258
Ocuje.	28	1.38	1.64	50 100 150 200	.030 .060 .090 .120	.030 .030 .030 .030	1500000	Yava H_3.	40	1.50	1.51	50 100 150 200	.065 .130 .196 .262	.065 .065 .066 .066	2393000
Sabicu A_1.	28	1.22	1.82	50 100 150 200	.015 .030 .045 .060		2486070								

THE DEFINITION OF FORCE.

From "The Engineer."

Force is a something of which most people think they have had experience, and which to an engineer in particular is the very element in which he moves. It may well seem strange, therefore, that doubts should still exist as to its proper definition. Yet that there are such doubts it is impossible to deny. There are, in fact, at the present moment three separate schools of thought on the subject. Two of them are products of these latter days—equally bold and positive in their novel views, but wholly irreconcilable with each other. The third represents those who are content *stare super antiquas vias*, and to retain the definition which satisfied their fathers, but who, nevertheless, are quite aware that their friends of the new light—or lights rather, possibly somewhat interfering with each

other—regard them as sunk in worse than Egyptian darkness.

The first of these new schools shelters itself under the ægis of Mr. Herbert Spencer. That gentleman, ever since he published his "First Principles," has claimed a high place amongst the authorities on mechanical science; and this claim seems to be most readily admitted in all those circles where mechanical science is least understood. Now it is not too much to say that, in the eyes of Mr. Herbert Spencer, force is everything, and everything is force. The Persistence of Force is the one great, unquestionable, all-embracing principle, which explains all other principles, including evolution itself, and is the foundation and essence of the physical universe. True, Mr. Spencer does not anywhere define the persistence of force. But so far as we can gather, the persistence of force means that the forces of nature are continuous, not discontinuous; that they are always in action, not sometimes acting and sometimes quiescent. If we go further, and ask for a definition of force, we fear that neither will this be forthcoming; but at any rate we may gather that force is reality, if not the only reality. For this definition, if we could get it, would be a most comprehensive one; it would embrace what we mean when we speak of the force of a sledge hammer, and also what we mean when we speak of the force of an argument. So at least we may gather from Mr. Spencer's disciples, if not from himself. The latest of these disciples appears under the name of Mr. Norman Pearson. This gentleman deliberately, and without a smile on his countenance, adopts the view just stated; and actually founds an argument for the immortality of the soul on the ground that the soul is a force, and that all forces "persist." It is true, he admits frankly, first, that he knows nothing at all about the force of the soul; and secondly, that, so far as he does know, it is a chemical force, resulting from special combinations of phosphorus with carbon, &c. Now, nothing can well be more certain, than that these processes of combination cease when the man dies and his brain turns to dust; but as the soul must of necessity persist, that only proves that the soul is a force of some other character. By parity of reasoning, the same will of course hold of other descriptions of force—say, the force of a conclusion, the force of a repartee; the force of a joke; nay, we are thereby emboldened to put all the force we can into this present article, in the assurance of thereby rendering it as immortal as ourselves.

The second party we have alluded to proceed in a wholly different fashion. Far from regarding force as everything, they regard it as nothing. The leader of the party in this country is Professor Tait, whose knowledge of mechanics, unlike that of the gentleman named above, will be most fully recognized where mechanics are best understood. Now Professor Tait has said repeatedly that we have no right to regard force as having any objective existence whatever. All we know is that bodies, under certain circumstances, alter their velocities at a certain rate, and this rate of change of velocity—or taking into account the mass of the bodies, their rate of change of momentum—is that to which Professor Tait gives the name of force. When we speak of force, all we mean, or ought to mean, is this rate of change of momentum. This, and only this, is what we have to investigate. On this view the persistence of force, which to Herbert Spencer is the first of all truths, becomes not only meaningless, but false; for if there is one thing clear, it is that all bodies are not continuously changing their relative velocities. Moreover, since force is only a rate of change of momentum, it ought to be possible to write a book on elementary mechanics without introducing the conception of force at all. Professor Tait recognizes this, and some little time ago he presented to the Royal Society of Edinburgh a sketch of the way in which such a book might be drawn up. How the Society felt after it we have not heard. We do not, of course, question for a moment that Professor Tait understood his own meaning. We will go further, and admit that if he could have got one of his hearers quietly by himself for an afternoon or so, he might have made him understand it also—say that he understood it, at any rate; but further than this we are not able to go. Nevertheless, Professor Tait has followers no less confident and enthusiastic than those of Mr. Spencer; and only a few weeks ago, in these columns, the as-

sertion was made that in physical science no other meaning can possibly be given to force than the rate at which momentum is transferred from one body to another.

Who shall decide, when doctors thus disagree? The humble student of mechanics, anxious to learn what the "men of light and leading" in his generation have to tell him about the ultimate principles of his science, stands by perplexed, embarrassed, perhaps at last a little indignant. We will not presume to offer him advice, but, as one brother recounts his spiritual experiences for the good of another, so we, as humble students also, may perhaps whisper to him how we have succeeded in taking comfort to ourselves. In the first place, we would suggest that he need not trouble himself much with the views of Mr. Herbert Spencer or his followers. They may be left with perfect confidence to time and to themselves. In the second place, as regards the school of Prof. Tait, we would ask him to look at an utterance coming, not from an enemy, but from a supporter, or at least a candid friend of the school in question. In the *Philosophical Magazine* for April, 1883, he will find a short note by Mr. Maxwell Close, in which he points out very clearly the manifold confusions, the almost inextricable jumble, in which the ordinary terms and conceptions of mechanics are involved, by the adherence to this wrong definition of force as the rate of change of momentum. Having read this article, our student will probably do one of two things—he will either give up mechanics in disgust, or he will take heart and resolve to see whether after all there is not some third definition which has been given for force, and which has been upheld by men with whom a student need not be altogether ashamed to agree. And he will be surprised to find that this is so. For instance, he may come across the following words: Force is an action exercised on a body tending to change its state either of rest or of uniform motion in one direction." This is a literal translation from a book called the "Principia," written—some time ago it must be confessed—by one Isaac Newton. Now, Newton is generally supposed to have known something of mechanics—in fact, a good many people are disposed to put him very near the head of mechanical philosophers. True, freshmen of Trinity College, Dublin, are said to prefer Salmon; the students of Edinburgh may thunder "Tait," and the Burschen of Germany may shout for Kant or Helmholtz. But all these, we are inclined to think, would put Newton next to their favorite hero; and therefore—as in the celebrated Greek election, where each man voted for himself first and Themistocles second—we may fairly forecast what would be the result of an unbiassed decision. But Newton's view of force, as we have seen, was quite different from either Spencer's or Tait's; and our student will be still more surprised to learn that this was not an antiquated superstition of his—that it has been largely held since, and by men not in themselves inconsiderable. Thus we read: "Force is that which tends to cause or to destroy motion, or which actually causes or destroys it?" This is the first sentence in "The Mechanical Principles of Engineering and Architecture," by Henry Moseley. Again, "Force is said to be whatever produces, destroys, or changes motion:" this is the definition of Whewell. Whewell and Moseley were not unknown in their day as students of mechanics; their fame even yet lingers among us. Coming down to more recent times, we find the definition of Newton restated as follows: "Force is an action between two bodies, either causing or tending to cause change in their relative rest or motion." These are the words of William Macquorn Rankine, to whom even Edinburgh students will not refuse a measure of respect. In like terms Harvey Goodwin—who is undoubtedly a mathematican, and when he wrote these words could not even be branded as a bishop—"Force in any cause which changes or tends to change a body's state of rest or motion." If he looks abroad he finds that Navier, Morin, and Redtenbacher measure force in kilogrammes; and whatever we take a kilogramme to be, it certainly is not a rate of change of momentum. Lastly, if he turns to the "Elements of Natural Philosophy" itself, he will be astonished to find that its distinguished authors, *nolentes volentes*, have delivered themselves in the following terms: "Force is any cause which tends to alter a body's natural state of rest, or of uniform motion in a straight line."

Fresh from this search our student will

be able to appreciate at its full worth the confidence which asserts that it is quite impossible in physical science to look at force in any other light than as the rate of change of momentum. He will assert humbly, but fearlessly, that it is quite possible; that, in fact, it has often been done; that better men than himself or his interlocutor have been content so to do; nay, perhaps his bad passions may get the better of him, and he may be led to declare that it is not he who lives in darkness, but rather in the light of truth, with Newton, and Whewell, and Rankine, and Thomson, and Tait.

The fact is, the whole question is simple enough to those who approach it from the right direction. Force is known to us in two ways—first, as exercised by ourselves upon other things; secondly, as exercised by other things upon us or each other. It is the second class of forces which is studied in mechanics. These forces, like everything else outside us, can only be known ultimately by their effects. The most special and obvious effect of force is motion, and it is the leading principle of mechanics that forces are proportional to the motions which they cause, these motions being measured by the momentum given to the bodies on which they act. So far, there is no room for difference of opinion. But it is easy to make a step further, and to say that forces are not only *proportional* to the changes of momentum they cause, but that they *are* those changes of momentum, and nothing else. It is this step which has been made by Professor Tait, or rather it would seem by his followers. Let us see how it is justified by an appeal to similar cases. We will take first an illustration furnished us by Mr. Maxwell Close in the article already referred to. Disease causes death, and the strength of different diseases, or of the same disease at different times and places, might be measured by the increased rate of mortality which they have induced; therefore, on these principles, we must define disease as being a rate of increase of mortality, and must demand that our medical literature should be re-written in order that it may square with this definition. Again, the burning of fuel under a boiler causes the evaporation of steam, and, other things being kept the same, the rate of evaporation in cubic feet per hour will be proportional to the quantity of fuel burned; therefore, we must say that fuel is the rate of evaporation of water per hour in a given boiler, that and nothing else; and our treatises on steam boilers must proceed on that supposition.

We may go on forever quoting instances to show the absurdity of the step thus confidently taken, but it is needless. Our opponents can really find only one thing to say. They may point out that we do not call disease an increase in death-rate, or fuel a rate of evaporation, because we know a good deal about disease and fuel over and above the particular effects which they thus produce. We will not stop to inquire what would be the force of this objection, if it were true—as is here tacitly assumed—that we know nothing of force except from the motions it causes. It is sufficient to observe that this assumption is obviously and absurdly false. As it has been well put to us, it would seem that these gentlemen can never have got wedged in a crowd. We know that force produces at least one thing besides motion, namely, pressure. In fact, in our persons we know it much oftener as the cause of the latter than of the former. To put this latter effect aside altogether, and insist on dealing with the former as the absolute and exclusive effect of force, is as unwarrantable in theory as it is absurd in practice. A definition of force which involves the statement that there is no force acting between oneself and the earth, no force between the jib of a crane and the weights hanging motionless from it, no force between a locomotive engine and the train which it is trying in vain to start—such a definition is actually worse than that absence of all definition which we find among the followers of Mr. Spencer. Like them, it may safely be left to itself; meanwhile, we shall be contented with that view of force which contented the men whose names are quoted above, and on which they and their compeers have reared the magnificent fabric which goes by the name of mechanical science.

A NEW electric arc lamp, the invention of Mr F. L. Willard, has appeared at the Fisheries Exhibition in London. The novelty consists in the device for holding and releasing the pendulum which controls the fall of the carbon.

SOLIDITY AND BREADTH IN ARCHITECTURE.

From "The Building News."

The quality of solidity is one deservedly valued in architectural composition, though we have very little evidence of it in modern buildings, partly owing, it must be observed, to the prevailing practice of designing in elevation. We shall probably never know the real extent to which the old architects relied upon plans and models for producing their grandest expressions of formal beauty, though the little insight we have into their modes of working prove beyond a doubt that the elevation was never made use of in the manner it is now. Much of the superficial character of modern architectural work is due to the limited areas we have to build upon, which renders it necessary to make the boundary lines of our buildings straight, or nearly so. To save ground, the plan must have few projections or recessions, and any depth for play of light and shadow must be reduced to the smallest dimensions. It is upon this principle that architects perforce are content to work and to give us drawings of faces instead of solids. Alison, in the section of his work on the sublimity of forms, cites the rectangular as the most expressive of strength and contrast. "The great constituent parts of every building," he says, "require direct and angular lines, because in such parts we require the expression of strength and stability." Again, speaking of sublimity in form, he mentions "magnitude in height," and "magnitude in depth." Ruskin also in his "Seven Lamps of Architecture," Chapter III., observes, "the relative majesty of buildings depends more on the weight and the vigor of their masses than on any other attribute of their design; mass of everything, of bulk, of light, of darkness, of color, not mere sum of any of these, but breadth of them; not broken light, nor scattered darkness, nor divided weight, but solid stone, broad sunshine, starless shade." In this respect the Greeks excelled all other architects. All their forms were rectangular. Both in the plan and openings the right angle was the predominant element. One author, Mr. Garbett, remarks the square-headed openings and recesses had an expression of power we fail to observe in Gothic edifices. The same principle pervaded the abacus, the cornice, the architrave, and other parts of the Doric temple. "Until," says Ruskin, "our street architecture is improved, until we give it some size and boldness, until we give our windows recess and our walls thickness, I know not how we can blame our architects for their feebleness in more important work." Unfortunately, the elevational treatment of facades renders them more or less pasteboard imitations. In every set of competition designs we see flatness and fritter of surface. Want of recess or depth is one of the chief weaknesses of modern architecture. Mr. Garbett, among others, has pointed out the want of thickness of walls and recess or window reveals, and it is in these respects the competition system has been so prejudicial to good architecture. There is a natural desire on the part of competition draughtsmen to give their prespectives exaggerated depth of returns in excess of that absolutely shown on the scaled drawings. The walls and projections are made to look thicker and bolder than they are intended to be; and the result is disappointment when the designs are carried out. Everything has been taken with the scantiest allowance; the pilasters are mere strips, the window reveals are only a few inches in depth, and the porticoes or projections are the merest subterfuges for the realities which they are intended to represent in elevation. It is this apparent flatness of surface and want of contrast of surfaces that has robbed the river front of the Houses of Parliament of half its dignity and effect. The windows look like perforated openings in thick pasteboard, so little is the thickness of recess, or the distance to the glass. The perspective drawings showed recessed openings of considerable depth from the wall plane. But this shallowness of recess is common in buildings of the present day. A reveal of 4½in. is often seen in houses of large dimensions,

which for their size ought to have windows of at least 9in. or 14in. recess to the glass. A Pall Mall copy of a celebrated facade at Venice—namely, the Sansovino Library—is spoilt by the want of depth of reveal given to the windows. In the Pall Mall example there is only depth enough for the return of the pilaster; in the original the depth of return admits two columns and space between. At least a sixth of the width of opening ought to be set back as a return to the glass, so that a 5 ft. window or opening ought to have at least a 9 in. reveal. The question of depth or thickness of wall is, of course, governed by the principle of fitness in proportion. The height of a wall requires a certain thickness to give an apparent stability to it so the width and heigth of an opening demands a certain depth of reveal. Vitruvius, in his treatise on Architecture, mentions ichnography, orthography, and scenography as necessary to design. The first is the representation of the ground plan of the building; the orthography is the elevation of the front, shadowed, and the last, scenography, is the art of representing the front and *receding* sides properly shadowed, the lines being drawn to their proper vanishing points. So that the most ancient writer on architecture has propounded the necessity of plan, elevation, and perspective—as equally requisite means of representing a building. Scenography—in other words, perspective—is the only method by which the element of depth or thickness can be expressed; but how few architects ever design their buildings in this manner, or if they do, exhibit them to a proper scale! Architecture is a thing of three dimensions, it must have length, breadth, and depth—in other words, soldity; but architects draw their designs as if they had only two: the third dimension is disregarded: even if it is shown, it is often purposely misrepresented. Variety and contrast of surfaces can be obtained only by showing extent of depths as well as surfaces. The magnitude and sublimity of St. Paul's Cathedral, the British Museum, the Royal Exchange, the Bank, Somerset House, and the fine towers by Wren, are attributable mainly to the element of a third dimension. What would they be if they were only facades, without depth of return or projections or quadrangle? The breaking up of plan is thought a means of obtaining the quality of solidity; but the expedient is very frequently carried too far. In Gothic buildings, the length of actual walling so broken up might have inclosed an area of double the size, and real solidity is sacrificed to picturesqueness. On the whole, it may be shown that buildings of rectangular form in plan have more solidity of effect than the same area broken up into small parts. The plan of St. George's Hall, Liverpool. is three parallelograms conjoined, and a central attic over the great hall. The magnitude and sublimity of this structure arise from its severe rectangular outline and fewness of parts. Here we have a facade of 400 ft. in length, with a portico of 16 columns 46 ft. in height. The projections are few, and both in plan and elevation the breaks consist of simple masses of square form. But a situation in a square is not always obtainable. It is not always possible to go round a building, or to see its depth and length at a glance, and seldom are more than two sides exposed. An architect who can exhibit two sides of his building may consider himself fortunate, as then he can impose two fronts on a spectator, and impress him by a depth as well as a frontage. But it is not often that the architect has the opportunity of exposing two fronts of a building; he is content to endeavor to impress the public by the length and height of his facade, he cuts it horizontally into stories, and piles order upon order. The projections and recesses are slight, and the depth of window reveals scarcely that of a plank. To cover this deficiency of solidity, the surface is strewn with ornament, and the facade under a strong sunlight becomes as expressionless as a face without characteristic features. There are few recent buildings in London which can show this quality of solidity. The City of London School on the Embankment, by its bold receding window fronts and the depth of columns and soffit of arched recess, is one of the few buildings in which the quality of solidity and depth of return has been made a feature.

Apparent thickness can be obtained externally by other means than those which suggest themselves to ordinary

builders. If thickness of wall cannot be made available, the windows can be set back within the area; the jambs of openings can be made to look solid by return pilasters or columns, with spaces between, not, as frequently seen, by shallow pilaster returns to the window frame. In many recent designs for buildings, we find the projections and recesses made so slight as scarcely to give a *raison d'etre* for the break. For example, a portico, or center, breaking forward a few inches looks a very insignificant effort for mere show sake. It is far better not to have broken the front at all. If a massive tower is introduced in the facade, there is reason for a decided projection; a very slight one becomes often exceedingly ridiculous. Solidity of appearance may be obtained by carrying up features, such as towers and wings, so as to exhibit the return sides of those parts. How much the modern architect may learn from the Greek and Gothic artist can only be seen by noticing the plans of their buildings. The severe, right-angled form of the Grecian temple and its features gave a solidity of effect to them; the peristyle imparted depth and shadow to the facades; while the deep buttresses and projecting transepts and portals of the Mediæval church served the same purpose by throwing bold shadows. From what we have said, it will be noted that there are at least two modes of imparting the effect of solidity to buildings. One is by a study of plan—that is, in making projections of parts and features, and observing the principle that the right angle is the most expressive of strength and power; the other plan is by recessing the windows and entrances, by intercolumnar shadows and buttress projections. We may wait vainly for improvements in these respects, until the designer is taught to learn the viewing of a building in perspective, a study of it in its three dimensions of length, breadth, and thickness, and not simply in orthographic projection. Architects who have been endeavoring to reproduce "Queen Anne" and Late Gothic have strangely fallen short of this power, the projections of overhanging stories, gables, and other features being often so small as to appear mere counterfeits.

The subject of solidity leads us to notice the want of "breadth" in our modern work—a quality sadly sinned against, if it is at all respected. By breadth, we mean that amount of plain surface rest for the eye or repetition of parts which a building requires to give it consistency. It is a very common fault of most designs that they are divided or broken up into a multiplicity of parts; the surface is frittered away by ornament and frivolous features, so that the unity of the composition is almost lost. Such an effect is commonly seen in Gothic villas, in which breaks of gables, bay windows, chimneys, and turrets occur in a promiscuous fashion. Even the breaking up of a front by orders or pilasters may be carried so far as to suggest feebleness in design. Pilasters or insulated columns may be placed too close to each other, and there seems to be a proper proportion to be observed in their introduction, as in all mere ornament, beyond which they become mischievous and utterly destructive to repose. Solidity and breadth are two qualities which are violated every day by designers of the English Renaissance and half-timber styles, and both proceed from the same error of not studying plan, and in the equal distribution of ornament over flat surfaces without regard to perspective, light, and shadow.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—Sept. 19th, 1883.—A discussion by Mr. Charles Douglas Fox, of London, Corresponding Member of the Society, "On the Increased Efficiency of Railways," was read by the Secretary.

Mr. Fox referred to the fact that English railway managers and engineers have long realized the great importance and economy of a thoroughly substantial road bed. The formation widths on their chief railways are now made 30 feet both in cuttings and on embankments for the double lines, and very great care is taken to thoroughly dry this formation in cuttings by deep ditches on each side, with earthenware drain pipes in them, and fill in with broken stone, or other dry material. The ballast consisting of broken stone, clean gravel, coarse sand, burnt clay or ashes, is not allowed to be less than one foot in thickness below the bottom of the tie. For lines of constant and heavy traffic, the bull head grade, double-headed rail, having a large top member for wear, and a very small bottom member, is found to be the best section for steel rails. The weight of these rails is 84 pounds per yard. The chairs are from 40 to 46 pounds each, and the rails are secured in them by keys of compressed oak. The tendency of the Eng-

lish companies is to expedite traffic, both passenger and goods, not by higher rates of speed, but by reducing the number of stoppages. The traffic lines are gradually quadruplicating their tracks, in some cases throughout, in others by sidings several miles in length. There is a very general feeling in England in favor of identifying the driver with his engine, and holding him responsible for its working. On some lines the name of the driver is conspicuously attached to the engine. Mr. Fox forwarded also the railway regulations of the English Board of Trade, which give very minute directions in reference to the construction and running of railways.

A paper by Mr. Wm. Howard White, M. Am. Soc. C. E., was also read upon the subject of "Railroad Bridge Floors." Mr. White advocates inside guard rails for the purpose of preventing, as far as possible, serious results from the derailment of wheels.

His reasons for advocating the inside guard rails are, that he considers them more efficient for the same height above the tie than the outside guard; that they can be placed so as to hold the wheel nearer the rail, particularly when the use of the snow plow is considered; that they can be more strongly secured at the ends for the purpose of drawing derailed wheels towards the rail, or to secure the ditching of a car which has gone too far to be safely drawn back; that they are more economical.

He considers that the ties should have five inches of clear distance between them.

The papers were discussed by Messrs. Wm. H. Paine, Cooper, Blunden and Bogart.

A paper by Mr. James Christie, M. Am. Soc. C. E., on Experiments on the Strength of Wrought Iron Struts, was read by the Secretary in the absence of the author. These experiments were made at the Pencoyd Iron Works for the purpose of determining the comparative resistances to compression of long and short struts of rolled angles, tees, beams and channel sections. The specimens were tested by four different methods, viz., with flat ends between parallel plates to which the specimen was in no way connected; with fixed ends or ends rigidly clamped to parallel plates, the plates substantially forming flanges to the specimen; with hinged ends, or both ends fitted to hemispherical balls and sockets or cylindrical pins; with round ends or both ends fitted to balls resting on flat plates. The specimens varied in length from six inches up to 16 feet and were selected to obtain a uniform character of material. The paper gave tabulated results of 299 experiments, and these results are illustrated by a number of diagrams. There were also results given of a number of tests of welded tubes. The general conclusions drawn from these experiments were as follows:

($\frac{l}{r}$ being length divided by least radius of gyration.)

When struts are short, say $\frac{l}{r}$ below 20, there will be no practical difference in the strength of the four classes, so long as reasonable care is taken to keep the center of pressure in the center of the strut. Hinged ended struts vary all the way from round ended up to flat ended in strength. If the hinges are pins of substantial diameter, well fitted, and exactly coincident with the axis of greatest resistance of the strut, the strength of the strut will be fully equal to that of a flat ended; but considering the impractability of maintaining this rigid accuracy, the average hinged struts, as compared with flat ended, will fall in strength as the length is increased until $\frac{l}{r}$ is about 250, when they will average one-third less resistance than flat ended. From this point they will gain comparatively until $\frac{l}{r}$ becomes about 500, when both classes will be practically equal. Fixed ended struts gain in comparative resistance from the shortest lengths upwards until $\frac{l}{r}$ becomes about 500, when they are twice as strong as either the flat or hinged ended.

Round ended struts continually lose in comparative resistance as the length is increased, when $\frac{l}{r}$ is about 340 they will be half as strong as hinged ended, and when $\frac{l}{r}$ is about 160, they will have only half the strength of flat ended.

The iron from which the tests were made exhibited the following resistances to direct compression, being the general results of several tests of small section fifteen inches long and secured in such a manner as to prevent lateral flexure.

With 30,000 lbs. pressure per square inch incipient permanent reduction of length was observed.

With 35,000 lbs. pressure per square inch failure of elasticity occurred, and marked permanent reduction of length.

With 50,000 lbs. per square inch a permanent reduction of length of three per cent. occurred.

With 75,000 lbs. a permanent reduction of ten per cent., and with 100,000 lbs. pressure per square inch a permanent reduction of twenty-eight (28) per cent. of the length.

The paper was discussed by Messrs. Theodore Cooper and Charles E. Enery, who both expressed the opinion that these experiments were of very great value, being made with material of uniform character, and in such a way that comparisons could be made directly between the different methods adopted in testing.

ENGINEERING NOTES.

A singular accident happened lately at Zell, Canton Zurich. A new iron bridge over the Joess had just been completed, and in order to test its stability three wagons laden with cotton were drawn across it, a number of the local authorities being on the spot watching the proceedings. As the wagons started the bridge yielded slightly to their weight, and they had hardly reached the middle when the entire structure collapsed, and wagons, horses, and spectators were precipitated into the

stream. Herr Ott, Mayor of Zell, was killed, and Herr Winkler, a member of the Great Council, and two other persons, one of them the engineer, were so badly hurt that they are not expected to recover. The breakdown is ascribed to the indifferent quality of the iron of which the bridge was constructed.

SUBMARINE EXPLORATION.—Signor Toselli is reported to have invented an apparatus for exploring the depths of the ocean without danger or inconvenience. It is about 25 feet in height, and constructed of steel plate with gun-metal castings. It is calculated to resist a pressure of 180 lbs. to the square inch, so as to be able to attain a depth of 65 fathoms. The internal space is divided into three compartments—at the bottom, a chamber capable of being enlarged or contracted by a flexible diaphragm, so as to increase or diminish the volume of water displaced, and thus permit of rising or sinking; a room capable of holding eight explorers, occupying the central portion of the spheroid, and provided with lenses, so as to permit of looking out; and the upper space, reserved for those entrusted with manœuvring the vessel. A powerful electric lamp is to shed its rays all round the apparatus for a considerable area, and telegraphic and telephonic wires will place those in the vessel in communication with the steamer from which it is suspended.

DEEPENING RIVERS.—It is well known that the Mississippi River gives a great deal of trouble, and it would appear that the losses and dangers incurred by floods are augmenting. We have long since pointed out in these pages that the proper way to prevent floods from doing harm is to lower the beds of the rivers instead of building embankments. American engineers begin at last to realize the fact that this theory is sound, and a scheme for lowering the bed of the great river is now being discussed. Mr. Erkson proposes the use of barges or deep-water boats from 500 ft. to 600 ft. long, so constructed as to be capable of being sunk and anchored to the bottom of the river, so as to create strong currents, and thus cut away bars or cut-off bends, and give a uniformity of current to the stream. The upper portion of the barges deflects the top or surface current of the river, and assists it in carrying off the obstructive matter which is raised. The barges can be taken up, it is said, and removed in two hours to some other place, or their positions can be entirely changed in a much shorter space of time, and by their use Mr. Erkson holds that rivers and bars can be ploughed to a depth of 30 ft. According to an American contemporary, this scheme has met with the approval of the United States Corps of Engineers. The idea is very ingenious, how far it is practicable must depend mainly on the river. There are, however, many places in which the use of movable obstructions would no doubt prove useful in causing the automatic modifications of river beds, and we commend Mr. Erkson's scheme to the attention of our readers without claiming more for it than that it is worth consideration and investigation. As regards the Mississippi, something must be done very soon. It is stated by the ablest and best informed engineers, that in a comparatively short space of time the Crescent City will be practically stranded, high and dry, thirty miles from the river, by its breaking through into Lake Ponchartrain; and Vicksburg and Greenville will each be twenty miles distant from the river bank. From Greenville to Memphis, the majority, if not all, of the plantations and villages will within twenty years be ruined. Within a shorter time Fort Randolph is in danger of being left fifteen miles from the river bank. The towns and plantations in the vicinage of Island No. 10 are in great danger of being destroyed, and Cairo, some engineers tell us, will shortly become a town on the Ohio River, twenty miles from the Mississippi. Even St. Louis, strange as it may seem, is in no little danger.

BRIDGING OVER THE STRAITS OF MESSINA.—Attention is called by the *Rheinisch Westfalische Zeitung* to a proposal of Signor A. Giambastiani for erecting a bridge over the Straits of Messina, instead of piercing the tunnel which has been for some time under discussion in Italy. His experience in designing bridges of extensive span has been, it is said, of a varied character, and he was chief engineer in the construction of the Italian approaches to the St. Gothard Tunnel. He proposes to have five openings, the three middle to be each 1,100 yards in length, and the two side openings to be each have that length. The pillars are to be of granite, and the openings will be spanned by arched girders of steel, the rise of which is designed to be one-tenth of the width of the arch. Signor Giambastiani intends, it is said, to perfect his design in accordance with the detailed local investigations he proposes to make, and then to place it before the Italian Minister of Public Works. With reference to this scheme, Herr Cottrau, director of the "*Impresa Industriale Italiana*," has called attention to the fact that the idea is not new, as he made studies for the same purpose in 1866. He had proposed openings of 650 to 875 yards, but after careful examination had arrived at the conclusion that the secure placing in position of solid pillars in the Straits of Messina was either impossible, or only practicable by means of an expenditure out of proportion with the results to be obtained. He founded this opinion on his investigations respecting the depth of the channel and the force of the current, and it was for this reason that the matter was then abandoned.

RAILWAY NOTES.

ELECTRIC TRAMCARS IN PARIS.—Some authentic facts have now been published about the recent trials of tramcars driven by storage batteries in Paris. M. E. Rouby, engineer of arts and manufactures, has furnished the data which we are about to give. The car propelled is of the large size, constructed by the Compagnie Générale des Omnibus de Paris. It contains fifty-two passengers, and was built for horse traction. M. Raffard, the engineer who had charge of the experiments, fixed the dynamo or motor (a Siemens D_2 type) under the

floor of the car near the platform. This dynamo received the current from eighty accumulators placed under the seats of the car. From this dynamo a belt and pulley, with differential movement controlled an intermediate shaft, which carried two pinions gearing with chains and toothed crowns carried by the wheels of the car. On June 24, a car with thirty passengers left the Place de la Nation at 4 A. M., and arrived at La Muette, near the gates of the Bois de Bologne *via* the external Boulevards, at 5.20 A.M. After half an hour's halt, it returned to La Nation by 7 A. M., having accomplished 32 kilometers with a mean speed of 11 to 12 kilometers per hour. The total weight of the car, with the eighty accumulators of 30 kilogrammes each, was about 9 tons. The mean current was 35 ampères at 160 volts E.M.F. The electric work furnished to the dynamo was, therefore, $\frac{35 \times 160}{746}$ = (about) 7 horse-power during $2\frac{1}{2}$ hours, and the accumulators were still unexhausted. In the second experiment, at the Champs Elysées, the car went indifferently on the rails or macadamized road.—*Engineer.*

At the commencement of last year Germany possessed 33,707 kilos.—1 kilo. = $\frac{5}{8}$ mile—of railways of ordinary gauge, 192 of narrow gauge, and 1477 of mountain lines. Of this number 22,325 were owned and worked by the State, 3737 were owned by private companies, but worked by the State, while 7644 were owned and worked by private companies. The State possessed in Prussia 11,505 kilos., 4267 in Bavaria, 1942 in Saxony, 1535 in Wurtemburg, 1185 in Baden, 270 in Hesse, 278 in Oldenburg, and 89 in Saxe-Coburg-Gotha. But if we include all the private lines administered by the State, then we find that Prussia possessed about 15,000 kilos, almost half of the whole German system. The most important private company lines are those of Altona-Kiel, Berlin-Hamburg, Brunswick and the Palatinate. The cost of establishing the German railway system was 8400 millions of marks—£420,000,000—varying from 45,333 marks to 759,654 per kilo. The proportion of first-class traveling to second-class is 104 first in every 10,000 travelers, to 1355 second. The railway administration employs altogether about 300,000 persons, thus distributed: In the general management 7977, and 3457 temporary employés, with 840 artisans; on the lines themselves, 30,060 permanent and 2663 temporary employés, with a staff of 58,021 workmen; or, in round numbers, 90,143 persons, while the traffic necessitates a body of 72,555 employés and 55,852 workmen.—*Engineer.*

IRON AND STEEL NOTES.

On some Theoretical Considerations Connected with the Hardening and Tempering of Steel.—By Professor Chandler Roberts, F. R. S.

The Cutlers' Company of London, have during the past few years, instituted a series of lectures in connection with the general scheme for technical education. The last of the series for the present session was delivered on Thursday, April 12, by Prof. Chandler Roberts, F. R. S., "On some Theoretical Considerations connected with the Hardening and Tempering of Steel."

He traced the history of the subject, and, quoting Guyton Morveau, said that Stahl, the great supporter of the Phlogistic theory, considerd that iron, "cemented" with carbon in closed vessels, gained phlogiston, and became in consequence steel; and this was the opinion of his disciples, who considered steel to be merely iron possessing the characteristics of a metal in a higher degree than iron, a view which will be found in the works of Henckel, Cramer, Gellert, Rinman, and Maquer. Bergmann (1871), to whom we owe the discovery of the fact that the presence or absence of graphite makes all the difference in the properties of wrought-iron, steel, and cast-iron, retained the phlogistic theory generally, although he considered that steel contains less phlogiston than wrought-iron. Professor Roberts pointed out that we are still repeating Bergman's question, "How does the graphite act" in producing the singular difference between hard and soft steel. The early experimenters who followed Bergman knew the importance of establishing the action of carbon in converting iron into steel, and Clouet, in 1796, followed by others in this country, converted soft iron into steel by heating it with the diamond. In these early experiments furnace gases had not been excluded, and it was urged that they might have converted the iron into steel without the intervention of the diamond. A past master of the Company of Cutlers, Mr. W. Haseltine Pepys, repeated in 1815, Clouet's experiment, under conditions which left no doubt as to the action of the diamond, for he employed electricity as a source of heat, and thus avoided the action of furnace gases altogether.

It was then shown that in soft, tempered, and hardened steel respectively, the carbon has a "distinct mode of existence," and the evidence as to whether carbon in steel is *combined* in the chemical sense, or is merely *dissolved*, was considered at length. The chemical evidence given by Berzelius, Karsten, Gurlt, Forquinon, and recently by Sir F. Abel, the distinguished chemist of the War Department, was then reviewed. With regard to the "solution" theory held by Vandermonde, Berthollet, and Monge in 1786, there is the recent and important calorimetric work of Troost and Hautefeuille, who showed that in white cast-iron, and probably in steel, the carbon is merely dissolved, a view which the lecturer adopted, as he did not consider it to be in any way in opposition to the fact established by Abel, that the carbon left by the slow action of a chromic acid solution is in the form of a definite carbide.

The various physical, as distinguished from the chemical, theories which had been set forth from the time of Rèaumur, 1722, to that of Akerman, 1879, to account for the "intimacy of the relation" of carbon to iron in hardened as compared with soft steel, were then described. In recent years much importance had been attached to the physical evidence as to the peculiar constitution of steel, and it had been shown that there is a remarkable relation be-

tween the amount of carbon contained in different varieties of steel and their electrical resistance. The latest work, however, in this direction has been done by Prof. Hughes, and his very interesting experiments on the effect of torsion on wires of wrought-iron, soft and hard steel, through which a current is passed, was then described. The effect of hardening in oil on the tensile strength of steel of different degrees of carburization was then shown by the aid of curves, and it was incidentally pointed out that in the case of the variety of steel used for the manufacture of coinage dies a variation of 1-10th per cent. of carbon makes a great difference in the quality of the metal.

The New Standard Wire Gauge for Great Britain.—

Schedule.—Denominations of Standards.

Descriptive number.	Equivalents in parts of an inch.	Descriptive number.	Equivalents in parts of an inch.
No.	Inch.	No.	Inch.
7/0	.500	23	.024
6/0	.464	24	.022
5/0	.432	25	.020
4/0	.400	26	.018
3/0	.372	27	.0164
2/0	.348	28	.0148
0	.324	29	.0136
1	.300	30	.0124
2	.276	31	.0116
3	.252	32	.0108
4	.232	33	.0100
5	.212	34	.0092
6	.192	35	.0084
7	.176	36	.0076
8	.160	37	.0068
9	.144	38	.0060
10	.128	39	0052
11	.116	40	.0048
12	.104	41	.0044
13	.092	42	.0040
14	.080	43	.0036
15	.072	44	.0032
16	.064	45	.0028
17	.056	46	.0024
18	.048	47	.0020
19	.040	48	.0016
20	.036	49	.0012
21	.032	50	.0010
22	.028		

The *British Trade Journal* points out that on and after March 1st next no other wire gauge can therefore be used in trade in that country—that is to say, no contracts or dealings can be legally enforced which are made by any other sizes than those above given. Wire drawers and users of the Birmingham wire gauge would do well, therefore, to provide themselves with gauge plates corresponding to the above sizes.

ORDNANCE AND NAVAL.

Nordenfelt Machine Guns.—On the invitation of Mr. Thorsten Nordenfelt, a large and influential company assembled at Dartford, on July 26, to witness the practiae carried out with the many different kinds of Nordenfelt machine guns, ranging from the single-barrelled rifle calibre weapon, up to the 2.2 in. single-barrelled 6-pounder gun. The practice was carried out at Mr. Nordenfelt's private range, at Dartford, and before the commencement Mr. Nordenfelt addressed a few words to the company explaining his views as to the use of machine guns afloat and ashore, and the nature of the mechanism of his various machine guns about to be fired.

The proceedings opened with the firing of the 12-barrelled rifle calibre gun, which is similar in all respects, but in the number of its barrels, to the 10-barrelled weapon which has been previously described and illustrated in this journal. Firing for rapidity without aiming, 590 shots were discharged from this gun in half a minute.

Then the single-barrelled rifle caliber gun, weighing only 13 lb., was similarly fired, with the result of 54 rounds in half a minute. This gun has been constructed as a cavalry weapon, or for use with mounted rifles, but we doubt whether a single-barrelled riffe caliber machine gun is ever likely to be adopted unless it be so exceedingly light that it might be practically introduced for one or other of the foregoing purposes. The 3-barrelled rifle caliber gun, which has a different and even more simple mechanism than either of the other rifle caliber guns exhibited, was next fired for rapidity with the result of 186 shots, or 62 volleys in the half-minute; this gun only weighs 56 lb., and its portability was shown by the ease with which two men carried it over rough ground on its tripod stand. This gun, in common with all the Nordenfelt guns of more than one barrel, has independent action of each barrel.

From the 5-barrelled gun, weighing 128 lb., 300 rounds were fired in the half-minute, and 10 volleys (50 shots) in $4\frac{1}{2}$ seconds; this gun was then dismounted from its wheels and axle, and mounted on its tripod stand, which forms the trail when used with the wheels. The gun and stand were then each carried on poles by two men, to show the portability of this weapon.

Next came the 10-barrelled gun, from which 900 shots were fired in one minute; 100 rounds, with the automatic spreading gear in use, in 7 seconds; 200 rounds at 30 deg. elevation in 13 seconds; and 200 rounds at 30 deg. depression in 11 seconds. The improved scattering motion was here used, by which the bullets are separated 3 ft., so that the 10 shots of the volley cover a space of some 30 ft., and to throw the following volley to the right or left of the preceding one, it is only necessary to turn slightly the training wheel.

This concluded the firing of the small bore guns, from which 2,580 rounds were fired without a single jam, and by one man, thus proving the comparative ease with which these Nordenfelt machine guns, with the horizontal movement of their firing levers can be worked, and also the perfection of their mechanism.

The 2-barrelled 1-in. gun (185 lb. in weight), designed for the armament of torpedo boats, was then fired, at a distance of 50 yards, against a $\frac{1}{8}$ in. steel plate, placed in front of a 1-inch

iron plate, both of which were penetrated by its steel shot. For rapidity of fire, 20 shots were discharged from this weapon in 6 seconds. A series of firing were then made with the 4-barrelled 1-in. gun, usually termed the "anti-torpedo-boat gun," with the following results: 8 single shots in 4 seconds, 12 shots (3 volleys) in 2 seconds, 100 shots (25 volleys) in 24 seconds.

The shell-gun practice was then commenced by the firing of the single-barrelled $1\frac{1}{2}$-in. 2-pounder gun, weighing 3 cwt. At a distance of fifty yards the steel shot penetrated three 1-in. iron plates: and we were informed that at the Portsmouth machine shell gun trial in 1881, it penetrated a $1\frac{1}{2}$-in. steel plate at 300 yards. Captain Lord C. Beresford, R.N., in his recent lecture on machine guns at the Royal United Service Institution, strongly advocated the adoption for the Navy of a shell gun of this nature, and we believe experiments for this purpose are about to be instituted; and it certainly appears to be a most suitable form of armament as judging from what we saw at Dartford, and from the Portsmouth trials of 1881, a $1\frac{1}{2}$-in. shell gun can combine the features of lightness, rapidity of fire, penetrative power, considerable range and accuracy.—*Engineering*.

Earth Torpedoes in Switzerland.—The Geneva correspondent of the *Times* writes: Her von Zubowitz, inventor of the earth torpedoes which were tried the other day at Thun, is an Austrian hussar officer. He has led a very adventurous life, and served as a volunteer in wars in Spain, Albania, Montenegro, and Turkey. At Plevna he was one of Osman Pasha's aides-de-camp. There being no more fighting to be done just now, he has given his attention to perfecting facilities for future warfare. He has recently given a full explanation of the theory of his invention in the presence of the commission charged with its consideration, and of several superior officers of the Federal Army, among whom were General Herzog, Colonel Bleuler, and others who have seen active service in foreign armies. The Zubowitz torpedoes can be placed underground and used as substitutes for *fougasses;* they may be placed in a cart or wagon forming part of a barricade, and so arranged that the least movement of the vehicle will explode the engine. The trials at Thun are described as surprising. The torpedoes are composed of two boxes, one above the other, the first of which contains the charge, a sort of gelatine, while in the second is placed the exploding apparatus. By means of a conducting wire, cleverly hidden, the engine may be exploded at the most propitious moment at the will of a distant, and possibly an invisible, operator, or it may be arranged to go off by simple contact with a man or a horse. When the torpedo is put underground, at a very slight depth, its presence is concealed by stones, turf, or brushwood, according to the nature of the ground. The vibration of the air produced by the bursting of one of these truly infernal machines can be felt at a great distance, and the general effect is something prodigious. The most interesting experiment tried at Thun was the blowing up of two old caissons, which had been placed on a lorry. So violent was the explosion that not alone were the caissons and the lorry literally annihilated, but the spectators who watched the experiment from a "barrack of observation" at a distance of 200 metres were almost blinded by the cloud of dust raised by the blast. The opinion of the officers present at the trials was that the Zubowitz torpedoes will greatly facilitate defence, especially of a country like Switzerland, that possesses so many mountain passes and no strong places; but they cannot be made to supersede fortifications, and their adoption will solve only in part the problem of fortifying the territory of the Confederation, which has long been a burning question in Swiss military circles.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

Selected Papers of the Institution of Civil Engineers.—A Method of Correcting Errors in the Observation of Angles of Plane Triangles. By Robert Manning, M. Inst C. E.

Apparatus for Solar Distillation. By Josiah Harding, M. Inst. C. E.

Tests of German Coals. By Dr. H. Bunte.

The Coal and Mineral Deposits of Indo-China. By Edmund Fuchs and E. Saladin.

Air-Compressor and Turbine. By Benjamin Frederic Wright, A. M. Inst. C. E.

Resistance on Railway Curves as an Element of Danger. By John Mackenzie, Assoc. M. Inst. C. E.

Cheap Gas for Motive-Power. By Joseph Emerson Dorson, A. M. Inst. C. E.

Abstracts from Foreign Transactions and Periodicals.

A Treatise on Electricity and Magnetism. By E. Mascart, J. Joubert. Vol. I. London: Thomas De la Rue & Co.

This work when entire will comprise two parts. Only one is at present complete. The first volume is principally theoretical, and is a complete treatise in itself.

The authors have given special prominence to the views introduced into science by Faraday, and so largely developed by Clerk Maxwell on the consideration of lines of force.

The aim has been to prepare a book for physicists, and to simplify the demonstrations without sacrificing strictness of reasoning.

The translation from the original was made by Dr. Atkinson.

The Modern Applications of Electricity. Second Edition. Revised and Enlarged. By E. Hospitalier. New York: D. Appleton & Co.

Extensive as is the subject comprehended in the title to this work, the treatise doubtless fulfils the expectations roused by it. It is only twelve months since one volume quite satisfactorily covered the ground; now two royal octavo volumes are compactly filled with applications.

The subjects are presented in the following order:

Vol. I., Part I., Sources of Electricity.

Electric Batteries: Thermo-Electric Batteries; Electric Generators; Apparatus for Transforming Electricity.

Vol. I., Part II., Electric Lighting.

Arc Lamps; Electric Candles; Lighting by Incandescence; Applications of Electric Lighting.

Vol. II., Part I., Telephones and Microphones.

Tone Telephones; Speaking Telephones; Special Telephones; Applications of the Telephone.

Vol. II., Part II., Various Applications of Electricity.

Methanometers; Fire Alarms; Electric Water Gauges; Electrical Appliances for Navigation; Electrical Appliances for Meteorological Observations; Electro-Sorting; Electro-Metallurgy; Electrolytical Methods Applied to Manufacturing Processes; Electro-Medical Appliances; Preparation of Parabolic Mirrors by Centrifugal Force; Etching on Glass by Electricity; Electrical Appliances for Railway Communication; Bourdin's Plough for Laying Electric Cables; Siemens Galvanometers: Raimond Coulin's Photometer.

Vol. II., Part III., Electric Motors and Electric Transmission of Energy; The Distribution of Electricity.

ZOÖLOGICAL ATLASES. Invertebrates—Vertebrates. By D. M'Alpine. New York: The Century Co.

The two Atlases, one for each of the sections, are designed for students and teachers of Comparative Anatomy.

Sixteen large plates, with parts colored, are afforded in illustration of the Invertebrates. A page of text faces each plate.

Four plates are devoted to each of the sub-kingdoms, Protozoa, Annuloida, Annulosa and Mollusea.

The Atlas exhibiting Vertebrates contains twenty-four plates, of which nine are devoted to the anatomy of fishes, the skate and cod being selected as illustrations; four plates to reptilian structure, with the salamander and tortoise for examples; four plates to the anatomy of birds, the pigeon forming the example, and seven plates to mammalism anatomy, with the rabbit for the example.

The plates are large enough in most cases to be exhibited to classes of moderate size, and the distinct color is largely designed to aid such a purpose.

For teachers of Zoölogy who make use of the blackboard, these atlases afford invaluable suggestions.

THE ELASTICITY AND RESISTANCE OF THE MATERIALS OF ENGINEERING. By William H. Burr, C. E. New York: John Wiley & Sons.

The author of this new work is the Professor of Rational and Technical Mechanics in the foremost engineering school in the country. The lectures prepared for the classes at the Rensselaer Polytechnic Institute form the basis of the present work.

There are two distinct parts to the treatise. Part I., Rational: presents the General Theory of Elasticity in Amorphous Solid Bodies—Thick, Hollow Cylinders and Spheres and Torsion—The Energy of Elasticity, and—Theory of Flexure.

Part II., Technical: Tension—Compression—Long Columns—Shearing and Torsion—Bending or Flexure—Connections—Miscellaneous Problems—Working Stresses and Safety Factors—The Fatigue of Metals—The Flow of Solids.

The first or analytical part is designed chiefly for students. Part II. exhibits the mathematical results subjected to the test of experiments.

The tables of this part, for which the work will be most widely sought by engineers, possess a value not usually belonging to compilations of this character, inasmuch as they have been prepared by a skilled instructor of engineering.

From the author's position and reputation as a writer on engineering subjects, it is safe to predict a wide demand for the book.

ELEMENTS OF SURVEYING AND LEVELING. By Charles Davis, LL.D. Revised by J. Howard Van Amringe, A. M., Ph. D. New York and Chicago: A. S. Barnes & Co.

If there is a better work than this on surveying, either for students or surveyors, our attention has not been called to it. The preceding edition is probably the best known work on the subject in this country. The present work is offered as a revision and enlargement suited to the newer demands of the engineering profession.

The editor, Dr. Van Amringe, is in a position to become fully aware of the wants to be satisfied, and eminently fitted to present the new subjects in a clear and logical manner. Every section has been carefully revised. Among the newly written chapters may be noted Trigonometrical Surveying, Topographical Surveying, and Mining Surveying. The more important revisions of the old matter relate to new methods of computing areas of land, volumes of earthwork, and the location of compound curves.

The Adjustment of the Transit, the Determination of the Meridian, and the Survey of Public Lands, are subjects materially extended.

An Appendix contains descriptions of the Solar Compass and Sextant, and of the method of their use.

The number of illustrations has been largely increased, and the typography exhibits the excellencies which distinguished the former editions.

DIXON'S MACHINISTS AND ENGINEER'S CALCULATOR.—The Machinists and Engineers' Practical Calculator; a compilation of useful rules and problems arithmetically solved, together with general information applicable to shop-tools, mill gearing, pulleys and shafts, steam boilers and engines; embracing valuable tables and instruction in screw cutting, valve and link-motion, etc., etc. By D. B. Dixon.

New York: D. Van Nostrand, Publisher, 27 Warren Street.

Of books for machinists and engineers there are no end, most of them professing to afford information upon points not generally known, and to give simple rules for arriving at the proportions of machinery and steam apparatus in general. Unfortunately, the compilers of these works address a comparatively small class in the community of workers, for they have, for the most part, locked up the knowledge they possess in algebraic formulæ, and advanced arithmetical problems, with the result of disappointing the very persons who most need the information. The mathematicians in any community are a small proportion of the whole, and among working men, in particular, there are very few who have any more than rudimentary knowledge of the first four rules of arithmetic. If they could make these available, and solve some rules by their aid, they would be encouraged to seek further, and in due time become expert with figures.

It is just in this respect that the average pocket-book for engineers and machinists disappoints the class who have a special need of it.

We are glad to find, in the work under notice, that the author is fully aware of the defects in pocket-books generally, for the use of those who have had limited educations, and he has so simplified standard rules that any young man, who can multiply and subtract, can avail himself of a great deal in mechanical engineering, which he has hitherto been debarred from. Moreover, the author instructs in mathematics; avoiding the jargon of schools, and the trade terms of text-book makers; he leads the willing pupil into a thorough knowledge of square and cube root, involution, evolution, and mensuration. Before he is aware that he is learning something the mysteries vanish, and the student finds that, by the aid of a competent teacher, who knows his needs, mathematics, so far as we have noted, is quite within his capacity. Many men have fancied that they were in some way mentally deficient, as regards the solution of problems, and have wondered why matters, which are simple to others, seemed so difficult to them. The trouble lies, in a majority of cases, wlth the methods by which they have attempted to learn, and not with their mental constitutions.

For example, in one book for engineers and machinists, we find the following rule for the size of a shaft to resist torsion, or twisting strains:

"The diameter of a cast iron shaft to transmit a certain horse-power, H, is to be multiplied by 400 and divided by the number of revolutions per minute, n, then extract the third root of the quotient; this gives the diameter in inches." The formula is

$$\frac{\sqrt[3]{400 \times H}}{n.}$$

This is simple enough to those who know all about it; but to the man who is building a water-wheel, and has not had the pleasure of the acquaintance of that mysterious zig-zag in front, the knowledge he seeks is as far off as ever. If he had Dixon's Pocket-Book, he would turn to it and find, under the head of torsion, this rule, p. 166:

"*To determine the diameter required for a given torsional stress.*

Rule. Multiply the twisting force in pounds by the length of the crank in feet, divide by 120, and the cube root of the product will be the diameter in inches."

When the millwright or working engineer sees this, he says: "Here is that terrible cube root again," but, as he has previously read the contents of the work mentioned, he finds that cube root and how to extract it is all laid down plain before him.

This wonderful process, he sees, is nothing but the first four rules of arithmetic after all, used in a certain sequence; and before he is aware of it he has learned it. We use this term—"before he is aware of it"—advisedly, because Mr. Dixon's methods of induction (leading up to a subject) are so clear that any one can understand and practice them.

We have been thus diffuse in our notice of Dixon's Pocket-book for the reason that it opens a new field to the working mechanic and engiheer. It is not only couched in simple arithmetical practice, as to its explanations, but it also instructs in the higher mathematical branches. In a sense, it might be called a mechanics' arithmetic, for, in combination with the various tables and rules for proportions of steam-boilers and engines, gears, screw-cutting, etc., etc., it is an arithmetic. For the class to whom it is chiefly addressed, it is a most valuable work and the most complete of its kind. When a mechanic has mastered it, he will find it a key to many other more advanced works and will derive many times its cost in knowledge he would otherwise acquire either very slowly or not at all. The price is $2.00, and we think all who aspire to be more than mere hewers of wood and drawers of water will send for a copy at once.—*The Mechanical Engineer.*

MISCELLANEOUS.

At a recent meeting of the Paris Academy of Sciences M. Loewy explained his new method for determining at any moment the relative position of the instrumental equator in relation to the real equator. This method is analogous to that already given for right ascensions, being founded on the observation of the stars near the pole, and on the variations in the relations of the co-ordinates due to the deflection of the instrument. M. Leowy demonstrates mathematically that his plan combines all the theoreticel and practical conditions required for the complete solution of the problem. It is based on the theorem here demonstrated that when the track described by a star in apparent distance from the pole coincides with its distance in relation to the instrumental plane, the angle may be exactly determined which is formed by the terrestial axis with the line of the instrumental poles, by means of the variation observed between the apparent polar

distance and the distance in relation to the instrumental plane. The method is independent of any possible variations in the state of the instrument during a period of twelve hours, and it excludes the cause of systematic error due to refraction. It is, moreover, capable of extreme accuracy, which, by multiplying the points, may be carried so far as desirable.—*Engineer*.

ROYAL TESTING ESTABLISHMENT, BERLIN.—A new journal has recently made its appearance which promises exceptionally well. It is devoted to information on testing materials, forms the official organ of the royal testing establishments in connection with the Berlin Polytechnic, and is edited by Dr. H. Wedding, whose name is a guarantee for the high standing of the new journal. The first number, which has just reached us, is a quarto journal of forty-eight pages containing a variety of information on the subject of tests of material, regulations for the Berlin laboratories for mechanical and chemical tests, the tests of building materials, &c. An elaborate article by Dr. Bohme, the assisant manager of the institution, deals with the most suitable form for the ends of test bars, and in it are illustrated some very ingenious ball sockets for holding the test-piece, the latter having only a straight collar at the end if round, or being widened if flat. Professor Finkenir, the manager, contributes an article on investigations of pig iron, dephosphorized by the basic process, while some exhaustive tests of cement and cement concretes, made by Dr. Bohme with a view of comparing German and Russian standards, are given in full. The journal has probably not a very wide field, but in the interest of scientific research, we trust it will prosper. Published by Julius Springer, of Berlin, it is quite up to the best standard of journals in typography and illustrations. The latter are excellent engravings on stone. The title of the journal is "Mittheilungen aus den konigl. Technischen Versuchsan-Stalton zu Berlin."

SOUTH AMERICAN TIMBER.—Some investigations by M. Thanneur show that South America is rich in woods for engineering purposes. The yandubay is exceedingly hard and durable; the couroupay is also very hard and rich in tannin. The quebracho is, however, more interesting than any, and grows abundantly in the forests of La Plata and Brazil. It resembles oak in the trunk, and is used for railway sleepers, telegraph poles, piles, and so on. It is heavier than water, its specific gravity varying between 1.203 and 1.333. The color at first is reddish like mahogany but grows darker with time. Being rich in tannin it is employed for tanning leather in Brazil, and has recently been introduced for that purpose into France. A mixture of one-third of powdered quebracho and two-thirds of ordinary tan gives good results.

A DESCRIPTION of the hektograph or gelatine pad now so extensively used for reproducing copies of letters is given by the *Glassware Reporter* as follows:—"An old French method of printing and transferring was to cast a sheet of glue, $\frac{1}{4}$in. thick, diluted, while warm, to such a consistence that when cool it was perfectly flexible and pliable as leather. The impression was first taken from the copper plate upon this sheet of glue, and then transferred to the article requiring decorating. The glue could be applied to the ware two or three times before it became necessary to take a fresh impression from the plate. Black printing in the Staffordshire potteries, was at one time done by a similar process, the gelatine bats being cast on dish bottoms, and then cut to the size required for the patterns. But this printing from bats has now fallen into disuse."

SOME varieties of South American wood have been described by M. Thanneur which seem likely to become valuable for engineering purposes. The yandubay is exceedingly hard and very durable. The couroupay is also very hard and very rich in tannin. It bears some resemblance to the quebracho, which is perhaps the most interesting of all and the most used. It is very abundant in Brazil and La Plata. Its diameter varies within the same limits as that of the oak, but the trunk is shorter. It is used for railway sleepers, telegraphic poles, piles, &c. It is very durable, especially when well seasoned. Its specific gravity is from 1.203 to 1.333. Its color is reddish, like mahogany, but it becomes darker in time. On account of its hardness it is difficult to work, and it cannot be readily cut with an axe, but it has been introduced into France on account of its richness in tannin.

PHOSPHOR-COPPER.—Mr G. Otto, of Darmstadt, has brought out a new combination of phosphorus, and copper, called phosphor-copper containing fifteen per cent. of phosphorus and intended for admixture with other metals. The favorable influence of phosphorus on copper and copper alloys is owing to its energetic reducing action on the oxygen which they contain, the result being extreme closeness of grain, combined with great elasticity. The new material may be added to copper alloys of all descriptions without any admixture of tin, such as brass, German silver, &c.; it can also be used in refining copper. It is supplied in tablets weighing about 2lb. each, 15lb. to 20lb. being sufficient for the production of one ton of phosphor-bronze, and only increasing the cost from one to three shillings per cwt. Most of the Continental government works have used phosphor-copper for a considerable time with most successful results, and it is now being introduced into this country by Mr. G. Hartmann, of 16, Great St. Helens, E. C. To produce homogeneity in castings, both of this material and also of other metals and alloys, the same inventor has devised a stirrer or agitator, which consists of a handle having at its lower extremity a dovetailed groove, into which is inserted a piece of chalk, marble or other mineral capable of giving off carbonic acid gas. When the metal is molten it is stirred with this instrument and large quantities of gas are liberated with great force, the result being that a thorough admixture of the metal is produced. It is claimed that all the gases previously present are expelled, and that no trace of the carbonic acid remains, while the latter accumulating on the surface, prevents the absorption of oxygen from the atmosphere.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXXX.—DECEMBER, 1883.—VOL. XXIX.

ON THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS.

AN INVESTIGATION OF THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS USED IN ENGINEERING CONSTRUCTION, CONDUCTED IN THE MECHANICAL LABORATORY OF THE DEPARTMENT OF ENGINEERING OF THE STEVENS INSTITUTE OF TECHNOLOGY.

By ESTEBAN DUQUE ESTRADA, M. E.

Contributed to Van Nostrand's Engineering Magazine.

II.

Details of Transverse Tests.

No. 1. *Caoba* was straight grained, and free from cracks or any other weakening defects. Broke at tension side, midway between the supports. Square rupture. First rupture occurring at 2,300 pounds, second at 1,600, third at 1,950 pounds, fourth at 800 pounds, when the total deflection was 1.25 inches.

No. 2. *Dagame* (J_2). Straight grained, very uniform color, and sound appearance. First rupture took place at 3,450 pounds at tension side, upper side unbroken; very fine, fibrous rupture, and directly under the cross-head. Second rupture at 3,350 pounds. The total number of ruptures were five, at the last of which the hand-wheel had been turned to its lowest position, the sustained load being 150 pounds, and the deflection at this point 7 inches.

No. 3. *Baria* (I_2) broke at tension side diagonally across with the grain. It sustained 1,200 pounds after the first rupture, increasing up to 1,325 pounds, when it broke suddenly in two, the deflection at this point being 1.847 inches.

No. 4. *Majaguilla* (L_3) broke at tension side, upper side compressed. Fibrous rupture. Sustained 3,200 pounds after the first; diminishing gradually in strength, and sustaining 100 pounds when the final deflection was seven inches.

No. 5. *Majagua* (D_1) broke at tension side with a fibrous rupture. After removal of the load the stick recovered remarkably well, a small deflection remaining. Straight grained and of sound condition.

No. 6. *Dagame* (J_3) broke at tension side, fibrous rupture occurring at 2,100 pounds, second at 2,250 pounds, third at 2,280 pounds, fourth at 2,000 pounds, fifth at 2,150 pounds, when the deflection was 4.75 inches. When the deflection was 5.75 inches the load sustained was 1,750 pounds, being held for more than five minutes, after which the load was removed and the set produced was $2\frac{1}{6}$ of an inch, thus showing it to be an excellent

material, very elastic, tenacious, and remarkably strong.

No. 7. *Jiqui* (E_2). First rupture at 3,300 pounds on the tension side, with a long crack. Continued to break gradually.

No. 8. *Jocuma Amarilla* (G_3) broke suddenly in two at 850 pounds. The deflection at the breaking point was 1.65 inches.

No. 9. *Majagua Azul* (D_2) broke first at tension side at 1050 pounds, and midways between the supports. Continued to break gradually, and when the load was 300 pounds the final deflection was 5.75 inches; fibrous fracture.

No. 10. *Ocuje.* First rupture at 1,000 pounds; second rupture at 880 pounds, being long and with the grain.

No. 11. *Cocuyo* (K_1) broke at 2,650 pounds, when the deflection was 1.25 inches. Eight more fractures followed, at the last of which the load sustained was 600 pounds and the deflection 4.25 inches; cross and fibrous rupture.

No. 12. *Baria* (I_3). First rupture at 2,575 pounds on tension side. Broke again at 1,100 pounds, then continued to break gradually at the middle, and with a fibrous rupture.

No. 13. *Yava* (H_2) broke at 1,325 pounds. Broke again at 600 pounds, when the deflection was 1.75 inches. Showed it to be very good in recovering its original form, even after rupture.

No. 14. *Yava* (H_1) broke at tension side at 1,125 pounds, the deflection at this point being 1.5 inches. Broke again at 200 pounds, the deflection then being 3.5 inches. Continued to break gradually. Whenever the load was removed the stick recovered remarkably well.

No. 15. *Guayacancillo* broke at 3,750 pounds, when the deflection was 2.45 inches. Remarkably good specimen, straight grained, and well seasoned. First rupture at tension side.

No. 16. *Caobilla.* First break at tension side at 3,000 pounds, with a deflection of 1.12 inches. Continued to break gradually. Very good specimen, well seasoned and free from knots or other defects.

No. 17. *Sabicu* (A_1) broke with the grain and diagonally across at 2,125 pounds, and a deflection of .875 inches; sudden rupture; good specimen.

No. 18. *Jiqui* (E_1) broke at 2,650 pounds, with a deflection of 1.36 inches. Three large knots in the specimen; one under the cross-head where rupture occurred. Specimen otherwise good.

No. 19. *Majaguilla* (L_2) broke at 2,250 pounds, with a deflection of 1.723 inches. Tension side first; very good and fibrous rupture. Continued to break gradually. Proved to be strong and exceedingly tough material.

No. 20. *Jacuma* (G_1) broke first at tension side at 800 pounds, and a deflection of 1.75 inches. Rupture not fibrous, but quite square and at the middle.

No. 21. *Jucaro Mastelero* (B_5) broke suddenly at a knot two inches from right hand support. Breaking load 1,150 pounds, deflection 2.25 inches. The same piece was tried again, making the distance between the supports equal to 32 inches. Loads were then applied, and it broke at 1,500 pounds, with a deflection of 1.75 inches. Continued to break gradually, and when the sustained load was 100 pounds the deflection was 4 inches.

No. 22. *Sabicu* (A_5) broke at 1,750 pounds, with a deflection of one inch. Sudden rupture, and diagonally across with the grain; good specimen.

No. 23. *Cocuyo* (K_3) broke at 2,300 pounds, the deflection being 1.63 inches. Continued to break very gradually. Good specimen.

No. 24. *Jucaro Prieto* (B_2) broke at 1,675 pounds, with a deflection of 0.85 inches. Continued to break, and at the last break the total deflection was 6 inches. Remarkably tough wood.

No. 25. *Quiebra-hacha* (C_1) broke at 1,375 pounds, with a deflection of 1.32 inches; not fibrous rupture, but square across the grain.

Thus, it may be observed from the preceding, that with the exception of the Sabicu and the Jacuma Amarilla, all the woods tested broke by gradual rupture, the Dagame, Jucara Prieto, Jucaro Mastelero, Majagua and the Majaguilla proving to be the best of all the woods tested, so far as toughness, elasticity and strength are concerned.

The following tables exhibit the data and "Modulus of Rupture," obtained by transverse stress.

They were the same pieces which had previously been used to determine the "Modulus of Elasticity."

The "Modulus of Rupture" was reduced from the formula $R = \frac{3\,P\,l}{2\,b\,d^2}$ in which P represents the breaking load, l the distance between the supports, b and d the breadth and depth of the test-piece respectively.

TRANSVERSE STRESS.

Wood.	Distance between supports in inches.	Breadth in inches.	Depth in inches.	Modulus of Rupture $R=\frac{3}{2}\frac{Pl}{bd^2}$	Modulus of elasticity per formula $E=\frac{Pl^3}{4\,\Delta\,bd^3}$	Load producing rupture.	Total deflection in inches.	Weight of a cubic foot in pounds.
Baria (I_1)	36	1.50	2.00	14850	1794461	1650	1.197	48.6
Baria (I_3)	36	2.10	2.12	13127	1620000	2575	1.436	48 6
Caoba	28	1.72	2.65	8049	1429166	2300	0.399	53.0
Caobilla	30	1.46	2 50	15000	1849315	3000	1.120	49.9
Cocuyo (K_1)	36	2.15	2.04	15900	2310469	2650	1.250	71.7
Cocuyo (K_3)	40	2.16	2.00	15967	2130000	2300	1.630	71.7
Dagame (J_2)	40	1.94	2.28	20700	2280820	3450	1.938	56.1
Dagame (J_3)	40	1.50	2.03	21000	2361620	2100	1.875	56.1
Guayacancillo	32	1.50	2.59	18000	1503561	3750	2 450	67.3
Jocuma Am. (G_1)	30	1.55	1.54	15483	2250000	800	1.750	64.8
Jocuma Am (G_2)	36	1.47	1.61	16392	2300000	850	1.650	64.8
Jucaro Prieto (B_3)	28	1.50	1.87	17400	1995636	2175	0.725	67.3
Jucaro Prieto (B_2)	28	1.45	1.75	15844	2195200	1675	0.825	67.3
Jucaro Mast (B_5)	38	1.56	1.50	18727	2500000	1150	2.250	55.5
Tiqui (E_2)	40	1.90	2 35	19800	2490272	3300	1.233	74.8
Tiqui (E_1)	32	1.82	2.00	17451	2520912	2650	1.370	74.8
Majagua (D_1)	40	1.65	1 88	17436	1939393	1450	2.063	43.9
Majagua (D_3)	36	1.98	2.12	15000	2000000	2500	1.500	43.9
Majaguilla (L_3)	36	2.25	2.31	16200	2257548	1600	1.387	69.2
Majaguilla (L_2)	40	1.66	2.12	18000	2000000	2250	1.723	69.2
Ocuje	30	1.38	1.64	12161	1500000	1000	1.250	52.4
Sabicu (A_1)	24	1.22	1.82	19125	2486070	2125	0.875	59.2
Sabicu (A_5)	32	1.25	1.76	22000	2400000	1750	1.000	59.2
Qiuebra-hacha (b_1)	36	1.41	1.75	18562	2100000	1375	1.320	81.1
Yava (H_1)	40	1.51	1.54	18750	2322463	1125	1.490	54.9
Yava (H_2)	32	1.50	1.51	18596	2393258	1325	1.500	54.9

TABLES OF DEFLECTIONS AND PERMANENT SETS.

YAVA (H_1).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.063	0.063		350	0.435	0.062	
100	0.125	0.062		400	0.498	0.063	0.002
150	0.187	0.062		450	0.560	0.062	
200	0.249	0.062	0.0005	500	0.622	0.062	
250	0.311	0.062		550	0.684	0.062	
300	0.373	0.062		600	0.746	0.062	0.006

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
650	0 808	0.062		950	1.205	0.075	
700	0.871	63		1000	1.280	0.075	
750	0.936	65		1050	1.370	0.090	
800	0.998	62	0.012	1100	1.490	0.120	
850	1 061	63		1125			
900	1.130	70					

FIQUI (E_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.013	0.013		750	0.183	0.013	
100	0.027	0.013		800	0.195	0.012	0.003
150	0.037	0.011		850	0.207	0.012	
200	0.049	0.012	0.0005	900	0.220	0.013	
250	0.063	0.014		950	0.233	0.013	
300	0.074	0.011		1000	0.245	0.012	0.005
350	0.085	0.011		1050	0.257	0.012	
400	0.097	0.011	0.001	1100	0.271	0.014	
450	0.110	0.012		1150	0.284	0.013	
500	0.122	0.013		1200	0.297	0.013	0.008
550	0.134	0.012		1250	0.310	0.013	
600	0.146	0.012	0.002	1300	0.323	0.013	0.010
650	0 158	0.012		1350	0.337	0.014	
700	0.170	0.012		1400	0.350	0.013	0.013

FIQUI (E_2)—CONTINUED.

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
1450	0.363	0.013		2400	0.621	0.016	
1500	0.377	0.014		2450	0.637	0.016	
1550	0.389	0.012		2500	0.654	0.017	
1600	0.400	0.011	0.015	2550	0.671	0.017	
1650	0.413	9.013		2600	0.687	0.016	
1700	0.425	0.012		2650	0.703	0.016	
1750	0.438	0.013		2700	0.722	0.019	
1800	0.451	0.013	0.018	2750	0.739	0.017	
1850	0.463	0.012		2800	0.761	0.022	
2900	0.475	0.012		2850	0.782	0.021	
2950	0.488	0.013		2900	0.804	0.022	
2000	0.502	0.014	0.020	2950	0.827	0.023	
2050	0.518	0.014		3000	0.857	0.030	
2100	0.530	0.012		3050	0.888	0.031	
2150	0.545	0.015		3100	0.919	0.031	
2200	0.561	0.016	0.024	3150	0.989	0.070	
2250	0.575	0.014		3200	1.058	0.078	
2300	0.599	0.015		3250	1.143	0.085	
2350	0.605	0.015		3300	1.233	0.090	

MAJAGUILLA (L_3).

Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.010	0.010		1450	0.302	0.015	
100	0.019	0.009		1500	0.314	0.012	0.023
150	0.028	0.009		1550	0.325	0.011	
200	0.037	0.009		1600	0.338	0.013	0.026
250	0.045	0.008		1650	0.350	0.012	
300	0.057	0.012	0.001	1700	0.364	0.014	0.040
350	0.065	0.008		1750	0.375	0.011	
400	0.074	0.009	0.002	1800	0.388	0.013	0.045
450	0.085	0.011		1850	0.405	0.017	
500	0.094	0.009	0.003	1900	0.418	0.013	0.050
550	0.103	0.009		1950	0.432	0.014	
600	0.113	0.010	0.004	2000	0.445	0.013	0.053
650	0.123	0.010		2050	0.455	0.010	
700	0.133	0.010	0.005	2100	0.472	0.017	
750	0.143	0.010		2150	0.490	0.018	
800	0.153	0.010	0.006	2200	0.508	0.018	0.067
850	0.163	0.010		2250	0.526	0.018	
900	0.175	0.012	0.009	2300	0.546	0.020	
950	0.188	0.013		2350	0.565	0.019	
1000	0.198	0.010	0.012	2400	0.584	0.019	0.080
1050	0.208	0.010		2450	0.604	0.020	
1100	0.218	0.010	0.014	2500	0.624	0.020	
1150	0.233	0.015		2550	0.645	0.021	
1200	0.243	0.010	0.017	2600	0.663	0.018	
1250	0.253	0.010		2650	0.683	0.020	
1300	0.265	0.012	0.021	2700	0.704	0.021	
1350	0.275	0.010		2750	0.724	0.021	
1400	0.287	0.012	0.022	2800	0.747	0.022	
2850	0.768	0.021		3250	0.990	0.035	
2900	0.790	0.022		3300	1.030	0.040	
2950	0.813	0.023		3350	1.076	0.046	
3000	0.838	0.025		3400	1.126	0.050	
3050	0.863	0.027		3450	1.176	0.050	
3100	0.890	0.031		3500	1.233	0.057	
3150	0.920	0.030		3550	1.293	0.060	
3200	0.955	0.035		3600	1.387	0.084	

(DAGAME T_2).

Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.016	0.013		1450	0.452	0.016	
100	0.031	0.016	0	1500	0.470	0.018	
150	0.046	0.015		1550	0.485	0.015	
200	0.061	0.015	0	1600	0.506	0.021	0.030
250	0.076	0.015		1650	0.523	0.017	
300	0.091	0.015	0.001	1700	0.540	0.017	
350	0.105	0.014		1750	0.560	0.017	
400	0.120	0.015	0.002	1800	0.576	0.020	
450	0.136	0.016		1850	0.597	0.016	
500	0.151	0.015	0.003	1900	0.613	0.020	0.041
550	0.166	0.015		1950	0.635	0.022	
600	0.181	0.015		2000	0.655	0.020	
650	0.196	0.015		2050	0.678	0.023	
700	0.212	0.016	0.006	2100	0.702	0.024	
750	0.227	0.015		2150	0.725	0.023	
800	0.242	0.015		2200	0.750	0.025	
850	0.258	0.016		2250	0.775	0.025	
900	0.274	0.016	0.009	2300	0.80[illegible]	0.025	
950	0.290	0.016		2350	0.826	0.026	
1000	0.306	0.016		2400	0.851	0.025	
1050	0.322	0.016		2450	0.879	0.028	
1100	0.338	0.016	0.014	2500	0.906	0.027	
1150	0.354	0.016		2550	0.936	0.030	
1200	0.370	0.016		2600	0.971	0.035	
1250	0.386	0.016		2650	1.006	0.035	
1300	0.402	0.016	0.017	2700	1.046	0.040	
1350	0.419	0.017		2750	1.086	0.040	
1400	0.436	0.017		2800	1.128	0.042	
2850	1.172	0.044		3200	1.549	0.063	
290[illegible]	1.217	0.045		3250	1.619	0.070	
2950	1.267	0.050		3300	1.693	0.074	
3000	1.319	0.052		3350	1.773	0.080	
3050	1.371	0.052		3400	1.858	0.085	
3100	1.426	0.055		3450	1.938	0.080	
3150	1.486	0.060					

BARIA (I_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.027	0.027		900	0.486	0.029	
100	0.054	0.027		950	0.515	0.029	
150	0.081	0.027		1000	0.545	0.030	0.018
200	0.108	0.027	0.0015	1050	0.575	0.030	
250	0.133	0.025		1100	0.615	0.040	
300	0.159	0.026	0.003	1150	0.650	0.035	
350	0.184	0.025		1200	0.683	0.033	0.037
400	0.210	0.026	0.004	1250	0.725	0.048	
450	0.239	0.029		1300	0.780	0.055	
500	0.265	0.026	0.006	1350	0.820	0.040	
550	0.290	0.025		1400	0.862	0.042	0.068
600	0.316	0.026	0.008	1450	0.922	0.060	
650	0.345	0.029		1500	0.987	0.065	
700	0.371	0.026	0.009	1550	1.057	0.070	
750	0.400	0.029		1600	1.127	0.070	
800	0.427	0.027	0.010	1650	1.197	0.070	
850	0.457	0.030					

JUCARO PRIETO (B_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.016	0.016		900	0.325	0.020	0.020
100	0.032	0.016		950	0.345	0.020	
150	0.050	0.018		1000	0.365	0.020	0.026
200	0.068	0.018	0.001	1050	0.388	0.023	
250	0.086	0.018		1100	0.414	0.026	0.035
300	0.102	0 016	0.002	1150	0.442	0.028	
350	0.119	0.017		1200	0.470	0.028	0.044
400	0.137	0.018	0.003	1250	0.500	0.030	
450	0.154	0.017		1300	0.530	0.030	0.060
500	0.172	0.018	0.005	1350	0.560	0.030	
550	0.189	0.017		1400	0.600	0.040	0.084
600	0.207	0.018	0.007	1450	0.640	0.040	
650	0.226	0.019		1500	0.680	0.040	0.104
700	0.245	0.019	0.011	1550	0.725	0.045	
750	0.265	0.020		1600	0.775	0.050	0.145
800	0.285	0.020	0.015	1650	0.825	0.050	
850	0.305	0.020		1675			

MAJAGUA AZUL (D_1).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0 038	0.038		800	0.629	0.045	
100	0.075	0.037		850	0.678	0.049	
150	0.113	0.038		900	0.729	0.049	0.031
200	0.150	0.037	0.002	950	0.790	0.063	
250	0.187	0.037		1000	0.840	0.050	
300	0.225	0.038		1050	0.900	0.060	
350	0.263	0.038		1100	0.965	0.065	
400	0.301	0.038	0.006	1150	1.050	0.085	
450	0.340	0.039		1200	1.133	0.083	
500	0.379	0.039		1250	1.243	0.110	
550	0.418	0.039		1300	1.373	0.130	
600	0.458	0.040	0.013	1350	1.513	0.140	
650	0.498	0.040		1400	1.713	0.200	
700	9.540	0.042		1450	2.067	0.350	
750	0.584	0.044					

CAOBA.

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.006	0.006		1200	0.165	0.010	0.009
100	0.012	0.006		1250	0.174	0.009	
150	0.020	0.008		1300	0.183	0.009	
200	0.026	0.006	0.0005	1350	0.191	0.008	
250	0.032	0.006		1400	0.198	0.007	0.017
300	0.038	0.006		1450	0.205	0 007	
350	0.045	0.006		1500	0.217	0.008	
400	0.051	0.007	0.001	1550	0.222	0.009	
450	0.057	0.006		1600	0.230	0.008	0.021
500	0.063	0.006	0.002	1650	0.240	0.010	
550	0.069	0.006		1700	0.250	0.010	
600	0.077	0.008	0 003	1750	0.260	0.010	
650	0.084	0.007		1800	0.270	0.010	
700	0.091	0.007	0.004	1850	0.280	0.010	
750	0.098	0.007		1900	0.290	0.010	
800	0.105	0.007	0.005	1950	0.300	0.010	
850	0.107	0.007		2000	0.310	0.010	0.035
900	0.119	0.007	0.006	2050	0.325	0.015	
950	0.126	0.007		2100	0.340	0.015	
1000	0.133	0.007	0.007	2150	0.350	0.010	
1050	0.140	0.007		2200	0.360	0 010	0.049
1100	0.148	0.008		2250	0.375	0.015	
1150	0.155	0.007	0.009	2300	0.394	0.019	

COCUYO (K_3).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.021	0.021		1200	0.535	0.030	0.025
100	0.042	0.021	0	1250	0.565	0.030	
150	0.063	0.021		1300	0.595	0.030	
200	0.084	0.021	0	1350	0.625	0.030	
250	0.105	0.021		1400	0.650	0.025	
300	0.126	0.021	0.003	1450	0.680	0.030	
350	0.147	0.021		1500	0.710	0.040	0.036
400	0.169	0.021	0.004	1550	0.750	0.040	
450	0.190	0.021		1600	0.790	0 040	
500	0.211	0.021	0.005	1650	0.830	0.040	
550	0.223	0.022		1700	0.870	0.040	
600	0.256	0.022	0.006	1750	0.910	0.040	
650	0.278	0.022		1800	0.950	0.040	
700	0.299	0.021	0.008	1850	0.990	0.040	
750	0.320	0.021		1900	1.040	0.050	
800	0.344	0.024	0.011	1950	1.090	0.050	
850	0.366	0.022		2000	1.150	0.060	
900	9.390	0.024		2050	1.210	0.060	
950	0 414	0.024		2100	1.270	0.060	
1000	0.434	0.020	0.016	2150	1.340	0.070	
1050	0.458	0.024		2200	1.420	0.080	
1100	0.482	0.024		2250	1.510	0.090	
1150	0.505	0.023		2300	1.630	1.020	

Experiments by Tension.

These experiments, as already stated, are somewhat unsatisfactory. It is clear that rupture in many instances did not occur by tension, but rather by shearing or splitting, which always gives a much lower result. The manner of breaking is illustrated in the last column of the table, page 449 :

Experiments by Compressive Stress.

The modulus of rupture for crushing was deduced from the formula $C=\frac{P}{K}$, in which P represents the crushing load, and k the sectional area of the test-piece, which in this case equals πr^2.

TESTS BY COMPRESSION.

WOOD.	Number of specimen.	Length in inches.	Diameter in inches.	Crushing loads in pounds.	Modulus of rupture per formula $C=\frac{P}{K}$	Mean value for C.
Baria	1	8.00	1.75	21500	9472	9472
Caoba	2	7.50	1.50	9500	5397	5397
Caobilla	3	6.25	1.25	11500	10087	10087
Cocuyo	4	8.00	1.75	21500	9472	9757
Cocuyo	5	8.00	1.75	22800	10043	
Dagame	6	8 00	1.75	26500	11674	11092.5
Dagame	7	7.50	1.50	18500	10511	
Guayacancillo	8	5.00	1.25	11500	10087	10262
Guayacancillo	9	5.00	1.25	11900	10438	
Jocuma Am	10	6.00	1.25	13000	11403	
Jocuma Am	11	6.00	1.25	12500	10973	10967
Jocuma Am	12	6.00	1.25	12000	10522	
Jucaro Prieto	13	7.00	1.30	17000	12883	
Jucaro Prieto	14	6.00	1.30	17000	12883	
Jucaro Prieto	15	5.00	1.30	17500	13258	13158
Jucaro Prieto	16	4.00	1.30	17500	13258	
Jucaro Prieto	17	3.00	1.30	18000	13600	
Jucaro Mastelero	18	6.00	1.25	12500	10973	10973
Jucaro Mastelero	19	6.00	1.25	12500	10973	
Jiqui Comun	20	8.00	1.75	32000	14096	
Jiqui "	21	7.00	1.50	21000	11931	
Jiqui "	22	2.50	1 25	12200	10701	12314
Jiqui "	23	8.00	1.75	29500	13000	
Jiqui "	24	7.00	1.25	13500	11843	
Majagua Azul	25	7.50	1.50	14500	8238	8504
Majagua Azul	26	6.00	1.25	10000	8771	
Majaguilla	27	5.00	1.50	15250	8664	
Majaguilla	28	2 50	1.50	16500	9378	9044
Majaguilla	29	7.00	1.50	16000	9090	
Ocuje	30	6.00	1.25	9900	8684	8684
Quiebra-hacha	31	5.00	1.25	16500	14473	13596
Quiebra-hacha	32	5.00	1.25	14500	12719	
Sabicu	33	6.00	1.25	11500	10087	10043
Sabicu	34	6.00	1 27	12500	10000	
Yava	35	7 50	1.50	20800	11818	
Yava	36	5 00	1.25	14500	12719	12618
Yava	37	6.00	1.25	14500	12719	

EXPERIMENTS BY TORSIONAL STRESS.

The test-pieces used in these experiments were carefully turned in an engine lathe, and great care was taken to secure uniformity of size.

In these tests the pendulum bob was removed from the machine, and it was therefore necessary to standardize the machine in order to ascertain the modifications made by this change.

It was found that the maximum moment of the pendulum arm, *i. e.*, its moment when in a horizontal position, was equal to 24 pounds multiplied by 4.17 feet, which is the distance from the axis of rotation to the bolt hole. The maximum height of pencil when the arm was horizontal was 4.95 inches.

Therefore $(24\times4.17)\div4.95=20$ will give the stress in foot pounds for each inch of ordinate, on the diagram. Each inch of abscisa equals 10° of twist. The resistance to friction was found to be very small, and may be taken as $\frac{1}{2}$ foot-pound.

The modulus of torsional elasticity is found from the formula $G=\frac{PAl}{\theta I p}$ in which P represents the twisting force in foot pounds, A the arm of P, l the length of neck of test-piece in inches, θ the angle of torsion within the elastic limit, and Ip the polar moment of inertia, which, for a rectangular section, equals $\frac{1}{2}\pi r^4$.

TENSION TESTS.

Wood.	Laboratory number.	Breaking loads in pounds.	Modulus of rupt'e per form'a $T=\frac{P}{K}$	Kind of rupture.
Baria	1	4000	8000	Irregular.
Caoba	2	5200	10400	Square break
Caobilla	3	5600	11200	Irregular.
Cocuyo	4	6000	12000	Detrusive,
Dagame	5	6500	13000	Irregular.
Guayacancillo	6	8000	16000	"
Jocuma	7	1500	3000	Detrusive.
Jucaro Prieto	8	7600	15200	Square.
Jucaro Mast'lero	9	6000	12000	"
Jiqui Comun	10	7500	15000	Detrusive.
Majagua Azul	11	5800	11600	Irregular.
Majaguilla	12	6000	12000	Nearly sq'are
Ocuje	13	2000	4000	Detrusive.
Sabicu	14	7000	14000	Irregular.
Quiebrahacha	15	5600	11200	Nearly sq'are
Yava	16	5000	10200	" "

TORSION TESTS.

Wood.	Angle.		Stress in torsion foot pounds.		Area in square inches.	
	At elastic limit.	Final.	At elastic limit.	At maximum ordinate.	Up to the elastic limit.	Total
Baria	5°	237°	16	18	.40	
Caoba	10°	305°	12	12	.40	
Caobilla	8°	115°	14	14	.45	
Cocuyo	14°	260°	12	16	.70	
Dagame	15°	341°	16	26	.90	
Guayacancillo	6°	254°	16	18	.35	
Jocuma	8°	172°	16	16	.40	
Jucaro Prieto	7°	281°	20	20	.45	
Jucaro Mastelero	10°	290°	18	20	.60	
Jiqui Comun	9°	280°	22	22	.85	
Majagua	3°	348°	12	12	.15	
Majaguilla	11°	305°	16	18	.80	
Ocuje	11°	220°	16	16	.50	
Sabicu	7°	320°	18	18	.40	
Quiebra-hacha	8°	235°	26	26	.50	
Yava	6°	235°	12	14	.20	

Conclusions.

It appears from this investigation that the native woods of Cuba, of which the specimens experimented upon are good representatives, possess qualities which render them superior in every respect to the hard woods of the United States or Europe, not only on account of their greater hardness, closer grain, finer texture, and great durability, but also owing to their greater elasticity and strength, in which respects they are far better, as a rule, than even ash or oak. Their moduli of elasticity vary from 1,500,000 to 2,500,000, and is generally somewhat above 2,000,000. The modulus of rupture varies from 10,000 to 20,000, and averages about 18,000.

THE PROPORTIONS OF ARCHES, DERIVED FROM FRENCH PRACTICE.

Written for Van Nostrand's Magazine, by E. Sherman Gould, C. E.

Considered mathematically, the designing and calculating of arches form one of the most intricate and involved problems of applied mechanics; a fact abundantly proven by the numerous and conflicting theories of the arch which have been advanced, from time to time, by various and eminent mathematicians. Considered practically, however, the problem, at least so far as regards design, is greatly simplified by the many examples furnished by existing structures of all varieties and dimensions, which afford incontrovertible demonstration of the correctness of their proportions by safely fulfilling their intended duties, to say nothing of that other large class of non-existing structures which furnish the negative instruction to be derived from failure.

The purpose of this paper is to present as careful a digest as I have been able to prepare, of the actual practice of French engineers in regard to the proportioning of arches. The authorities which I have mainly used are: "*Routine de l'etablissment des routes*," by Dejardin, a work much quoted by French writers. "*Pratique de l'art de construire.*" by Claudel and Laroque, and a most excellent and

very recent volume, already noticed in this magazine, by Dubosque, on the theory and practice of the building of retaining walls, bridges and viaducts. The last two of these are themselves digests of the writings of many standard French authors. I may add, that these works were written by practical men, themselves engineers, or constructed and connected with many important public works in France.

In designing an arch, the first step is to determine the proper depth of keystone or thickness at the crown. The great Perronnet, of whose genius the magnificent Bridge of Neuilly is but one example among many, deduced from his own observations and practice a formula which, reduced to English measures, may be best written thus:

$$e=1+0.035\,S.$$

In this equation e=thickness of arch at crown in feet, S=span in feet.

A modern writer, Monsieur Lêveillé, *ingenieur en chef des ponts et chaussees*, has, from a careful study of the dimensions of existing structures, declared this formula to be of general application, and suited to arches of all forms, whether semi-circular, segmental, elliptical or false-elliptical, and even to railroad bridges and arches sustaining heavy surcharges of earth.

Another modern 1854 author, Monsieur Dejardin, *injenieur des ponts et chaussees*, whose death at an early age did not prevent him from connecting his name with more than one important structure, and who has left as a legacy to the profession the admirable practical treatise on arches named above, gives the following series of formulæ:

For a semi-circular arch,

$$e=1+0.1\,R$$

In which R=radius of the intrados.

For segmental arches, putting S=span, and V=ver-sine or rise, then;

when $\frac{v}{s}=\frac{1}{6}$; $e=1+0.05\,R$

" $\frac{v}{s}=\frac{1}{8}$; $e=1+0.035\,R$

" $\frac{v}{s}=\frac{1}{10}$; $e=1+0.02\,R.$

For elliptical, or false-elliptical, or "basket-handled" arches

when $\frac{v}{s}=\frac{1}{3}$; $e=1+0.07\,R,$

in which R equals the radius of curvature at the crown. In the case of the ellipse, R equals the minor axis.

Another approved authority, Monsieur Croizette,-Desnoyers, gives for semi-circular arches, and also segmental, etc., arches, when $\frac{v}{s}$ is greater than $\frac{1}{6}$, the general formula;

$$e=0.50+0.28\sqrt{2\,R}.$$

In the case of elliptical, or false-elliptical arches, R equals the radius of a fictitious arc of a circle, having a ver-sine equal to the rise of the arch under consideration.

In the case of segmental arches of low ver-sine and wide span, Monsieur Croizette-Desnoyers consider that the coefficient of the radical may be proportionally diminished, thus:

when $\frac{v}{s}=\frac{1}{6}$; $e=0.50+0.26\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{8}$; $e=0.50+0.24\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{10}$; $e=0.50+0.22\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{12}$; $e=0.50+0.20\sqrt{2\,R}$

It will be noticed that in none of the above formulæ does the character of the material used enter as a factor. Now it is evident that an arch built of granite voussoirs dressed to quarter-inch joints and laid up in Portland cement—to take a rather extreme case—would admit of much lighter dimensions than an ordinary brick arch laid up in common lime mortar.

To meet the case of exceptionally good materials and workmanship, another author, Monsieur Dupint, gives, for semicircular arches,

$$e=0.37\sqrt{s}$$

For segmental, etc.

$$e=0.28\sqrt{s}$$

in which s=span. In all the above formulas the dimensions are always in feet.

Among French practiciens, the formulæ of Dejardin seem to have received the most general acceptance for arches of moderate span. They give in many cases thicknesses which American engineers might consider excessive. It must be borne in mind, however, that the French quarries furnish a great deal of building-stone weighing only 125 lbs. and under, per cubic foot, with a resistance to crushing of but from 4,000 to 6,000 lbs. per square inch.

For general practice I should feel disposed to adapt Perronnet's formula.

(No. 1) $e=1+0.035\,s$

Not only on account of its simplicity, which is a small matter, for in the case of an important structure like a bridge one might well afford a few moments' additional time in working out a more complicated formula if there were anything to be gained by it, but because the results it gives tally so well with many existing structures. In substantiation of this I append a most instructive table, giving the dimensions, real and calculated, of a large number of bridges. It will be seen that the last two columns contain the thickness of the abutments. The calculated thickness of these is obtained by a formula given by Monsieur Léveillé, of which mention and use will be made later on. Two of the terms of this formula contain e, or the thickness of the crown. It is the calculated value of e that is used. The height of roadway above the extrados of the arch also enters one term of the formula. This is assumed at 2 feet in calculating the last column.

I have added the dimensions of two English bridges; that of Chester, and that carrying the Great Western Railway over the Thames at Maidenhead, not given in the original table. I have also added a column giving the thickness at the crown or depth of keystone according to Dupuit's formulæ

(*See Table on following page.*)

In the column of calculated thickness, in the above table, slight discrepancies in the decimals may be perceived from the fact that in most cases the meters of the original table were simply reduced to feet by multiplying by 3.3, and also because Perronnet's formula, as used by Léveillé, is modified by him, for the sake of round numbers, so as to give results very slightly less than in the form which I have given, and which is nearer the original.

I think the general agreement of results obtained by this formula with the dimensions of a number of wide span railroad bridges of tested security, as given by the above table, justifies confidence in it. Dupuit's formula is found to agree very well for semicircular arches of the comparatively short spans given in the table, while the divergence is quite wide in the other classes. It is noteworthy that his formula exactly checks the depth of keystone of the Chester bridge, which is perhaps the boldest of its kind extant.

Although Monsieur Léveillé considers his formula as giving a sufficient depth of keystone either for carrying heavy railroad traffic over a roadbed raised two feet above the extrados of the crown, or supporting a heavy surcharge of earth, it is proper to remark that other engineers consider it necessary to introduce a variable term in the case of an extra heavy surcharge, whether of earth or any other load. If it is considered necessary to augment the depth of keystone in such cases, we may borrow an expression from this variable term, from the German and Russian practice, and write our formula thus:

$$e=1+0.035+0.02\text{ H}.$$

H=height in feet of the earth embankment over the extrados at the crown. If the surcharge be of any other kind of loading, its weight is to be reduced to an equivalent height of earth embankment. This term can be added to any one of the formulæ used for determining the value of e.

As regards the question of surcharging an arch, it must be borne in mind that a great difference exists between a surcharge, properly so called, consisting of a high bank of earth, or other stationery and inert material, and the "live load" occasioned by, for instance, the passage of heavy trains. In the former case, the surcharge nearly increased the dead load, more or less evenly distributed over the arch; and while, up to a certain point, it augments the crushing pressure upon the arch sustaining it, on the other hand, *if there is enough of it and the abutments are sufficiently solid,* it effectually pre-

Designation of Bridge.	Span.	Rise.	Depth of keystone.			Height of abutment.	Thickness of abutment.	
			Act'al	Calculated.	Dupuit's formula.		Act'al	Calculated.
	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
1° *Segmental Arches.*								
Des Fruitiers, chemin de fer du Nord..	13.20	2.31	1.81	1.55	1.00	13.20	5.94	5.97
De Paisia........................	16.50	2.64	1.72	1.65	1.10	6.60	5.61	6.43
De Méry, chemin de fer du Nord.....	25.17	2.97	2.14	1.94	1.40	14.20	11.71	11.88
De Mélisey	37.60	4.95	1.98	2.34	1.70	11.71	17.61	15.44
De Couturette, at Arbois............	42.90	6.13	2.97	2.54	1.80	6.60	17.16	13.95
Over the Salat....................	46.10	6.27	3.63	2.64	1.90	20.49	19.14	20.00
De la rue des Abattoirs, at Paris, chemin de fer de Strasbourg..	52.90	5.11	2.97	2.87	2.00	12.96	33.00	23.89
Over the Forth, at Stirling..........	53.50	10.25	2.75	2.90	2.00	20.75	16.00	16.90
Saint-Maxence, over the Oise.......	77.20	6.40	4.80	3.65	2.50	27.85	38.94	40.10
Over the Oise, chemin de fer du Nord.	82.70	11.75	4.60	3.85	2.60	17.90	31.65	30.75
De Dorlaston	87.00	13.50	3.50	3.95	2.60	16.55	32.20	29.7
Grosvenor Bridge, Chester..........	200.00	42.00	4.00	8.00	4.00			
2° *Semi-Circular Arches.*								
Du crochet, chemin de fer de Paris à chartres....................	13.20		1.65	1.55	1.30	13.20	4.95	5.30
De Long-Sauts, chemin de fer de Paris à chartres................	16.50		1.81	1.65	1.50	9.90	5.90	5.85
D'Enghien, chemin de fer du Nord...	24.40		1.95	1.90	1.80	6.60	6.93	7.15
De Pantin, canal St. Martin..........	27.00		2.47	2.00	1.90	11.85	10.55	8.16
De la Bastille, canal St. Martin......	36.30		3.95	2.20	2.20	20.75	9.90	10.70
Des Basses-Granges, Orléans à Tours.	49.40		3.95	2.73	2.60	6.60	12.50	11.55
3° *Elliptical, or false-elliptical arches.*								
De Charolles.......................	19.80	7.55	1.95	1.75	1.25	1.30	5.25	5.25
Du Canal Saint-Denis...............	39.50	14.85	2.95	2.40	1.80	10.20	12.35	11.20
De Chateau-Thierry.................	51.30	17.10	3.75	2.75	2.00	13.65	15.00	13.90
De Dôle, over the Doubs............	52.40	17.50	3.75	2.80	2.00	1.35	11.85	12.85
Wellesley, at Limeric...............	70.00	17.50	2.00	3.45	2.30	12.00	16.50	21.30
D'Orléans, chemin de fer de Vierzon.	79.50	26.30	3.95	3.75	2.50	2.85	18.40	17.55
De Trilport.........................	80.70	27.80	4.45	3.75	2.50	6.40	19.30	20.40
De Mantes, over the Seine	115.20	34.60	6.40	5.00	3.00	3.20	28.90	28.50
De Neuilly, over the Seine..........	128.00	32.00	5.35	5.35	3.20	7.55	35 50	35.50
Maidenhead, Great Western Railway.	128.00	24.25	5.25	5.45	3.20			

vents the arch from yielding in any way save by direct crushing of the material of which it is composed. In this manner, under fitting conditions, heavy surcharging becomes an element of strength to an arch, neutralizing as it does the sometimes dangerous effects of a variable load, like that of a passing train which brings unbalanced strains upon the structure. Indeed, Captain Woodbury in his treatise upon the arch, maintains the proposition "that an arch impossible or impracticable without surcharge, may become perfectly safe when a load of sufficient depth has been added."

In a correctly proportioned arch, the thickness should increase from the keystone towards the haunches. Before showing how the proper increase is arrived at, I will give the formulæ by which the thickness of the abutments is obtained.

Monsieur Léveillé gives the following series of formula for the thickness of the abutments of railroad bridges, carrying

a horizontal surcharge of ballast raised two feet above the extrados of the key.

(No. 2) Semicircular arches; E=

$$(2+0.162\,S)\sqrt{\frac{h+0.25\,S}{H}\times\frac{0.865\,S}{0.25\,S+e}}$$

(No. 3) Segmental arches; E=

$$(1+0.212\,S)\sqrt{\frac{h}{H}\times\frac{s}{f+e}}$$

(No. 4) Elliptical and false elliptical;

$$E=(1.41+0.154S)\sqrt{\frac{h+0.54f}{H}\times\frac{0.84\,S}{0.465f+e}}$$

E=thickness of abutment below springing line.

s=span.

h=height of abutment, or distance between springing line and top of foundation.

e=depth of keystone.

f=rise. In a full-centered arch f=R.

H=vertical distance between top of roadway and top of foundations. It will be equal to $h+f+e+2$.

It is from these formulæ that the thickness of the abutments in the last column of the above table has been calculated. According to Monsieur L., it is not neccesary to augment the dimensions obtained by his formulæ for heavy surcharges. The best justification of his formulæ is the comparison shown by the table.

There is a much simpler formula, which I have adapted from German and Russian practice, which gives, for semicircular arches, values very near those given by Léveillé's. It is

(No. 5) $$E=1+0.04\,(5s+4h)$$

We will now examine the principles which govern the proportions of the other parts of an arch.

It is a matter of common observation that a semicircular arch can be carried up to a certain height above the springing line, without the support of centering. This height varies, of course, according to the nature of the materials used and the character of the workmanship. As a general rule, however, and probably in most cases well within the truth, we may take this height at half the rise of the arch, that is, half the radius with which the intrados is struck. Expressed in angular distance, this height corresponds with an angle of 30° from the horizontal, or 60° from the vertical. Owing to the self-supporting character of the arch below this point, the joint situated 30° above the springing line is called the *joint of rupture.* We may, therefore, consider the central portion, situated between the two joints of rupture, as the arch proper; all that portion lying below the joint of rupture on each side being regarded as forming part of the abutment. As the total angular amplitude of a full-center arch is 180°, the deduction of 30° from eacn side leaves a remainder of 120°. Dejardin says, therefore, "*There can be no arch beyond* 120°."

This method of finding the position of the joint of rupture by placing it at half the rise of the arch, is general; that is, it applies to segmental, elliptical and "basket-handled" arches as well as semicircular ones. In the case of a segmental arch of which the angular amplitude is equal to, or less than 120°, the joint of rupture is, of course, situated at the springing line. If its amplitude is greater than 120°, we imagine the full-center arch of same radius to be struck, and measure down from the crown of the intrados, a distance equal to half the radius. A horizontal line drawn through this point will then cut the curve of the intrados at the joint of rupture. Or, we may lay off 60° from the vertical passing through the crown of the arch and obtain the desired point in that way.

For elliptical and false-elliptical arches, the joint of rupture is also found at half the rise. Should the arch be incomplete, *i.e.*, if it is not carried down until the springing line is horizontal, it must be completed upon the drawing, and half the rise of the completed arch measured down from the crown. A horizontal line drawn through the point thus determined will, by its intersection with the intrados, indicate the position of the joint of rupture. It is very rarely, however, that incomplete elliptical arches are met with.

It is evident that in order to preserve an uniform resistance in all parts of the arch that its thickness should increase from the crown to the joint of rupture. There is a very convenient rule for proportioning this increase, which is, simply that *the length of each joint, between the*

joint of rupture and the crown, measured radically, should be such that its vertical projection is equal to the depth of keystone. Expressed as a formula, this rule would be stated thus:

$$l' = \frac{e}{\sin a'}$$

in which l'=the length, measured radially, of any joint, a'=the angle which such joint makes with the horizontal, and e=depth of keystone.

This rule enables us to at once determine the *length of the joint of rupture*, which we will designate by l. It can always be expressed in terms of e, that is, by the depth of keystone multiplied by a certain coefficient. Thus, we have for full centered arches, or segmental arches, when $\frac{v}{s}$ is equal to or greater than $\frac{1}{4}$,

$$l=2e. \qquad \text{(No. 6)}$$

For flatter segmental arches, Monsieur Croizette-Desnoyers has calculated the following series of coefficients:

$$\left.\begin{array}{llll} \text{For} & \frac{v}{s}=\frac{1}{6}; & l=1.40\,e \;.\;.\; & (a) \\ \text{“} & \frac{v}{s}=\frac{1}{8}; & l=124\,e \;.\;.\; & (b) \\ \text{“} & \frac{v}{s}=\frac{1}{10} & l=1.15\,e \;.\;.\; & (c) \\ \text{“} & \frac{v}{s}=\frac{1}{12} & l=1.10\,e \;.\;.\; & (d) \end{array}\right\} \text{No. 7}$$

For other ratios of $\frac{v}{s}$, the value of l may be determined by interpolation.

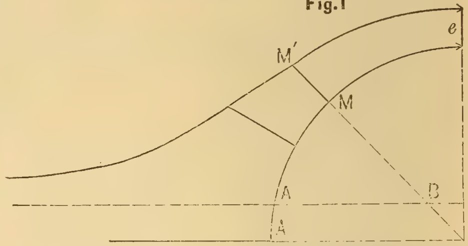
Monsieur Desnoyers also gives, for elliptical and "basket-handled" arches,

$$\left.\begin{array}{llll} \text{For} & \frac{v}{s}=\frac{1}{3}; & l=1.80\,e \;.\;.\; & (a) \\ \text{“} & \frac{v}{s}=\frac{1}{4}; & l=1.60\,e \;.\;.\; & (b) \\ \text{“} & \frac{v}{s}=\frac{1}{5}; & l=1.40\,e \;.\;.\; & (c) \end{array}\right\} \text{No. 8}$$

Having the depths of keystone and the length of the joints of rupture, it is now only necessary to connect their extremities in order to attain the curve of the extrados. To do this, and conform to the formula $l'=\frac{e}{\sin a'}$, Dejardin gives the following graphical method, for full-centered or segmental arches:

Let AO, Fig. 1, be the springing line of a full-centered arch. Draw the horizontal line O′A′ distant O′O=e from AO. Then the length of any joint MM′ is obtained by drawing OM produced, and making BM′=OM=R. This construction may be continued below the joint of rupture MM′, as is sometimes desirable in the case of contiguous arches. Below this joint the length rapidly increases, and finally an asymptote to the springing line produced is obtained.

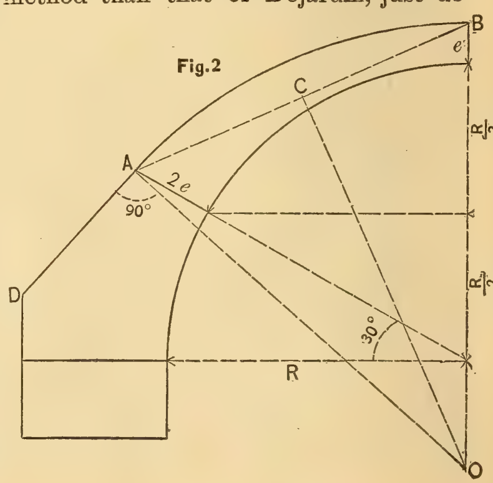
Fig. 1



We have the authority of Dejardin for stating, that *if the extrados of the arch proper, i.e., that portion of it comprised between the two joints of rupture, be drawn subject to the condition* $l'=\frac{e}{\sin a'}$, *the depth of keystone* e *being properly proportioned, the arch itself conforms strictly to the requirements of stability, and no further calculation is necessary to determine the fact.*

Monsieur Dubosque gives a more rapid method than that of Dejardin, just de-

Fig. 2

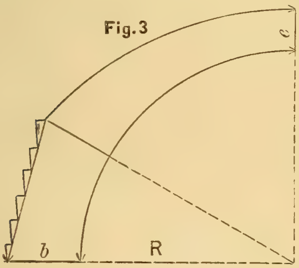


scribed, of striking the curve of the extrados between the joints of rupture, as follows: Join the outside extremity of the joint at the crown, and of the joint of rupture by the line AB (Fig. 2). Bisect AB in C. Draw CO perpendicular to AB, cutting the vertical BO in O. With the radius AO=BO, describe the arc AB, forming the required extrados. This method gives an intermediate thickness somewhat greater than does that of Dejardin.

Dubosque then finishes the exterior lines of his arch by drawing a tangent AD (Fig. 2) to the curve of the extrados, and producing it until it intersects at D with the back of the abutment. This construction is applicable to all kinds of raches, full-centered, segmental, etc.

Dejardin considers a full-centered arch as composed of three separate parts, namely the arch proper comprised between the two joints of rupture; an intermediate portion on each side of the center comprised between the joints of rupture and the springing line, and finally the abutments proper, comprising those portions of the structure lying between the springing lines and the top of the foundations.

Fig. 3



The first and third of these parts we have already considered. The intermediate parts are determined by Dejardin, by simply taking a distance b on the springing line (Fig. 3) equal to $\frac{1}{4}$ of the sum of the radius and twice the depth of keystone. Thus,

$$b=\frac{R+2e}{4}.$$

It should be understood, of course, that in this calculation e is determined by Dejardin's formula.

The extremity of the joint of rupture and of b are then joined by either a straight line, or offsets exterior to a straight line (Fig. 3). Should the radius be so small (less than 5 feet) that b thus determined is less than the horizontal projection of the joint of rupture, a perpendicular is dropped from its extremity upon the springing line.

I have given this method of Dejardin, simply to show how far, in the judgment of an approved authority, this portion of the arch may, with perfect safety, be reduced, as in special cases such reduction might be desirable. I think, however, that Dubosque's method, given above, will generally be preferred.

In order to exemplify the preceding rules, I will now give designs for four different arches, respectively, semi-circular, segmental, elliptical, and "basket-handled," being single-span railroad bridges, the level roadbed being in all cases 2 feet above the extrados of the arch at the crown.

First. A full-centered arch (Fig. 4), 50 feet span; abuments six feet high from top of foundation to springing line.

We have three dimensions only to determine by formula, viz., the depth of keystone, the thickness of the abutments, and the length of the joint of rupture. All the other dimensions will be determined by a purely graphical construction.

Substituting the given values in formulæ (1) and (2) we have,

$$e=1+0.035\times 50=2.75 \text{ ft.}$$

$$E=(2+0.162\times 50)\sqrt{\left(\frac{6+0.25\times 50}{6+25+2.75+2}\right)\times\left(\frac{0.865\times 50}{0.25\times 50+2.75}\right)}=(10.1)\sqrt{0.517\times 2.84}$$

$=12.22$ ft., say 12.25 ft.

Or, using the simplified formula No. 5,

$$E=1+0.04(5s+4h)=11.96 \text{ ft.}$$

which serves as a very good check upon the more complicated formula.

For the length of the joint of rupture we have, by substituting the value of e in formula (6),

$$l=2\times 2.75=5.5 \text{ ft.}$$

The curve of the extrados between the joint of rupture and the crown is struck in, as described above, and the tangent produced to its point of intersection with the back of the abutment produced, as also described, and the arch is complete. All the principal dimensions are given in the figure (Fig. 4).

joint of rupture and the crown, measured radically, should be such that its vertical projection is equal to the depth of keystone. Expressed as a formula, this rule would be stated thus:

$$l' = \frac{e}{\sin a'}$$

in which l'=the length, measured radially, of any joint, a'=the angle which such joint makes with the horizontal, and e=depth of keystone.

This rule enables us to at once determine the *length of the joint of rupture*, which we will designate by l. It can always be expressed in terms of e, that is, by the depth of keystone multiplied by a certain coefficient. Thus, we have for full centered arches, or segmental arches, when $\frac{v}{s}$ is equal to or greater than $\frac{1}{4}$,

$$l=2e. \qquad \text{(No. 6)}$$

For flatter segmental arches, Monsieur Croizette-Desnoyers has calculated the following series of coefficients:

$$\left.\begin{array}{llll} \text{For} & \frac{v}{s}=\frac{1}{6}; & l=1.40\,e \;.\;. & (a) \\ \text{"} & \frac{v}{s}=\frac{1}{8}; & l=124\,e \;.\;. & (b) \\ \text{"} & \frac{v}{s}=\frac{1}{10} & l=1.15\,e \;.\;. & (c) \\ \text{"} & \frac{v}{s}=\frac{1}{12} & l=1.10\,e \;.\;. & (d) \end{array}\right\} \text{No.7}$$

For other ratios of $\frac{v}{s}$, the value of l may be determined by interpolation.

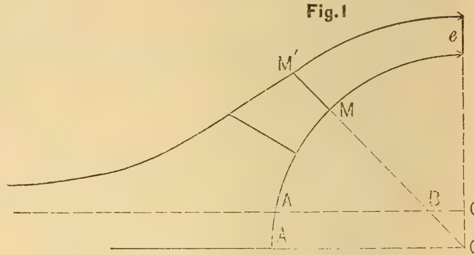
Monsieur Desnoyers also gives, for elliptical and "basket-handled" arches,

$$\left.\begin{array}{llll} \text{For} & \frac{v}{s}=\frac{1}{3}; & l=1.80\,e \;.\;. & (a) \\ \text{"} & \frac{v}{s}=\frac{1}{4}; & l=1.60\,e \;.\;. & (b) \\ \text{"} & \frac{v}{s}=\frac{1}{5}; & l=1.40\,e \;.\;. & (c) \end{array}\right\} \text{No. 8}$$

Having the depths of keystone and the length of the joints of rupture, it is now only necessary to connect their extremities in order to attain the curve of the extrados. To do this, and conform to the formula $l'=\frac{e}{\sin a'}$, Dejardin gives the following graphical method, for full-centered or segmental arches:

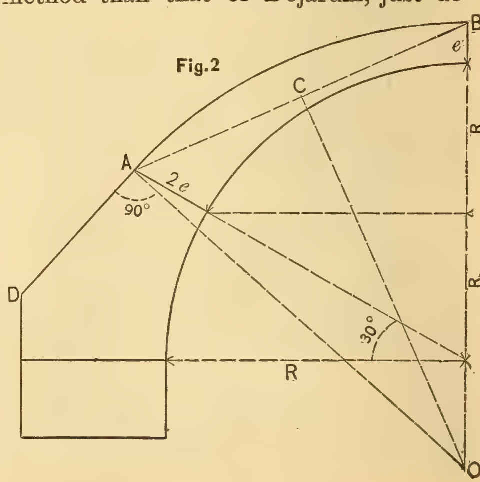
Let AO, Fig. 1, be the springing line of a full-centered arch. Draw the horizontal line O′A′ distant O′O=e from AO. Then the length of any joint MM′ is obtained by drawing OM produced, and making BM′=OM=R. This construction may be continued below the joint of rupture MM′, as is sometimes desirable in the case of contiguous arches. Below this joint the length rapidly increases, and finally an asymptote to the springing line produced is obtained.

Fig. 1



We have the authority of Dejardin for stating, that *if the extrados of the arch proper, i.e., that portion of it comprised between the two joints of rupture, be drawn subject to the condition* $l'=\frac{e}{\sin a'}$, *the depth of keystone* e *being properly proportioned, the arch itself conforms strictly to the requirements of stability, and no further calculation is necessary to determine the fact.*

Monsieur Dubosque gives a more rapid method than that of Dejardin, just de-

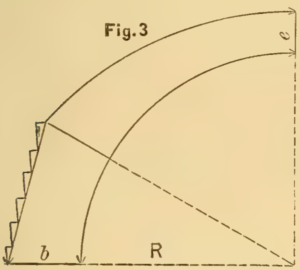


scribed, of striking the curve of the extrados between the joints of rupture, as follows: Join the outside extremity of the joint at the crown, and of the joint of rupture by the line AB (Fig. 2). Bisect AB in C. Draw CO perpendicular to AB, cutting the vertical BO in O. With the radius AO=BO, describe the arc AB, forming the required extrados. This method gives an intermediate thickness somewhat greater than does that of Dejardin.

Dubosque then finishes the exterior lines of his arch by drawing a tangent AD (Fig. 2) to the curve of the extrados, and producing it until it intersects at D with the back of the abutment. This construction is applicable to all kinds of raches, full-centered, segmental, etc.

Dejardin considers a full-centered arch as composed of three separate parts, namely the arch proper comprised between the two joints of rupture; an intermediate portion on each side of the center comprised between the joints of rupture and the springing line, and finally the abutments proper, comprising those portions of the structure lying between the springing lines and the top of the foundations.

Fig. 3



The first and third of these parts we have already considered. The intermediate parts are determined by Dejardin, by simply taking a distance b on the springing line (Fig. 3) equal to $\frac{1}{4}$ of the sum of the radius and twice the depth of keystone. Thus,

$$b=\frac{R+2e}{4}.$$

It should be understood, of course, that in this calculation e is determined by Dejardin's formula.

The extremity of the joint of rupture and of b are then joined by either a straight line, or offsets exterior to a straight line (Fig. 3). Should the radius be so small (less than 5 feet) that b thus determined is less than the horizontal projection of the joint of rupture, a perpendicular is dropped from its extremity upon the springing line.

I have given this method of Dejardin, simply to show how far, in the judgment of an approved authority, this portion of the arch may, with perfect safety, be reduced, as in special cases such reduction might be desirable. I think, however, that Dubosque's method, given above, will generally be preferred.

In order to exemplify the preceding rules, I will now give designs for four different arches, respectively, semi-circular, segmental, elliptical, and "basket-handled," being single-span railroad bridges, the level roadbed being in all cases 2 feet above the extrados of the arch at the crown.

First. A full-centered arch (Fig. 4), 50 feet span; abuments six feet high from top of foundation to springing line.

We have three dimensions only to determine by formula, viz., the depth of keystone, the thickness of the abutments, and the length of the joint of rupture. All the other dimensions will be determined by a purely graphical construction.

Substituting the given values in formulæ (1) and (2) we have,

$$e=1+0.035\times 50=2.75 \text{ ft.}$$

$$E=(2+0.162\times 50)\sqrt{\left(\frac{6+0.25\times 50}{6+25+2.75+2}\right)\times \left(\frac{0.865\times 50}{0.25\times 50+2.75}\right)}=(10.1)\sqrt{0.517\times 2.84}$$

$=12.22$ ft., say 12.25 ft.

Or, using the simplified formula No. 5,

$$E=1+0.04(5s+4h)=11.96 \text{ ft.}$$

which serves as a very good check upon the more complicated formula.

For the length of the joint of rupture we have, by substituting the value of e in formula (6),

$$l=2\times 2.75=5.5 \text{ ft.}$$

The curve of the extrados between the joint of rupture and the crown is struck in, as described above, and the tangent produced to its point of intersection with the back of the abutment produced, as also described, and the arch is complete. All the principal dimensions are given in the figure (Fig. 4).

Fig. 4

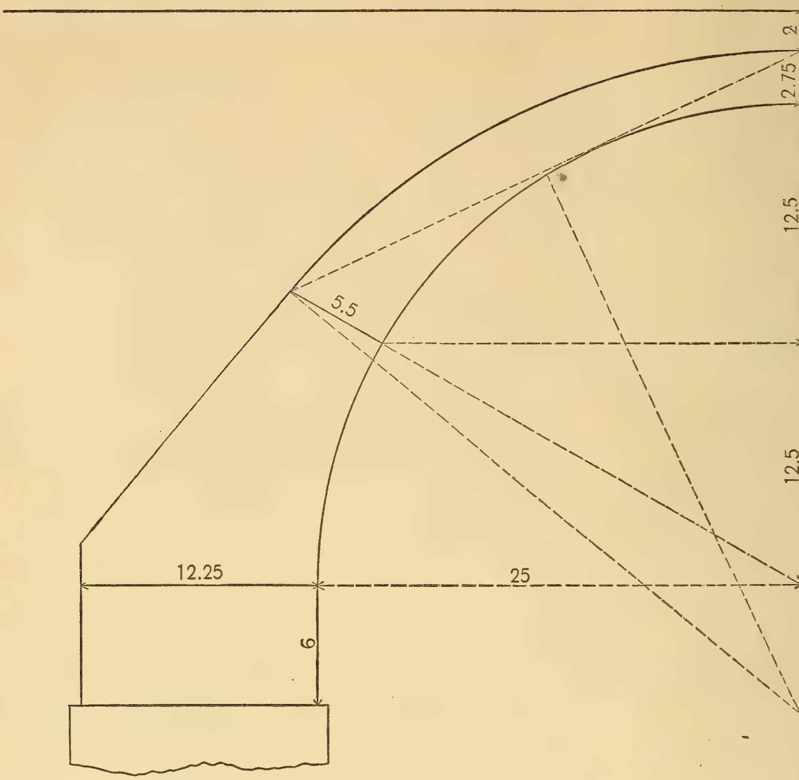
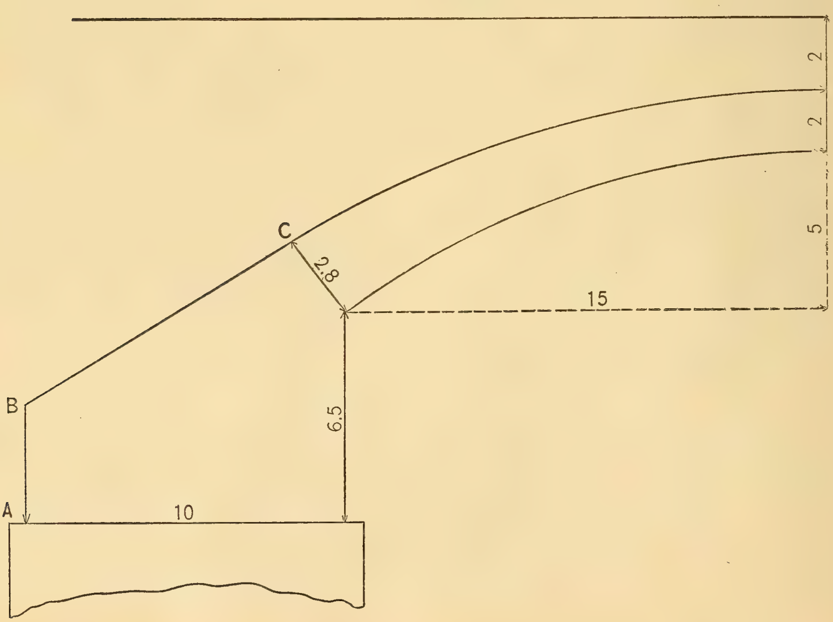


Fig. 5



Second. A segmental arch, 30 feet span, 5 feet rise; height of abutments, 6.5 feet (Fig. 5). Using formulæ (1) and (3) we have,

$$e=1+0.035\times 30=2.05 \text{ ft., say } 2 \text{ ft.}$$

$$E=(1+0.212\times 30)\sqrt{\left(\frac{6.5}{6.5+5+2+2}\right)\left(\frac{30}{5+2}\right)}$$

$$=7.36\sqrt{0.42\times 4.3}=9.86 \text{ ft, say } 10 \text{ ft.}$$

For the length of the joint of rupture, as $\frac{v}{s}=\frac{1}{6}$ we use formula (a) No. 7, which gives,

$$l=1.4\times 2=2.8 \text{ ft.}$$

As the arch in question is of less amplitude than 120°, that is, $\frac{v}{s}$ being smaller than $\frac{1}{4}$, the joint of rupture occurs at the springing line.

The curve of the extrados between the crown and the joint of rupture is struck in the same way as already described for a full-centered arch, and the outline is completed as for a full-centered arch, by producing the tangent of this curve beyond the joint of rupture until it intersects with the vertical back of the abutment.

Monsieur Dubosque remarks, in speaking of this class of arch, that the height AB should never be less than from 3 to 5 feet, so as to insure a square pressure against the bank. Should the tangent from the extremity of the joint of rupture strike lower than this, AB should be increased to about 4 feet, and B and C joined.

Third. An elliptical arch, major axis, or span, 30 ft.; semi-minor axis, or rise, 10 feet; height of abutment, 6.5 ft. (Fig. 6). Using formulæ (1) and (4) we have,

$$e=2 \text{ ft.}$$

$$E=(1.41+0.154\times 30)\sqrt{\left(\frac{6.5+0.54\times 10}{6.5+10+2+2}\right)\left(\frac{0.84\times 30}{0.465\times 10+2}\right)}$$

$$=6.03\sqrt{0.58\times 3.79}=8.92 \text{ ft., say } 9 \text{ ft.}$$

For the length of joint of rupture, since $\frac{v}{s}=\frac{1}{3}$, we have, formula (a) No. 8,

$$l=1.8\times 2=3.6 \text{ feet.}$$

We have now all the necessary data for our work, but before going into the detail of their application, it may be well to

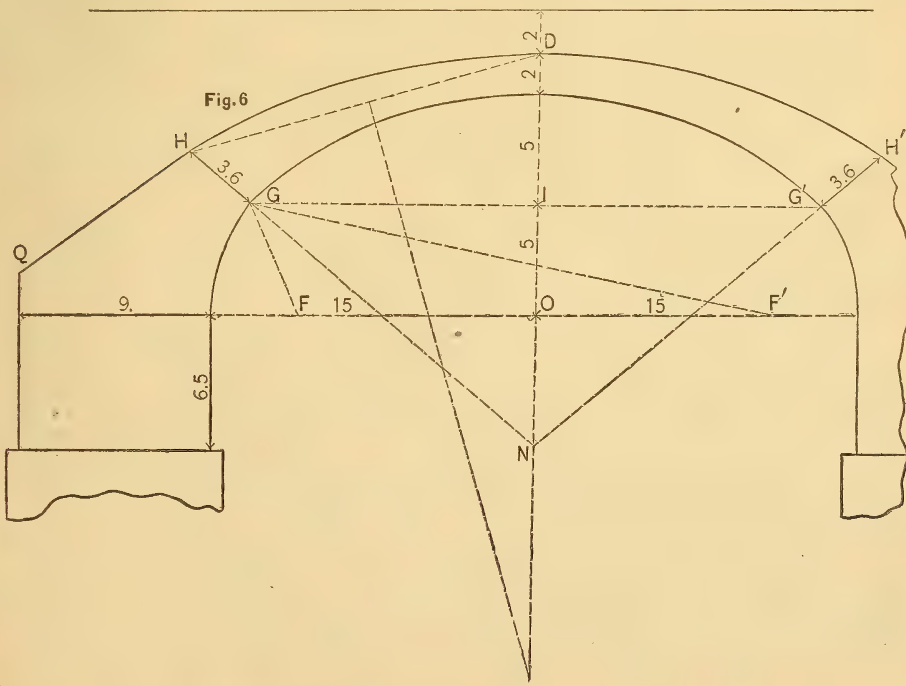
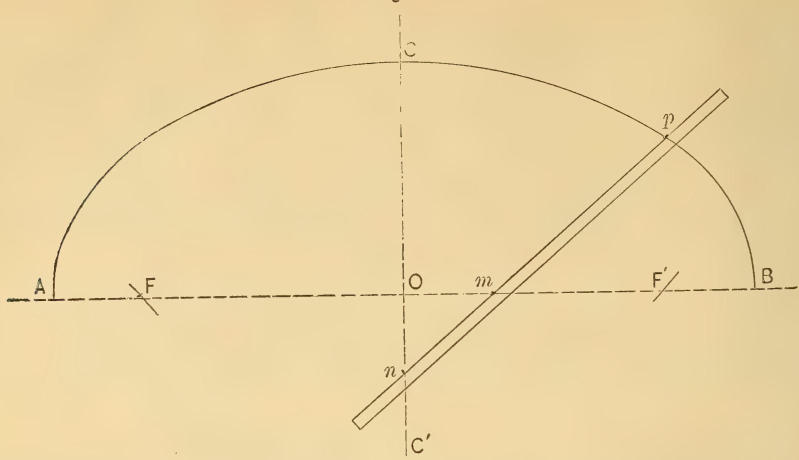


Fig. 7



describe the method given by Monsieur Dubosque for striking the curve, as follows: Draw the horizontal line AB (Fig. 7), and through O erect the perpendicular CC′. Lay off AB=major axis, and OC=semi-minor axis. Then take a rule, or strip of stiff paper with a true edge, and mark upon it a distance np=semi-major axis AO. Mark off, also, pm= semi-minor axis OC. The distance nm is then the difference of the two semi-axes. If now the rule or paper strip be moved in such a way that the point m is constantly upon the major axis, and the point n upon the minor axis, the point p will always indicate a point upon the ellipse. The curve being traced, from C as center with the radius OA, describe an arc cutting the major-axis at F and F′, which will be the foci of the curve.

The position of the joints of rupture GH, G′H′, is found by drawing the horizontal line GG′ at a distance OI= half the rise. It is an essential condition that these joints be drawn normal to the curve of the intrados. We therefore draw the radii rectors FG, F′G, to the point G, and bisect the angle FGF′ by the line GN, on which, produced, we take GH=3.6 feet, the length of the joint of rupture. G′H′ is similarly obtained. We have now the three points HDH′ of the extrados, which we may join by an arc of a circle obtained as for the other arches, as has been done in Fig. 6. An elliptical extrados involves a somewhat complicated drawing, and appears to offer no practical advantage.

The tangents HQ and H′Q′ are then drawn to their intersections Q and Q′, with the vertical backs of the abutments, and our drawing is complete.

Fourth. A false ellipse, struck from three centers; 30 feet span, 10 feet rise; height of abutments, 6.5 feet (Fig. 8).

Arches of which the intrados are false ellipses, called by the French "basket-handled" arches, are struck with an odd number of circular arcs, the centers of those next the haunches being situated upon the horizontal line passing through the two springing lines. It is an essential condition that these several arcs of circles should be tangent to each other at their points of junction. It is the fulfillment of this condition which constitutes the difficulty of correctly drawing the intrados of such arches when, besides, a fixed rise must be adhered to.

I will first describe the method of Monsieur Michal, by which the intrados of the above 3-centered arch may be struck.

Upon AB, as diameter, describe the semicircle AcB, which trisect in m and m'. Join Bm, mc, cm', m'A. Through C draw CM parallel to mc, intersecting Bm in M. CM′ is similarly drawn parallel to cm' intersecting Am' in M′. Draw the radii mO, m'O, and through the points M and M′ draw MO′, M′O′, parallel to mO and m'O, cutting the vertical axis CO, produced, in O′, and the horizontal line AB in D and D′. Then, from D and D′ as centers, with the radius DB=DM=D′A=D′M′, describe the arcs BM, AM′, and from O′ as center

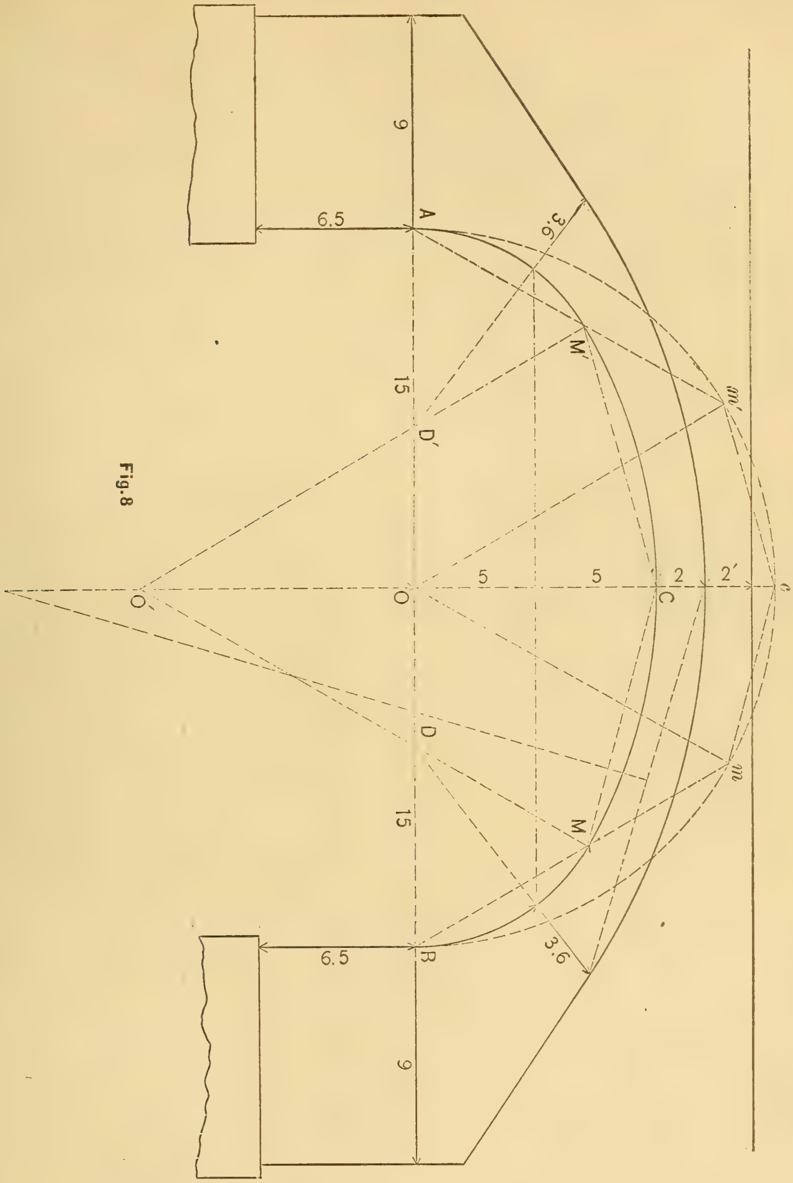


Fig. 8

with the radius O′M=O′M′, describe the arc MCM′.

We will now establish the governing dimensions of the arch, using formulæ (1), (4), and (*a*) No. 8, from which we obtain,

$$e=2 \text{ ft.}$$
$$E=9 \text{ ft.}$$
$$l=3.6 \text{ ft.}$$

The joint of rupture is situated at the distance of half the rise above the springing line.

The extrados is formed by a circular arc, as already described, between the two joints of rupture, and tangents intersecting the vertical backs of the abutments.

A full discussion of basket-handled arches, and the various methods of drawing them, would be a very long affair. I will only remark that a three-centered

arch is applicable only to cases where the ratio $\frac{v}{s}$ is not less than $\frac{1}{3}$. When it falls below $\frac{1}{3}$, more centers should be used.

Monsieur Dubosque gives the following formulæ for calculating the radius of the two smaller arcs, and of the larger central one, as follows:

$$r=a-1.366\ (a-f),$$
$$R=2a-r=a+1.366\ (a-f),$$

in which r=radius of smaller arcs; R=radius of larger arc; a=half span. f=rise. If we make $f=\frac{2a}{3}$, we have,

$$r=0.2725S,$$

$$R=0.7280S,$$

S being the span of the arch. These ratios are not far from $\frac{1}{4}$S and $\frac{3}{4}$S.

THE TRANSMISSION OF ENERGY.

By Prof. Osborne Reynolds, M.A., F.R.S.

From the "Journal of the Society of Arts."

Some few days ago, during a conversation with a friend, I remarked that I was going to give some lectures at the Society of Arts upon the transmission of energy, whereupon my friend inquired—"Is that the transmission of energy by electricity?" To this I replied, "No." The fact is, that I have heard so much about electricity that I began to think it was time to recall attention to the fact that there are other means of performing mechanical operations.

I am not sure whether, during the various lectures which have been given in this room on electricity, the actual term, transmission of energy, has been used. But, whether it has or not, some of the leading ideas connected with it have been before you.

I think it may be said that the great interest which the public has manifested in the recent advance in the arts relating to electricity has arisen, in a large measure, from the cry of joy with which Faure's battery was received. "A cry which said, in so many words, here we have at last a means of utilizing our waterfalls and natural sources of power" in a way that may relieve us of all the anxiety about our coal fields. To those who had studied the subject it was evident at the time that this cry was premature. And to some of us, at all events, it seemed to be a mistake to encourage false hopes, or, rather, knowingly to base hopes on a false foundation, to hold out as a means of replacing our coal what was, in all probability, only another means of increasing its rate of consumption, for every step in art which facilitates the application of power must increase the demand on the acting sources.

But this is not all; the exaggerated claim set up for electricity, diverted for a time, at all events, attention from the true claim, which would have been sufficient in itself had it not thus been put out of sight. It is not our object at present to save our coal, but to turn it to the best advantage to get the greatest result we can, and if Faure's battery or any subsequent advance in this direction conduces to this, it is no small matter. Now, during the last ten or fifteen years, an entirely new aspect has been given to mechanics by the general recognition of the physical entity which we call energy, in different forms.

We recognize the one thing under different forms in the raised hammer, the bent spring, the compressed air, the moving shot, the charged jar, the hot water in the boiler, and the separate existence of coal, corn, or metals, and oxygen. We see in the revolution of the shafts and the travel of belts in our mills, the passage of water, steam, and air along pipes, the conveyance of coal, corn, and metals, and the electric currents, the transmission of this same thing—energy—from one place to another; and in all mechanical actions we perceive but the change of form of the same thing.

Taking this general or energy point of view, we may get rid of all the complication arising from special purpose, and

recognize nothing but the form of energy in its source, the distance it has to be transmitted, and the special form that must be given to it for its application. And this view, although not the best in which to study the special purpose of mechanics or contrivances, is of great importance, inasmuch as it has revealed many general laws, and many fundamental limits to the possibilities of extension in certain directions.

My object in these lectures is to direct your atention to some of the leading mechanical facts and limits revealed by this view.

There is one general remark I would wish to make, by way of caution. I hope nothing I may say will be interpreted by any of my hearers into a prediction as to what may happen in the future. I have to deal with facts, and I shall try to deal with nothing but facts. Many of these facts, or the conclusions to be immediately drawn from them, may appear to bear on the possibilities—or rather, the impossibilities—of art. But in the Society of Arts I need not point out that art knows no limit; where one way is found to be closed, it is the function of art to find another. Science teaches us the reresults that will follow from a known condition of things; but there is always the unknown condition, the future effect of which no science can predict. You must have heard of the statement in 1837 that a steam voyage across the Atlantic was a physical impossibility, which was said to have been made by Dr. Lardner. What Dr. Lardner really stated, according to his own showing, was that such a voyage exceeded the then present limits of steam power. In this he was within the mark, as any one would be if he were to say now that conversation between England and America exceeded the limit of the power of the telephone. But to use such an argument against a proposed enterprise is to ignore the development of art to which such an enterprise may lead.

I wish to do nothing of this kind, and if, in following my subject, I have to point out circumstances which limit the possibillities of present art, and even seek to define the limits thus imposed, it is in the hope of concentrating the efforts of art into what may be possible directions, by pointing out the whereabouts of such barriers as science shows to be impassable.

Although the terms energy and power are in continual, we might almost say familiar, use, such use is seldom in strict accordance with their scientific meaning. In many ways the conception of energy has been rendered popular, but a clear idea of the relation of energy to power is difficult. This arises from the extreme generality of the terms; in any particular case the distinction is easy. I was going to say that it is easiest to express this distinction by an analogy, but, as a matter of fact, everything that seems analogous is really an instance of energy. Power may be considered to be directed energy; and we may liken many forms of energy to an excited mob, while the directed forms are likened to a disciplined army. Energy in the form of heat is in the mob form; while energy in the form of a bent spring, or a raised weight, matter moving in one direction, or of electricity, is in the army form. In the one case we can bring the whole effect to bear in any direction, while in the other case we can only bring a certain portion to bear, depending on its concentration. Out of energy in the mob form we may extract a certain portion, depending on its intensity and surrounding circumstances, and it is only this portion which is avail able for mechanical operations.

Now, energy in what we may call its natural sources has both these forms. All heat is in the mob form, hence all the energy of chemical separation, which can only be developed by combustion, is in the mob form; and this includes the energy stored in the medium of coal. The combustion of 1 lb of coal yields from ten to twelve million foot-pounds of energy in the mob form of heat; under no circumstances existing at present can all this be directed, nor have we a right, as is often done, to call this the power of coal. What the exact possible power is we do not know, but probably about four-fifths of this, that is to say, from eight to ten million foot-pounds of energy per pound of coal is the extreme limit it can yield under the present conditions of temperature at the earth's surface. But before this energy becomes power it must be directed. This direction is at present performed by the steam-engine, which is the best instru-

ment art has yet devised, but the efficiency of which is limited by the fact that before the very intense mob energy of the fire is at all directed, it has to be allowed to pass into the less intense mob energy of hot water or steam. The relative intensities of these energies are something like twenty-five to nine. The very first operation of the steam-engine is to diminish the directable portion of the energy of the pound of coal from nine millions to three millions. In addition to this there are necessary wastes of directable energy, and a considerable expenditure of already directed energy in the necessary mechanical operations. The result is that, as the limit in the very highest class engines the pound of coal yields about one and a-half millions of foot-pounds; in what are called "first-class engines," such as the compound engines on steamboats, the pound of coal yields one million, and in the majority of engines, about five or six hundred thousand foot-pounds. These quantities have been largely increased during the last few years, as far as science can predict, they are open to a further increase. In the steam-engine, art is limited to its three million foot-pounds per pound of coal; but gas engines have already made a new departure, and there seem no reasons why art should stop short of a large portion of the nine millions.

Other important natural sources of mechanical powers are energy in an already directed or army form, wind and water power. Here the power needs no development, but merely transmission and adaptation, and hence it has one important advantage over the energy of chemical separation. But there appears to be what are greater drawbacks—in the irregularity of these forces as regards time, and the distribution as regards space. These have both been, and are, good servants to man.

The application of the power of the wind to the propulsion of ships has, doubtless, influenced the economy of the world more than any other mechanical feat; and, not very long ago, water-power played no relatively unimportant part of the work of the world. But it would seem that both these have had their day, and are now relegated to a work of a secondary kind, not necessarily so. Some further development of art might bring them to a foremost place again, by developing their use to a hitherto unprecedented extent. Hitherto both wind and water have only had a local application—that is to say, they were used where and when they were wanted. Wind was only used in the sailing of ships on voyages, and for mills, distributed so as to be within range of such corn as was too far from water; while water-power, though very valuable to a certain limited extent, when near habitable country, was otherwise allowed to run to waste, and these wastes included by far the larger sources of this power—the larger rivers and water-falls, the tidal estuaries, and last, but not least, the waves of the sea, a source which has never been utilized for good. A modern idea is, that it needs nothing but a possible development of art to render these larger sources not only available for power in their immediate neighborhood, but available to supply-power wherever it is wanted, and so displace the coal, or replace the power as coal becomes exhausted. The desirability of such a result fully explains the entertainment of the pleasant idea; but, unfortunately, when we come to look closer into the question, the probability of its accomplishment diminishes rapidly. Many of the considerations of which I shall have to speak bear directly on this question; so that I shall now defer its further consideration, merely pointing out that, to accomplish this result, the power must not only be extracted from the water on the spot and at the same time, but it must be transmitted over hundreds or thousands of miles, and must be stored till it is wanted.

It may well be thought that energy in a directed form, or in the army form, may be better transmitted than in the undirected or mob form. As a matter of fact, however, energy has never been and never can be transmitted as mechanical power in large quantities over more than trifling distances, say, as a limit, twenty or thirty miles. I say never can, because such transmission depends on the strength of material; and unless there is some other material on the earth of which we know nothing, we know the limit of this. This is a part of my subject into which I shall enter more closely in my second or third lectures.

In depreciating the idea that wind and

water will ever largely supply the place of coal, I do not for a moment wish it to be thought that I take a gloomy view of the mechanical future of the earth. This, I believe admits of immense development, and will not for long depend, as it does at present, on the adjacency of coal fields. This will be explained as I proceed.

It must not be forgotten that, after all, the most important source of energy is not coal, but corn and vegetable matter. The power developed in the labor of animals exceeds the power derived from all other sources, including coal, in the ratio of, probably, 20 or 30 to 1; so that, after all, if we could find the means of employing such power for the purposes for which coal is specially employed—such as driving our ships and working our locomotives—an increase of ten per cent. in the agricultural yield of the earth would supply the place of all the coal burnt in engines. The energy which may be derived from the oxidization of corn has as yet only been artificially developed in the form of heat, and this may be the only possible way; but physiology has not yet advanced to the point of explaining the physical process of the development of energy consequent on the oxidization of the blood; and it is at all events an open question whether the energy of corn may not be really a form of directed energy, in which case corn would yield six or eight times as much energy as coal does at present, consumed in our engines. As consumed in animals it yields a larger proportion of energy—two or three times as much, and may be more—whereas, by burning it in steam-engines, we cannot get half as much. Should we find an artificial means of developing anything like the full directable power of corn—a problem which has not yet been attempted—coal would no longer be necessary for power. I do not mention this as a prediction, but as showing that there are, besides wind and water, other, and as yet untried, directions from which mechanical energy may be derived in the future.

Electricity is not a natural source of energy, for the simple reason that the metals have mostly been burnt or oxidized during the past history of the earth. But still it is important, at this stage of my lecture, to point out that the energy consequent on the separate existence of metals and oxygen can be developed without combustion, in a totally directed form, *i. e.*, totally available for power.

There are many peculiarities which distinguish the group of elementary substances we call metals, but there is no more distinctive feature than this. This is not a primary source of power, but, as it at present appears, it promises to become the most important secondary source. We cannot find metals existing in a separate form, but by the use of power; where and when it exists we can separate them from the salts, and so store the energy in a form completely available for power. The economical questions relating to such storage of energy will be considered in their place later in the course.

It is not, however, only as affecting storage of power that electricity demands our attention, it also affords a means of transmitting power, which has long held an important place in art, and to which all eyes have been recently turned in expectation of something new and startling.

Before considering the developments of art, and the circumstances on which their further development depend, I shall turn, for a moment, to the processes of nature. The mechanics of the universe, no less than those relating to human art, depend on the transmission of energy. In nature, energy is transmitted in all its forms and under all its circumstances, both those which we can imitate in art, and those we cannot.

The most important point with regard to the artificial transmission of energy is the proportion of power spent in effecting the transmission, and the circumstances on which this proportionate loss depends. Is such loss universal? So far as we know, it is attendant in a greater or less degree on all artificial means of transmission, and on all transmissions effected by nature on the surface of the earth. If it were not, this earth would be no place to live upon. No motion would ever cease. As it is, the winds and waters are rapidly brought to rest by the friction which they encounter. Currents of wind and currents of water form the principal means by which energy is transmitted over the surface of the

earth. But there are other means which experience less resistence. Oscillatory waves, those of sound, are a very efficient means of transmitting energy. Sounds are not transmitted to an unlimited distance, chiefly because, by the spreading of the wave the sound becomes weaker and weaker as it proceeds. It is also destroyed by the friction of the solid surface of the earth. Hence the sounds which reach us from bodies high up, as the explosion of a meteor, are heard much further than such sounds made at the surface of the earth, although there are two records of artillery having been heard two hundred miles. Owing to such incidental destruction of sound we cannot say from experience that sound waves in foul air are destroyed, but from the physical properties of gases we know they are.

Waves on the sea are another very efficient means of transmitting power, a means which may be called nature's mill. The waves which take up the energy or power from the wind in mid ocean travel onwards, carrying this energy, and experience such slight resistance that they will, after travelling hundreds or thousands of miles, destroy the shores on which they expend the last of their energy. If we could find a means of utilizing the energy of waves, we should not only save our coal, but also save our country from the waves; still, water waves experience resistance which we can better estimate theoretically than practically.

These are the principal ways in which energy is transmitted from one part of the earth to another. There are others, such as earthquakes, but they all show the same thing, that power is spent in the transmission of energy.

If we look away into interstellar space the case is altered. Here we see two ways in which energy is transmitted—heat, or light, and the motion of the heavenly bodies. In neither of these can we see any direct evidence of resistance or loss of power; and, as judged by any terrestrial measure, there certainly is none. The distance at which we see stars is a sufficient proof of the freedom with which the wave of light travels; while the regularity of the motion of the planetary bodies shows that they encounter no sensible resistance. Yet, although not directly perceivable, there are circumstances that strongly suggest that in both these forms, transmission of energy is resisted. If space is unlimited, and there are stars throughout it, why do not we see them at greater distances than we do? Under these circumstances there could be no spot in the heavens at which at a sufficiently great distance there was not a star, so that, if the light were not stopped, the whole heavens would be one fiery envelope as bright as the sun. This is a question which philosophers have not decided. But one, and the favorite way out of the difficulty, is to suppose that the light does encounter resistance, even in interstellar space. This is a subject on which your Chairman of Council has boldly launched; and whether his hypothesis be right or wrong, it has brought to the front a very interesting subject.

With regard to the resistance encountered by the planetary bodies, our evidence is even slighter. A few domesticated comets seem to diminish their speed; and it is not so long since we were all on the *qui vive*, by the promise of the spectacle of an old friend, who seemed to have come earlier than he was expected, on purpose to verify a prediction of plunging into the sun; but instead of doing so he passed away and was pronounced a stranger, to the joy of the nervous, but somewhat to the discomfiture of astronomers.

The energy which we derive from the sun comes to us in the form of sunshine, in a highly directed but extremely scattered form, being uniformly distributed over all the illuminated disc of the earth. It reaches the outer atmosphere nearly in the same condition as it left the sun, having traversed ninety odd millions of miles without any sensible expenditure of power. In the twenty or thirty miles of the lower atmosphere, however, it encounters very great, but variable, resistance. Sometimes half of it, or three-quarters of it, may reach the earth's surface. This is rare in our country, and on the average not more than a very small fraction ever reaches the surface.

When the sun does shine, the sunshine is a form of energy which may be, and is, very largely directed so as to yield power. Any such direction which may be accomplished by human art is undertaken at an

enormous disadvantage, on account of the scattered manner in which the energy reaches us. The sunshine must be collected before we can make any mechanical use of it.

In the abstract, there are two methods. The one would be to accumulate the energy of sunshine on a given place, over a long time. This is nature's method. The energy on each portion of the earth's surface, during days, weeks, the whole year, or many years, is accumulated by the growth of vegetables. Corresponding to this, however, art has as yet developed no means whatever. If we don't use the sunshine as it falls, energy is lost for all mechanical purposes. I say if we don't, not that we do use it, but because we can, and have done so in a small way. By means of a lens, or reflectors, the sunshine which falls on a certain place may be concentrated on to a smaller space, and so be sufficient to perform some mechanical operation. In this way, small vapor engines have been worked by sunshine. But the cost of tke apparatus necessary for such concentration is out of all proportion to the result accomplished, and shows the art difficulties must be got over by a new departure. There is one further consideration that sunshine on land is too valuable for the maintenance of vital energy to allow of its being devoted to mechanical purposes.

As regards the perfectness of nature's method, so far as I know, no attempts have ever been made to test this. It is probably very wasteful, as are all nature's methods, but it is effective. In the first instance, the energy of sunshine is stored on the spot where it falls, in the tissues, but chiefly in the sap of the grass and vegetation. If this is not removed, a large portion of the energy of the year's growth, that which is in the sap, is stored in the seed, and the rest, although apparently again scattered on the decay of the tissues, is to some extent preserved in the ground, and either forwards the next year's crop, or takes the permanent form of peat; and our coal fields are but evidence of the way in which the directable energy of sunshine has been stored under circumstances where there was no immediate purpose for which to apply it. Under present circumstances, however, this energy is almost everywhere too valuable to admit of secular storage.

It is either moved directly by nature's method, the teeth of animals, or allowed to accumulate for a longer period, and then removed by human industry. The further aggregation of this energy involves the transmission of energy in a mechanical sense, and hence involves the expenditure of power. Nature works by means of directly converting this energy into power. The plant accumulates the energy of sunshine, the animal collects and appropriates this energy. This collection is accomplished by the expenditure of power, which means a redistribution of that portion of the energy which is capable of direction. The scheme of nature, therefore, is a cycle. The vegetation accumulates the energy, so far as time is concerned, leaving it in a scattered form, requiring power to collect it; this power is in the grass, and only wants direction; this it receives in the animal, which again expends some of the energy in the operation of collecting. If vegetable energy be supplied to the animal in a collected form, then a large portion of the directed energy is available for mechanical purposes. And in this way we may form a rough estimate of the directed energy to be obtained from sunshine in this country. The common agricultural rule is one horse or bullock to two acres, such a horse pulling 120 lbs. at a rate of 3.6 feet per second for eight hours a day. That is a nominal horse.

We thus get something like 3,000,000,000 over and above the energy necessary for the energy spent in eating the corn and moving itself, which we must put down as at least equal in amount. Taking only the available portion, we have the equivalent per acre of nearly three tons of coal burnt in such steam-engines as exist at present. Now the average weight of the vegetable produce from one acre, taking the form of straw and corn, would be about two tons. So that, as far as mechanical power is concerned, coal burnt in our present steam-engines, and corn and straw eaten by horses, yield about the same energy, weight for weight.

The energy which we derive from sunshine is scattered all over the earth, and if it is to be utilized at any spot other than that at which the sunshine falls, it must be transmitted by the expenditure of power.

The energy required for immediate operations of agriculture absorbs a large proportion of the actual energy grown. The surplus is available for purposes of art, and we may say that the primary object of man has been to render this surplus as large as possible. This is accomplished, in the first instance, by applying the residue of energy to so ameliorate the conditions of agriculture as to increase the yield and diminish the labor. In this way the land is leveled, enclosed, and drained; buildings are erected, and finally, but most important of all, roads are made. The effect of roads in increasing the surplus energy is probably greater than any other human accomplishment. The only means of transmitting for purposes of collection or other purpose aggregate energy in the shape of corn, without roads, is on the backs of animals. In this way two or three hundred miles was the absolute limit to the distance an animal could proceed, carrying its own food. On a good road, a horse will draw a ton of food at twenty miles a day, which would mean that it would proceed 800 miles before it had exhausted its supply, or whatever surplus energy there might be available on one spot, half this would be available at 400 miles distance. The labor of maintaining the roads should, of course, be deducted, but this is very small.

The labor of constructing canals is very great, but the result is equal; a horse can move 800 tons twenty miles a day; or a horse could draw its own food for 80,000 miles on a canal. That is to say, with a canal properly formed, a horse could go five times round the world without consuming more energy than was in the boat behind it. Or corn could be sent round the world with a consumption of one-fifth. On railways, at low speeds, the force required is about ten times greater than on a canal, so that the expenditure in going round the world would be about equal to the total energy drawn. If for a moment we replace the horse by the steam-engine, and the corn by coal, we have to add the weight of the engine to the coal, and diminish the efficiency by one-third; we so get that the consumption of coal for the same load of coal as of corn, would be about double, or an engine would go about one-fourth round the world, consuming in coal the net weight in the train, that is exclusive of carriages and engine. Or for every thousand miles corn is carried by rail, something like 10 per cent. of the energy of the corn is expended in draft. This is exclusive of the expenditure in wear and repairs, which will be certainly equal, if not greater. Taking, then, the mean distance by rail between London and the West of America, as 2,000 miles, the present expenditure in the energy of corn in transit is somewhere about 10 per cent. The expenditure of energy on the ocean varies, but if transported by steam it would be probably 10 per cent. more, so that at the present time we are actually receiving available mechanical energy, transported in the form of corn, over 2,000 miles of land and 3,000 miles of sea, entirely by artifically directed power, with an expenditure of less than 50 per cent.; a proportion which 200 years ago would have had to have been spent in transmitting it fifty miles over land; a result which has been accomplished by the employment in the mean time of the residual energy over and above that necessary for agriculture, together with a further supply drawn from our coal beds.

Turning now our consideration to coal, we find per weight as used at present, this yields rather less power than corn, but not less than two-thirds, and it then appears that coal may be transmitted at the present time, between any two places on the earth which are connected by rail and water, with an expenditure of less than 50 per cent.

In instituting this comparison, the standard has been the actual available power, as developed in our present engines and in horses, with which, weight for weight, there is not much difference. But the adaptability of this energy, so developed for particular purposes, renders the one medium much more valuable than the other. Thus for agricultural purposes, weight for weight, horse food is worth in money ten times as much as coal. This shows the extreme difference in the value of energy according to its adaptability; and the extension, for which there is unlimited scope of the adaptability of steam power, may render it ten times as valuable as at present; nor would this be any small proportion compared with the total energy employed in

the work of the world. In this country there are said to be between two and three million horses, and we may put the laboring men down at five millions, or the total power derived from corn down as over three million horses. From the best information going, the work done by steam in this country does not exceed the labor of two million horses, so that more than half the energy is still derived from corn. A greater proportion of the actual corn used for horse food comes across the Atlantic; and for many years maize was sold in this country at an average price of £6 or £7 a ton, the cost of transit being a very small matter. Of course the same cost, say £1 per ton, applied to coal would be a serious matter, considering the low price of the latter. But if, in the present state of our art, energy can be transmitted by corn from any part of the world to this country with an insensible rise, there is no reason to suppose but that, with the advance which science shows us, there is every reason to expect coal may be transmitted with a corresponding small increase in its cost, wherever the demand for it is sufficient to recompense the outlay necessary for opening the roads or canals.

We come now to consider other means of transmitting energy in smaller quantities applicable to its distribution for immediate application. Such transmission is not a matter of secondary importance, although the distances over which it is transmitted may be comparatively insignificant. To emphasize this, I may recall what was previously mentioned, namely, that the relative price of corn and coal shows that the power given out by horses is at least ten times as valuable as that of steam, for more than half the purposes for which energy is used; or that it answers better to burn our coal in bringing corn from America to plough in England, than to use the coal here for ploughing.

In fact, for most of the detailed purposes for which power is used, to draw it from a large source (such as a steam-engine), distribute it and adapt it to its purpose, is ten or twenty times more costly than its transportation in large quantities over thousands of miles.

Now the means of artifically transmitting power may be considered as three. The power may be stored in matter in various ways, and the matter with the energy transported—as, for instance, in our watch springs. The second means is the transmission of power by moving matter, without actually storing the power in the matter—as in shafts and belts, hydraulic connection, &c. And the third method, which is distinct from the others, is the transmission of energy, in the form of heat or electricity, by the flow of currents through conductors; in this way all the power in the steam passes through the boiler-plates from the furnace into the boiler. Of course, each of these means includes an infinite variety of detailed contrivances, more or less dissimilar. But there is good reason for classing them under these three heads, for all the contrivances under each of these heads are subject to the same general limits, whether those of efficiency or distance.

There is one thing in common to all these means of transmission, and that is that they all involve a material medium. The quantity of matter required constitutes a primary consideration in all of them. This quantity of matter is fixed by what we may call the properties of matter, one of the most important of which, as regards the first two means, is the possible strength of material. Looking round. we see the effect of the limited strength of material in all nature's works. Of course it may be that we shall be able to work stronger materials than we have at present. Organic materials, such as the feathers and tissues of animals, are stronger than steel, weight for weight, so that there is a possibility of improvement, but that man will go beyond nature in constructing organic fibre seems improbable, and such possibility of improvement as exists may be discounted. At present we may set down our strongest working material as steel, the art of working in which is so perfect, that we may calculate on nearly the greatest strength for all purposes. I have taken fifteen tons on the square inch as the limit of safe working tension, in making the estimates which I shall now bring before you. First of all, I will ask your attention to the possibilities of transporting power in a stored form.

The question of economy in the conveyance of energy in a stored form is simply one of the intensity with which it

can be stored. If we want to carry energy about, we must have it stored in some material form—and this material has to be carried by ordinary means—so that the question of economy is simply the amount of available energy that we can store in a given amount of material.

If energy, stored in a particular manner, is more readily available for some special purpose than that stored in another, then it may, on the whole, be more economical to carry it in that form. This is abundantly illustrated in our watch springs.

The greatest amount of energy that can be stored in a given weight of steel is very small, compared with other means. To take a familiar unit, to store the energy necessary to maintain one horse-power for one hour would require no less than fifty tons of steel—that is to say, fifty tons of steel in the form of watch springs, all fresh wound-up, would not supply one horse-power for one hour; and yet this is the commonest form in which energy is carried about.

It is the adaptability of the spring, and the readiness with which energy can be put in and taken out, which recommend the steel spring.

India-rubber will store much more energy than the same weight of any other material, say, eight or ten times as much as steel; but with this, several tons would be required to store the horse-power for one hour. A much more capacious reservoir, according to its weight, is compressed air. There are certain difficulties in getting the energy in and out without loss; but with air, compressed to four times the pressure of the atmosphere, we should only require about 20 lbs. of air to yield the amount of one horse-power for one hour. Of course, if we were going to carry this air about, to the weight of the air would have to be added the weight of a case to contain it, and such a case, in the form of steel tubes, would weigh something like 230 lbs.; so that, in any form in which we can carry compressed air about, we shall have about 300 lbs. to carry for each horse-power per hour.

Another means of storing energy, very largely used, is hot water. This is largely used in a way not always recognized. The boiler serves another purpose besides that of converting the energy of the furnace into the power of the steam. It stores the power, and equalizes the stream between the fire and the engine, a function the importance of which has been brought to the front in the recent efforts to apply electricity for communication of power, where the want of a similar reservoir between the generator and the motor has, in many cases, proved fatal to the enterprise, a want which secondary batteries are now being used to meet. Hot water has also been employed as an independent reservoir, and as such it is better in some respects than compressed air. The fundamental limits are of much the same kind. In this case, however, the absolute limit is temperature. The vessel in which the water is carried must be strong enough to withstand the pressure, and all materials lose their strength as they get hot. The considerations are here much the same as in the steam-engine, and 400° Fahr. appears to be about the limit. At this temperature, for every 4 lbs. of water the cases would weigh 1 lb., and there would be no advantage of large over small cases; except as a matter of construction, the proportionate weight would be the same. The gross power of a pound of water, the steam being used without condensation, is about 20,000 foot-pounds, or we should require 50 lbs. to store 1,000,000; this is the extreme limit again. The present accomplishment would be about 150 lbs. per 1,000,000 foot-pounds stored—rather less than compressed air. The only other means of packing power, that is at present looked to, is that of the much talked about secondary battery. Here there is a great deal of doubt as to what is actually accomplished; take the most reliable statements, from which it seems that in order to get 1,000,000, something like 100 lbs. of battery is required, which will make this means of storing energy very much the same as compressed air or hot water.

It is important to notice that the initial cost of the energy stored by these means differs considerably. This cost is rather difficult to estimate; but a practical estimate may be formed in this way:

Taking the power, as delivered by the steam-engine, as 1, how much of this power will be given out after secondary storage? Here the hot water has an ad-

vantage, for it is heated directly by the coal, and is all on its way to the steam-engine.

With compressed air, there are three operations, each as costly as the steam-engine, and at least half the initial power is spent during the compression, storage, and expansion; so that the energy is at least double as costly in coal, and six times as costly in machinery. I have put it down as three times as costly as the energy in hot water, but this is considerably below the mark. The electricity has also to go through three operations, and cannot be less costly than compressed air.

Now, if we revert for one moment to the consideration of the main transmission of power, we see at what an immense disadvantage any form of packed energy is, compared with coal or corn; as at present packed it weighs at least 100 times as much.

While the limits imposed by the strength of material render it certain, as far as compressed air and hot water are concerned, that the weight can never be reduced by more than half, these limits are sufficient to show that packed energy cannot be transported over long distances, even if it can be obtained directly from such falls as Niagara. But this is no argument against the importance of these means for short distances and special purposes. As I have already pointed out, our watches show that circumstances may render the very heaviest means the best for particular purposes. And if in any of its forms packed energy were directly available for household purposes, though it cost ten or twenty times as much as power direct from the steam-engine, its use would still be assured..

One fact should be noticed, that in all these forms the power is packed, and needs nothing but drawing off, whereas corn or coal do not contain the power. The oxygen is an equally essential ingredient. In this fact lies the great advantage of corn and coal for transportation. They are really, so to speak, but cheques for power, which can be cashed at any spot where a bank, in the form of a steam-engine or a horse, exists. But, of course, not being energy, they are not generally current—in fact they are worthless, except where the bank exists, and where they represent such small amounts that the banks refuse them. Now these forms of packed power are, so to speak, generally current, that is to say, they are available under almost all circumstances, and in greater or less degrees of smallness; from the very smallest, which is the watch spring in our pockets, which supplies a continuous stream of power in less than one ten thousand millionth of a horse-power; or the Whitehead torpedo, which carries some million foot-pounds of energy under the sea. Perhaps the most pressing purpose for which these forms of packed energy are wanting is that of locomotion.

The distance which a locomotive body, be it animal or machine, can travel, loaded or free, is limited by the ratio of the power which it carries to its gross weight. The speed which it can attain is limited by the rate at which it can use its energy compared with its weight. Hence there are two particulars in which we can compare the different form of stored energy for locomotive purposes.

Let us take the horse and the locomotive. A full-sized horse weighs, say, 1,500 lbs., and, at a rate of $2\frac{1}{2}$ miles an hour, will go five hours without food, doing about 10,000,000 foot-pounds of work, including the work necessary to move itself; this represents the largest result, or about 150 lbs. per 1,000,000 foot-pounds. If the horse is put to ten miles an hour, it will not do more than 1.5 million foot-pounds in a single journey, besides moving itself. Probably the greatest rate at which a horse can use its energy is about 4,000,000 foot-pounds per hour, or 750 lbs. per horse-power.

A locomotive with its tender, say, weighing 60 tons, exerts 500 horse-power gross—270 lbs. per horse-power per hour; so that a first-class locomotive with tender is above one-fifth as heavy for its power as the horse; but then the horse cannot go more than ten miles an hour.

Now, in a general way, passenger locomotives carry coal and water for eighty or one hundred miles, *i.e.*, two hours; or the locomotive already mentioned expends at one run about 2,000,000,000 foot-pounds; which means that the gross weight of the locomotive is about 60 lbs. or 70 lbs. per 1,000,000 foot-pounds of power with which the locomotive starts.

In thus taking the gross weight of the

horse or locomotive. we must remember that this includes the weight of carriage and machinery, and that in whatever form the energy is carried, this weight must be added. In the locomotive the weight of water and coal in the tender for two hours' journey weighs about one quarter the gross load; and if we add the weight of the boiler, we may consider the carriage and machlnery at one-half to one-third the gross load. Taking the latter, and substituting for the boiler, coal, and water; energy in either of the above forms, the coal, water, and boiler would be about 40 lbs. per 1,000,000: so that, if we took compressed air instead, we should have one-fourth the power; or the engine would run for thirty minutes instead of two hours, a distance of twenty-five instead of a hundred miles. A fireless locomotive might do more than this, say, thirty-five minutes, or thirty miles, at the same speed as the locomotive. Faure's battery, if it could be made to work at all, would carry the locomotive forty-eight minutes, or thirty-five to forty miles.

These figures seem to show that the locomotive has little to fear from any of these rivals, that is under circumstances where the smoke and steam are no harm, and where a full-sized locomotive is required. But there are already some cases where the locomotive is required and where the burning of coal is impossible. Should the Channel Tunnel be made, there will be a great field for some form of packed energy; but it does not seem that the promoters have much faith in a smokeless means of working the tunnel being forthcoming. As regards horses, however, there is nothing to show why the horse should not be rivalled by some one of the forms of packed energy. There have been inventors who have constructed carriages to go by clockwork. This has now become possible, substituting hot water, compressed air, or a battery for the spring, and such means have already rivalled the horse on tramways. The fact that horses are not at all used for tramcars is a matter of as much surprise as that steam should be used on underground railways. For locomotives driven by compressed air might certainly be made cheaper and better in every way.

At the present time it would probably answer well, in a pecuniary point of view, to supply in compressed air energy at the rate of 2d. or 3d. per million foot-pounds, provided a sufficient quantity could be required; so that if simple and efficient means of applying such energy to perform the heavier part of manual labor, we might get as much power for 6d. as what a man will do in a day at 2s. But it is the means of applying it that is wanting.

Even for horse work—except where there is a railway or tramway—the mechanical means are wanting. We have no mechanical substitute for the horse's foot. So that although there are more than a million horses in this country continually engaged in the operations of husbandry, where they work in groups so as to get three or four horse-power at one operation, an amount of power not too small for the direct application of steam power; and although for twenty-five years steam-engine makers have been doing their very best to adapt the power of the steam-engine to this labor, which exceeds any other actual application of power, the possibility of steam ploughing with economy is still a question. The use of steam on paved or on macadam roads is much the same, so that, until steam has been applied to such purposes, there is little hope for other forms of stored energy.

Coming back for a moment to Faure's battery, I would carefully point out that the result which I have put down—100 lbs. per 1,000,000 foot-pounds of energy—refers to what has been accomplished, and not to any possible limit. The principles involved in the chemical action of these batteries, in fact in all batteries, are well understood; and so far as these principles are involved, it is easy to define limits; but there are a number of secondary actions which are not so well understood, and which have hitherto prevented any approach to the theoretical limits. In the Faure's battery, the theoretical limits are about 3 lbs. per 1,000,000 foot-pounds. That is to say, the oxidization of 1 lb. of lead to litharge, and the deoxidization of 1 lb. of peroxide, together, yield 360,000 foot-pounds. How far, at present, Faure's battery is within this limit, at once appears something like twenty-four times, Should this be accomplished, power could be

packed at the rate of 1,000,000 foot-pounds for 3 lbs., or say 6 lbs. weight, to allow for wastes, a result which would most certainly displace steam in the locomotive, but which would still leave coal and corn six times the lightest vehicle of power.

It should be noticed, however, that although the means of doing so are still entirely wanting, could other metals, such as iron or zinc, be used instead of lead, the results would be much greater. This is shown by the relative amount of power necessary to oxidize or deoxidize these materials, which we see for iron and zinc are five or six times greater than for lead ; here is an apparent opportunity for art.

Should this be realized, then, indeed, coal might be displaced as the cheapest medium for the transmission of power, but that would be a small matter compared with the change that would occur in our ways of applying power. For the dream of Jules Verne, of 20,000 miles under the sea, would become a reality, and, instead of steamboats, we should travel in submarine monsters as yet unnamed, which we may call steam-fish.

But if science as yet imposes no limits beyond those I have mentioned, at the same time it holds out no prospect. The chemistry of these batteries has been very deeply considered, and those who have studied the subject most deeply apparently see no direction in which to direct their efforts; so that any great advance in this art must entail a great discovery in science.

There now only remains for me to consider the transmission of power, as power, or by electricity, a most important branch of my subject.

So far I have spoken only of the conveyance of power by means of coal, corn, or in one or other of the several forms of packed energy. To-night I come to consider the transmission of power by what are more distinctly mechanical methods, and by currents along pipes and conductors. These are the means by which power is almost always distributed, *i. e.*, transmitted from the acting agent, be it horse, water-wheel or steam-engine, to its operation, whatever it may be. In most cases the distance of such transmission is so short as to be the subject of small consideration in determining the means to be employed. That is to say, the means are chosen rather by their adaptability to receive and render up the power than by the efficiency with which they transmit it. Thus, if we take an ordinary mill, the shaft which receives the power from the engine is generally driven at that speed which is best adapted to receive the power from the engine, and deliver it to the machinery in the mill, without considering whether a much smaller shaft might be used if it were caused to run at a much higher speed. Thus, in a mill driven by an engine of two or three hundred horse-power, the shaft which receives the power will generally be five or six inches in diameter, whereas it would be possible to use a shaft of two inches diameter if the efficiency of the shaft were the only consideration. Or, again, take a screw steamboat. The distance from the engines to the screw may be 250 feet, the power 10,000 horse. This could be transmitted by a shaft twelve inches in diameter, if allowed sufficient speed, but the screw has to make sixty revolutions per minute, and this determines the speed at which the shaft is made to run, and hence the shaft is made thirty inches instead of twelve inches. This is because, owing to the smallness of the distance, the efficiency of the means of transmitting the power is a small consideration. There are, however, many circumstances under which it is impossible to bring the source of power close to its work, and then either mechanical power is not used, or the efficiency of the means becomes a consideration.

In other cases it is a question whether it is better to distribute the sources of power, such as steam-engines, so that they may be near their work, or to use one large source, and distribute the power by some mechanical means. This rivalry exists in almost all engineering work which covers a large area, and generally a compromise is come to, engines being distributed about the works, and the power of these distributed to the machines by means of shafting. In many cases separate engines are used for each machine, but not often separate boilers, the power being distributed by steam-pipes.

Dockyards have long afforded a field for the competition of the various means of distributing power. Here, generally,

the distances between the operating machines, such as cranes and capstans, is considerable, and the work required from each machine very casual. And every means of distribution is, or has been, in use, from a separate engine and boiler to each machine, as at Glasgow (separate engines drawing their steam from central boilers), to a complete system of hydraulic transmission, from a central pumping station, as at Grimsby or Birkenhead.

But the question between centralization or distribution of steam-engines is not by any means the only one, or most important one, which depends on mechanical means of distributing power. Every improvement in the means of distributing power from a central engine opens a fresh field for its use.

The considerations relating to this subject are numerous. Hitherto, as regards the main transmission of power, the principal consideration has been the percentage of loss according to the distance, but, as regards the final distribution of power, the form in which it is distributed must be such as admits of its being at once available for its purpose. Thus, hydraulic distribution is favored in dockyards, because it is required for heavy forces and slow motions, but where rapid motion is required, hydraulic distribution gives place to some other.

Again, where the quantity of power that has to be distributed is almost important consideration, the distribution by means of water or compressed air will generally be the most efficient, whereas these would be by far the most costly means for small quantities. It thus has to be remembered that, besides the general question of efficiency, each means has particular recommendations for particular purposes.

It is not, however, with these particular recommendations that I am concerned. My object is to show the limits within which the use of each means is confined, however fit it may be for its purpose. Taking first the mechanical means, which are shafts and ropes, we find that the possible limits to both these means are absolutely defined by the strength of material. The amount of power any piece of material will transmit by motion against resistance is simply the mean product of the stress or force acting in the direction of motion on the section multiplied by the velocity, so that, if the stress is uniform over the section, the work is the product of the area and intensity of stress and the velocity.

In a revolving shaft, neither the stress nor the velocity are uniform over the section, both varying uniformly from nothing in the middle to their greatest value on the outside; so that their mean product is exactly half the product of the greatest values. The greatest power per square unit of section a shaft can transmit is half the product of the greatest stress into the velocity at the outside of the shaft.

Taking, then, the greatest safe working stress for steel at 15,000 lbs. on the square inch; taking what is the greatest practical velocity at the surface, 10 feet per second (the speed of railway journals), the work transmitted is 75,000 foot-pounds per second per square inch of section—135 horse-power; so that we should have to have a shaft of upwards of 7 square inches in section to transmit 1,000 horse-power, that is, a shaft of over 3 inches diameter. The friction between such a shaft and lubricated bearings is well known, .04; so that calculating the weight of the shaft 24 lbs. per foot, we have power spent in friction about 52,000 foot-pounds per mile, that is, one-tenth the total power the shaft will transmit. That is, if we put 1,000 horse-power into a 3-inch shaft, making 500 revolutions per minute, we ought, at the end of a mile, to be able to take 900 horse-power out of it. If we had to go farther the size of the shaft might be diminished, so that in the next mile we should again lose a tenth, and if we repeat this process seven times we shall, at the end of seven miles, have left about half the original power put in.

It will be thought, perhaps, that a 3-inch shaft is very small to transmit so large a force; this is because the speed of 500 revolutions per minute is inconveniently high for purposes of employing the power; but if it were merely a question of transmission it would be about the best speed. This, then, shows the limit of the capacity of shafts as transmitters of work.

Turning now to steel ropes, these have a great advantage over shafts, for the stress on the section will be uniform, the velocity will be uniform,

and may be at least ten to fifteen times as great as with shafts, say 100 feet per second; the rope is carried on friction pulleys, which may be at distances of five or six hundred feet, so that the coefficient of friction will not be more than .015, instead of .04. Taking all this into account, and turning to actual results, the work transmitted per inch would be 1,500,000 foot-pounds per second; or that a $\frac{3}{4}$ inch rope is all that is necessary to transmit 1,000 horse-power in one direction, this would make the loss per mile only $\frac{1}{60}$. But in practice, rope has to be worked backwards and forwards, and the tension in the backward portion of the rope must be half the tension in the forward portion. This reduces the performance from $\frac{1}{60}$ to $\frac{1}{20}$, which would cause half the work to be lost in ten miles. If we use a bigger rope, and run at lower speed, then the coefficient of friction would be reduced to 0.1 and the distance extended to fifteen miles. Experience with ropes is large, and they have been found, without question, to have been the most efficient mechanical means of transmitting power to long distances, but their use is subject to drawbacks. The ropes wear somewhat rapidly, as do also the pulleys on which they run, and this circumstance is very much against their use in any permanent work. Nevertheless, they are used for working mines, steep inclines, and steam ploughs; while at Schaffhausen they have been used for transmitting power to considerable distances.

Turning to the transmission of power along pipes, we find the conditions somewhat modified. The formula for the amount of power transmitted by water is the same, namely, the product of the area of section into the velocity. But the resistance obeys different laws. In the case of shafts and ropes we have seen that the distance is subject to an absolute limit.

In the case of fluid in pipes this is not so, no matter how long a pipe may be, if there is no leakage, water would flow along the pipe until the level of its surface were the same at both ends. But the rate of flow would diminish with the length and diameter of the pipe. Thus, we can transmit power through a perfectly tight pipe, however small, and however long; but when we come to consider the gross power that can be transmitted through a given pipe, with a given per-centage of loss, the question is different. Given the size and strength of the pipe, the gross amount of power and the per-centage of loss, and the limits are fixed. Thus, taking a 12-inch pipe capable of standing 1,400 pounds on the square inch, the loss in transmitting 1,000 horse-power would be about 5 per cent. per mile, at first increasing—as the pressure fell to 700 lbs.—to ten per cent. We should thus have lost half the power in about seven miles. We cannot say that seven miles is the absolute limit, for with a 24-inch pipe, which would cost four times as much per mile, we could transmit the same power thirty times as far with the same loss. The cost of laying a 12-inch pipe for seven miles, however, would probably be as much as even 1,000 horse-power would stand, while a 24-inch pipe for 200 miles would be out of all proportion. Then there is the consideration of leakage, which although very small for short lengths, is larger for greater lengths.

Seven miles is at present an outside economical limit of hydraulic transmission, even for such a large amount of power, but with air the case is different. This flows so much easier than water, that the cost of transmitting the same power through the same distance, with the same loss, would be about 12 per cent., or, at the same cost per mile, the air may be transmitted 100 times as far with the same loss. The total cost, however, would thus be 100 times as great, which would exceed the economical limit; but not only theory but practice has shown that power may be economically transmitted five times as far by air as by water—something like thirty miles. But on comparing these two means, one circumstance must not be lost sight of, and that is, that getting the power into the pipe in the form of compressed air will cost twice as much as getting it in in the form of water. This is a great advantage for water where the distance is short, but where the distance is long, the greater efficiency of air more than compensates for this initial loss.

Like water, air can only be transmitted economically where the quantity is large, the friction being proportionately greater

in small pipes than in large, varying as the four-fifths power of the diameter.

This is a great drawback, both as regards hydraulic and compressed air transmission. It does not affect ropes and shafts in the same way, but even in these cases considerations of durability prevent these means being used efficiently for the transmission of small quantities of power to considerable distances, so that, with the possibility already mentioned, there remains an opening for any means that will enable power to be transmitted efficiently in small quantities, and such a means we have in the flow of electricity along wires or conductors. In considering electricity we may well start with the questions—1. Will electricity enable us to transmit power in large quantities more efficiently than the foregoing means? 2. Will it enable us to transmit small quantities? These questions may be more definitely answered than they could a few weeks ago. Thanks to the experiments of M. Deprez, who appears to have been the only one, out of all those who are advocating the use of electricity, who has had the courage to try and see what can be done, we can now say with certainty that a current of electricity, equivalent to 5 horse-power, may be sent along a telegraph wire $\frac{1}{6}$ of an inch in diameter some ten miles long (there and back), with an expenditure of 29 per cent. of the power, because this has already been done. In order to do this it would seem that M. Deprez has perfected his apparatus so as to have nearly reached the possible limit. Compared with wire rope this means falls short in actual efficiency, as Messrs. Hems sends 500 horse-power along a $\frac{3}{4}$-inch rope. To carry this amount, as in the experiment of Deprez, one hundred telegraph wires would be required; these wound into a rope would make it more than 1.4 inches in diameter, four times the weight of Mr. Hems' rope. With the moving rope the loss per mile is only 1.4 per cent., while with the electricity it was nearly 6; so that, as regards weight of conductor and efficiency, the electric transmisssion is far inferior to the flying rope. Nor is this all. With the flying belt Mr. Hems found the loss at the ends, in getting the power into and out of the rope, $2\frac{1}{2}$ per cent.; whereas, in M. Deprez's experiment, 30 per cent. was lost in the electric machinery alone, which is very small as such machinery goes. But this is not all. No account is here taken of the loss of power in the transmission to and from the electric machinery, a matter which is, I believe, very much under-estimated.

The machines made revolutions at 1,000 and 700, much too high for direct connection with either a steam-engine or any mechanical operator; the power, then, had at each end to be transmitted through gearing, or a system of belts. And supposing this alteration of speed to have been five or six at each end, experience tells us that a loss of at least 15 per cent. must ensue. This loss was indeed apparent, for the dynamo-meter was connected with the machine with a belt, which showed a loss from this one belt alone of 20 per cent. Taking the whole result, it does not appear that more than 15 or 20 per cent. of the work done by the steam engine could have been applied to any mechanical operation at the other end of the line, as against 90 per cent. which might have been realized with wire rope transmission. To set off against this electricity has the enormous advantage in the conductor being fixed, and in the fact that it is likely to be, if anything, less costly and more efficient for small quantities of power than for large. These advantages will certainly insure a very large use for electricity in the distribution of power, particularly for high-speed machinery.

There is yet another means of communicating and distributing energy now coming rapidly into vogue. This is by the transmission of coal gas along pipes. The distances, often many miles, through which the gas is often transmitted before reaching the engine, are such that, with any other means of distributing power, would considerably enhance the cost of the power. But in the case of gas it does not appear that these distances are at all a matter of consideration. This may be at once explained. It takes about ten cubic feet of gas to develop 1,000,000 foot-pounds in a gas engine, whereas of air compressed in the ordinary way it would require something like 140 cubic feet to yield the same power. Hence, the comparative cost of transmission is the cost of transmitting ten cubic feet of gas against that of 140 cubic feet of compressed air, and these would be about as

one to twenty-five; so, as a means of distributing energy, gas is twenty-five times more efficient than compressed air.

I have now placed before you, as far as circumstances will allow, the various means by which energy, in a form available for power may be transmitted over long distances, together with the circumstances which limit such transmission. By means of the railway and steamboat corn and coal can be, nay is, transmitted half way around the earth with an expenditure of power of less than half the power represented by the coal carried, but this can only be done where the quantity to be transmitted is very large.

At present the efficiency is unrivalled, no means of packed energy or of current energy approaching even 1 per cent. And, further, there is apparent room for a large diminution in the present expenditure, small as it is in the improvement on the steam-engine as a means of directing the energy of coal. For the distribution of power, this means ceases to be efficient, nor can it be employed to transmit energy which has already taken the form of power. For these purposes other means have to be employed. These various means, although they differ greatly in efficiency, all fall so far below the efficiency of coal and corn, that a hundred miles appears to be the outside limit any economical transmission of power, in quantity for mechanical purposes, could be at present effected; and hence any power, be it derived from wind or water, must be used within this radius of its source; and, except in places far out of the reach of rail or water, this limit may be divided by ten.

So far as efficiency of transmission in considerable quantities, neither secondary batteries nor electrical transmission are more efficient than compressed air or belts, but when it comes to transmitting small quantities then electric transmission has a decided advantage. The cost of the electric conductor diminishes with the quantity to be transmitted, and by making the conductor sufficiently large, its efficiency may be increased to any extent.

At the present time, electric conductors are continuous half way around the world, and whenever a message is sent from England to Australia direct energy is transmitted 10,000 miles, but in what quantity? The energy of the current, as it arrives, is not much more than sufficient to keep a watch going, at any rate not more than $\frac{1}{1000}$ millionths of horsepower. The value of such energy, estimated at £17 per minute, would be equivalent to a billion pounds per horsepower per hour, whereas the highest price paid for animal labor in Australia or England is not more than 6d. per horsepower per hour. This shows the difference between the transmission of electricity for telegraphic purposes and its transmission for mechanical purposes. Energy differs in value greatly, but for operations that can be performed by men or horses, the price of energy must be regulated by the highest price of corn.

The prosperity of any spot in the past depended on the fertility of the adjacent soil. But the use of coal has altered this, and now the present prosperity of this country is owing to the adjacency of our coal fields, these having rendered it possible to bring our food across the earth. The improved means of transmitting coal and corn, it would seem, have, or may again, change this; and if, instead of looking on the life of this country as limited by the life of our coal fields, we look boldly forward and foster every means, political, social, and mechanical, which may render this a favorite spot to live upon, we need not fear that the necessity of bringing our coal from a distance will make a difference which will counterbalance the advantage we shall derive from the mechanical facilities we shall have here.

In a recent *Chronique Industrielle* an account is given of the examination of a large cylinder in a Woolf engine employed in the mines of Sarrebruck. On opening the cylinder there was found upon the piston a brown, wax-like mass, weighing more than 150 kilogrammes. It contained 60 per cent. oxide of iron, 26.77 per cent. of organic matters soluble in alcohol, 5.7 per cent. of insoluble organic matter. The residue being composed of water with a little silicic acid. The cylinder had been in use for about a year, during which time 192 kilogrammes of suet had been employed for lubrication. The decomposition of the suet by steam into glycerin and fatty acids led to the formation of a soap of protoxide of iron. The oxidation of the iron, which is limited chiefly to the interior surface of the cylinder, gradually produces an enlargement of the diameter. The evil may be obviated by using as a lubricant mineral oil of good quality, which boils only at a very high temperature.

THE DRYING OF GUNPOWDER MAGAZINES.

By PROF. CHARLES E. MUNROE, U. S. N. A.

Proceedings of Naval Institute.

In the Ordnance Instructions of the United States Navy, paragraph 1233, page 341, it is directed that, in order to absorb the moisture from a magazine, chloride of *lime* or charcoal should be suspended in an open box under the arch, and that it should be renewed from time to time.

On reading this I felt assured that an error had been committed, and that it had probably arisen from the fact that the chemical names of two quite different substances, chloride of *lime* and chloride of *calcium*, are really so very much alike in sense and sound as to be very often confused, and to be even regarded as synonymous by those who are quite conversant with them.

Chloride of *lime* is the substance which is sold in commerce under the name of bleaching powder, and it is believed to generally consist of a mixture of CaO, CaH_2O_2, $CaCl_2$ and $Ca(ClO)^2$ or $(CaO)Cl_2$. When charged as completely as possible with Cl and when in its purest form it is regarded by Kolb as having the composition represented by $Ca_3H_6O_6Cl_4$, which by the action of water is decomposed as follows:

$$Ca_3H_6O_6Cl_4 = CaH_2O_2 + CaCl_2 + Ca(ClO)^2 + 2H_2O.$$

When exposed to the air the bleaching powder absorbs water, probably in proportion to the $CaCl_2$ and CaO which it contains, but it is not regarded as a deliquescent salt. At the same time it absorbs CO_2 from the atmosphere, and the calcium hypochlorite is decomposed, probably in accordance with the reaction.

$$Ca(ClO)_2 + H_2O + CO = CaCO_3 + 2H\,ClO$$
or $$Ca(ClO)_2 + H_2O + CO_2 = CaCO_3 + H^2O + Cl_2 + O.$$

Chloride of *calcium*, on the other hand, has the formula $CaCl_2$. Its most distinguishing and characteristic property is that it is highly deliquescent; that is, it possesses the power of absorbing moisture from the atmosphere, when it is exposed to it, to such a degree as to become a liquid. So deliquescent is this substance that it is always used as the example of that property when it is defined. Brandes found that 100 pts. of it, exposed to an atmosphere saturated with moisture for ninety-six days, absorbed 124 parts of water. The atmosphere has no further effect upon it than to liquefy it.

To compare the relative absorptive powers of these two substances, I exposed watch-glasses, containing, one ordinary bleaching powder, the other chloride of calcium, side by side under a bell glass in which a vessel of water had been placed. After an exposure of three days they were weighed, and it was found that while the bleaching powder had gained 30.70 per cent. in weight, the calcium chloride had increased 60.50 per cent. The data are as follows:

	Weight taken. Grams.	Weight found. Grams.	Increase Grams.	per cent.
Calcium chloride	22.2724	36.0195	13.7471	60 50
Bleaching powder	32.9250	43.0380	10.1130	30.70

The conditions of the experiment were quite favorable to absorption of moisture by the bleaching powder, for there was necessarily but a limited supply of CO_2 in the bell glass. When it is exposed to the air the CO_2 which it absorbs forms a crust of $CaCO_3$ over its surface which impedes the absorption of moisture,

From the consideration, then, of the hygroscopic properties of these two substances it is evident that it is the chloride of *calcium* and not the chloride of *lime* which should be used as a desiccating agent for magazines, and as it is a by product in the manufacture of chlorine and of chloride of *lime* or bleaching powder it ought to be obtained very cheaply. The porous chloride which has been dried at about 200° C. is better adapted for absorbing water than the fused chloride, since the latter contains both CaO and $CaCO_3$ as a result of igniting the chloride in contact with air.

In addition to the fact that bleaching

powder is not the most efficient desiccating agent, either as regards its power or its price, it has occurred to me that, owing to certain other properties which it possesses, it might prove to be a very objectionable substance for use for this purpose.

It is known that after gunpowder has been stored for some time its initial velocity is reduced. This is held to be due to the absorption of moisture aud the consequent efflorescence of the nitre. While recognizing the force of this explanation, I have surmised that there are other causes for this deterioration, and that one of them might be found in the slow oxidation of the sulphur, its conversion into sulphuric acid, the decomposition of the nitre with the formation of potassium sulphate and nitric acid, and then the further oxidation of sulphur by this nitric acid. The potassium sulphate thus formed would act, like the glass in Gale's process, or the graphite, charcoal, and so on, of Piobert and Fadéieff, for gunpowder; the silica in use for the silicated gun-cotton, or the camphor in the gum dynamite, to reduce the rate of inflammation, or of the transmission of the explosive undulations. The most satisfactory way for testing this theory would be by examining samples of fresh gunpowder for sulphuric acid, and then, after it had been exposed for some years to the incidents of storage and transportation which obtain in the service, to examine the same lot of powder again. I have not as yet had an opportunity for putting the theory to the test.

It, however, seemed probable to me that if oxidation, of the nature spoken of, could take place in the presence of air and moisture only, it would certainly be hastened by the presence of bleaching powder, since when the latter is exposed to the air the CO_2 absorbed decomposes it in accordance with the reactions given above by which chlorine or oxides of chlorine are liberated. These products in the presence of water are powerful oxidizing agents, and will consequently act more energetically than the oxygen of the air alone. To test this I arranged an apparatus so that washed CO_2 might pass into a bottle in which bleaching powder suspended in water was placed, and the washed product of this reaction was passed into a flask in which the gunpowder to be tested was suspended in water. The gunpowder taken for the test was Oriental.

Two portions of this powder were weighed, each being placed in a separate flask, and 200 cm. of distilled water added to it. Through one of these the gas from the bleaching powder was allowed to bubble for twelve hours, and then it remained standing for some time. It was exposed to the action of the gas in all for 36 hours, most of the time being in strong daylight. The other flask stood, uncorked, for the same time in another room. Both were now filtered and 100 cm. of each were taken and treated with hydrochloric acid and barium chloride. The precipitate obtained in each case was washed and ignited as for the determination of sulphuric acid. The results were as follows:

	Weight taken. grms.	Weight $BaSO_4$fd. grms.	Per cent S oxidized.
Samples exposed to air	3.4070	.0266	0.16
" " " bleaching powder,..............	4.0692	.4768	1.60

That is, that in the sample of gunpowder exposed to the bleaching powder, there were ten times as much sulphur oxidized as in that which was exposed to the air.

The method of experiment described above was employed because it was known that the state of solution would favor the change, and it was supposed that, under the conditions which prevail in magazines, a marked change would not be noticed except after a considerable length of time. However, an experiment was set on foot which imitated the conditions exactly. I put a quantity of bleaching powder in the bottom of a desiccator, and on the shelf above I put a weighed quantity of the Oriental superfine saltpetre powder, in the granulated, glazed state in which it is sold. The desiccator was then covered and set aside. At the end of twenty-six days I examined the powder, and was surprised to see an appearance of change on the surface of the powder granules; so I immediately dissolved in hot water, filtered and precipitated with barium chloride and hydrochloric acid. For comparison, I made another determination of the sulphates in the fresh powder. The results are as follows:

	Weight taken grams.	Weight $BaSO_4$fd. gram.	Per cent of S oxidized.
Fresh powder	6.6818	.0816	0.17
Powder exposed 26 days to atmosphere of bleaching powder,	6.0256	.6566	1.50

It would seem to follow, from the above results, that while the chloride of *lime* is not so efficient a desiccating agent as the chloride of *calcium*, it is at the same time very objectionable, since it *may* cause a serious deterioration of the gunpowder.

I propose hereafter to examine samples of powder which have been acted upon by the gases from bleaching powder, by means of a method which I have recently devised for testing the incorporation of gun-powder, and I hope, before long, to have the honor of describing this method to you.

TERRESTRIAL DEFLECTIONS.

By Rd. Randolph, C. E.

Contributed to Van Nostrand's Engineering Magazine.

In the October number of *Van Nostrand's Magazine*, Prof Hendricks in his allusion to an article in the June number, seems to ignore a former article by the same writer in the February number, entitled *The Influence of the Earth's Rotation on Derailments*, a criticism of which by Prof. Davis was the occasion of the one to which he refers; for in the first article the centrifugal force from the earth's axis was not considered at all; the discussion being confined to the force which Prof. Hendricks says the writer failed to notice. The omission of the consideration of the centrifugal force of the earth's surface was one of the objections of Prof. Davis, which was answered by showing that this force was neutralized by the form of the spheroid of revolutions. To this Prof. Davis might have replied that this was true only of objects on the surface which had the same rotary velocity in their respective latitudes; not those which, having an independent velocity, increased or diminished this velocity of rotation. For the form of the spheroid not being sufficiently oblate for the greater velocity would not fully neutralize the tendency towards the equator; and being too much so for the lesser, gravitation towards the pole would not be fully resisted. To this fact Prof. Hendricks has incidentally called attention, and should be credited with so doing; but he fails to give a true computation of the effects of this modification, as will be shown. He is also liable to criticism for the manner in which he has treated the subject otherwise.

The first proposition in the ignored article was, that each minute horizontal plane composing the surface of a rotating sphere rotated about a normal to the surface in proportion to the sine of the latitude of the normal; and that a body passing over these planes would experience the deflective force of each in succession. This gives a clearer view of the effect of the rotation of the sphere than the first proposition of Prof. Hendricks, viz., that a vertical meridian plane deflected about each of its radii normal to the surface of the sphere in proportion to the sine of the latitude of the normal. But neither could assist any one in comprehending the remarkable proposition that this deflective force caused the body, moving over the surface in any direction, to describe a circle whose circumference is the independent velocity of the body multiplied by the time required for the meridian plane to deflect, or the horizontal plane to rotate 360 degrees. That is to say, if the independent velocity is 10 miles per hour at 30 degrees of latitude, whose sine is 0.5, and requiring 48 hours for the plane to deflect 360 degrees, then the circumference of the circle will be 480 miles. Of course the deflection which causes this circle to be described must be uniform, and therefore the body may be supposed to move along the same circle of latitude or upon the revolving cone formed by the minute horizontal planes on that circle being extended to a vertex in the axis of the sphere; but in neither case would the supposed circle be described. It is a false and misleading

representation, and will prove a stumbling block to many readers who attempt to follow the demonstration.

Although the deflective force represented by Prof. Hendricks' equation 2 is not a centrifugal force, it not being derived from any circle actually described on either fixed or moving surface, it is the same in quantity as the centrifugal force from a circle whose circumference is equal to the distance moved on the surface of the sphere during the period required for the line upon which the body moves to make a horizontal deflection of 360 degrees. Suppose a car to be passing rapidly over the center of a revolving turn-table; if friction were absent, there would be a horizontal pressure against the right hand or left hand rail as the rotation might be to the right or left, caused by the velocity along the circles of revolution being increased on one side of the center and checked on the other; both resulting in an equal pressure on the same rail. This is not a centrifugal force, but it is the same in quantity as if it were moving upon a circle whose accompanying tangent deflected at the same rate as that of the radius of the turn-table. The distance moves by the car upon the deflecting track in the horizontal plane during one rotation, is the circumference of the circle. In both cases the line upon which the body moves, the radius in one and the tangent in the other, makes a horizontal deflection at the same rate. Thus it was stated and demonstrated in the article which Prof. Hendricks has ignored. His equation 2, which should read $f = \frac{\frac{1}{289} \text{ W } 24\, v \text{ Sin. } \lambda}{2\pi \text{ R}}$ expresses it exactly, but he has not reached it by a correct philosophical reasoning.

The centrifugal force is in proportion to the square of the velocity divided by the radius in whatever manner that velocity may be produced. But Prof. Hendricks does not seem to acknowledge this where the rotary velocity in the circle of latitude has been increased by the eastern component of the independent velocity; for he estimates the centrifugal force on these two separately, taking the square of each and dividing by the radius, as if there were two separate bodies moving with these velocities respectively. The resulting force of the first, which is balanced by the form of the spheroid of revolution, being neglected, he adds to the second the centrifugal force of his imaginary circle. If this had luckily compensated for the loss resulting from not squaring the sum of the two, the actual velocity, his result would have been correct. But such is not the case.

For the sake of simplicity we will use only the variable factors which determine the centrifugal force.

Let V represent the rotary velocity, per day, of the sphere at the given latitude, and v the independent velocity. If β is the angle between the meridian and the line of the independent movement, the eastern component of it will be $v \sin \beta$. Then will the body move in the circle of latitude with the velocity of V + ($v \sin \beta$), the radius of the circle being the cosine of the latitude, λ; that of the great circle of the sphere being the unit.

The centrifugal force will then be represented by

$$\frac{(\text{V} + v \sin \beta)^2}{\cos \lambda}$$

$$\frac{\text{V}^2}{\cos \lambda} + \frac{2\,\text{V}(v \sin \beta)}{\cos \lambda} + \frac{(v \sin \beta)^2}{\cos \lambda}.$$

The first of these three quantities is not considered, because it is balanced by the spheroidal curve. To the horizontal component of the third Prof. Hendricks adds the force expressed by his equation 2. Unless this is equal to the horizontal component of the second, his result will be erroneous. This component will be $\frac{2\text{V}(v \sin \text{B}) \sin \lambda}{\cos \lambda}$. The sine of an angle being equal to the cosine divided by the cotangent, let it be substituted by $\frac{\cos \lambda}{\cot \lambda}$, then the expression will become

$$\frac{2\,\text{V}\,(v \sin \beta)}{\cot \lambda}.$$

Now to find the expression by the variable factors only, as before, which corresponds to equation 2, consider the cone described by a meridian tangent as developed into a plane forming the sector of a circle. The radius of this circle is cot λ and the arc of the sector is V, that is, the velocity per day; for it was described in one revolution. The distance passed over by the independent move-

ment during the same revolution forms an arc of the circle contemplated in equation 2. This distance is v, the independent velocity per day, and subtends the same angle as V. Therefore the radius of this circle is

$$V : v :: \cot\lambda : r = \frac{v \cot \lambda}{V};$$

and the centrifugal force is represented by

$$\frac{v^2 V}{v \cot \lambda} \text{ or } \frac{V v}{\cot \lambda}$$

which must be equal to

$$\frac{2 \, V v \sin \beta}{\cot \lambda}$$

if Prof. Hendrick's conclusion is correct. If the independent movement is at right angles to the meridian the sine of β becomes the unit, and the expression becomes

$$\frac{2 \, V v}{\cot \lambda}$$

which is double the quantity necessary to sustain the conclusion, and which is, therefore, incorrect.

The centrifugal force of the combined velocities, when the independent one is at right angles to the meridian, is all the force that can act at right angles to the line of movement, because it is the only one which can act in the plane of the meridian, and the horizontal component of this is the only force in question. But when this movement is not at right angles with the meridian, that component of the deflective force not absorbed in the centrifugal force acts at right angles to the line of movement. So that as the angle β diminishes both the second and third quantities in the expression for the centrifugal force diminish until they vanish when the movement is on the meridian, the remaining quantity being balanced by the curve of the spheroid. But as these disappear, those components of the deflective force which are at right angles to the meridian begin to act at right angles to the line of movement reaching the maximum when that is on the meridian, where equation 2 of Prof. Hendricks expresses the whole of the force in question, and where his conclusion is correct.

Now consider the case of the independent movement being to the west. The velocity is then $V - v \sin \beta$, and the centrifugal force is represented by

$$\frac{V^2}{\cos \lambda} - \frac{2 \, V v \sin \beta}{\cos \lambda} + \frac{(v \sin \beta)^2}{\cos \lambda}.$$

When the movement is due west the horizontal component of the second quantity becomes

$$- \frac{2 \, V v}{\cot \lambda}.$$

The deflective force of equation 2, when the movement is westward, is exerted towards the pole, and must, therefore, be considered a negative quantity when added to the centrifugal force, and will be represented by

$$- \frac{V v}{\cot \lambda};$$

which, as before, is only half of the quantity required to verify the conclusion.

Whatever may be the direction of the independent movement, the deflection of its line may be resolved into two components; one in the meridian and the other at right angles to it. The former reduces to zero when the movement is at right angles to the meridian where the deflective force of the independent movement is entirely absorbed in the centrifugal force. The latter is that portion of it which is not involved in the centrifugal force, and which is the entire quantity when the movement is on the meridian.

Considering the deflective force of the independent movement as unity, the meridian component is the sine of the angle β and that at right angles to it is the cosine. Therefore, the quantity representing this deflective force is to be multiplied by the cosine for the component which is not involved with the centrifugal force; which is

$$\frac{V v \cos \beta}{\cot \lambda}.$$

This is to be added to the centrifugal for eastern movements, and subtracted from it for western movements. So that the expression representing the unbalanced horizontal force acting at right angles to the line of independent movement is

$$\frac{2 V v \sin \beta + (v \sin \beta)^2 + V v \cos \beta}{\cot \lambda}$$

when it is eastward, and

$$\frac{-2Vv \sin \beta + (v \sin \beta)^2 - Vv \cos \beta}{\cot \lambda}$$

when it is westward. Here the minus quantities represent the forces towards the pole, and the plus quantities those towards the equator.

To obtain the numerical value of these forces the velocities must be reduced to feet per second and divided by $g=32.22$. And cot λ must be multiplied by the radius of the sphere in feet.

In the case of the pendulum there is no consideration of force involved. The question is simply the departure of the horizontal line fixed upon the surface from the plane of oscillation during one revolution of the sphere. And this is the sector developed from the cone described by the meridian tangent at the latitude of the pendulum; or $360 \times \sin \lambda$. The horizontal deflection of the fixed line corresponds to the rotation of the minute horizontal plane from which the pendulum is suspended. But the effect of this is only to twist the thread and to cause the pendulous body to rotate upon its own axis at the point of attachment to the thread. But this has no effect upon the direction of the arc through which it swings; and there being no cause for a deflection of this arc about the normal, it makes none. Therefore, the horizontal line which necessarily deflects about the normal must depart from the arc of oscillation to the full extent of the rotation of the plane.

GAS BURNERS.*

By WILLIAM SUGG, A. I. C. E.

From the "Journal of the Society of Arts."

Coal gas, as now supplied to the public, consists of hydrogen and marsh gas to the extent of about 80 per cent., and the remainder consists of luminous hydro-carbons of various qualities, with traces of sulphur in the form of disulphide.

The gas supply of London is watched over by a Commission, appointed by the Board of Trade, called gas referees, who are Professor Tyndall, Professor Vernon Harcourt, and Mr. Pole. They are empowered to prescribe the manner of verifying the illuminating power and purity of the gas, and they have also authority to act in cases of dispute in public lighting. Their prescriptions, as emanating from the only legal body of the kind in the Kingdom, are accepted as the proper methods, so that in effect, though they are appointed in London, they may be said to be gas referees for the whole kingdom. But the use that is made of gas is a matter which is altogether out of the control of gas companies and their engineers, and here the proverb about food and bad cooks may be paraphrased. The great hinderance to the improvements in the use of gas has been the general public themselves, who have not taken sufficient interest in the kinds of apparatus employed, but have, as a rule, supplied themselves with the cheapest burners, stoves and other apparatus, utterly regardless of the waste and annoyance which this system entailed; and when they have complained, they have always attributed the causes of failure to the quality or pressure of the gas, instead of aacribing it to the true cause.

In 1871, the gas referees made an examination of a number of gas burners which they had collected from various large estabiishments, newspaper offices, warehouses, shops and dwelling houses, and they found that those burners (samples of those generally employed by the public) would only give about one-half the light that the gas was capable of yielding per cubic foot consumed, and several of the burners tested by them gave only one-fourth of the proper light of the gas.

They say in their report to Parliament: "The economy to the public, arising from the use of good gas burners instead

* A paper read before the Mechanical Section of the British Association.

of bad ones, is so obvious as hardly to need remark. The gas-rental of London amounts annually to more than two millions sterling. Taking a very moderate estimate, upwards of one-fourth of this sum (£500,000 per annum) might be saved by the use of good burners. This is the saving which might be made in London alone; how much vaster the sum thus economized if good gas burners were to come into general use throughout England."

Now, the quantity of gas used in London last year, according to the analysis of the London Gas Companies' accounts, prepared by Mr. John Field, was, in round numbers, 20,230,000,000 cubic feet, which is equal to a bulk of mile square × 726 feet high, and its cost to the public was £2,911,000.

The result of careful trials, made with a number of burners taken lately from private houses and shops, shows that, as a rule, the amount of light obtained by the general public, from five cubic feet of gas, is less than one half of that which it is capable of giving. The iron and metal burners, of which a great many are used, give the best result in light when they are worn out, although the shape of the flame is bad. The reason why, is that in order to obtain from what is a compound of hydrogen, marsh gas and carbon, its best effect in light, the burner must be so made that the quantity of gas required to be consumed is proportionate to the size of the burner, so that it cannot exceed the maximum quantity which the burner is made for. Then the outlet of the burner itself, whether it be of the Argand or the flat-flame form, must be so arranged that the gas issues forth at a sufficiently low rate of velocity, so that it has time to get heated to a proper degree by the hydrogen and marsh gas before it comes into combustion with the oxygen of the atmosphere. When this rate of velocity is obtained in the Argand burner, the pressure at the point of ignition is almost *nil*. In flat-flame burners the pressure of the gas must be raised to a point sufficient to blow out the flame to a fan-like shape, but it must only be sufficient to do this if it is desired to obtain a good result per cubic foot of gas consumed.

One more point is of great importance in the construction of a gas burner—that is, that the gas should not be heated until it arrives at the point of ignition. The body of the chamber below the point of ignition must, therefore, be made of material which is a bad conductor of heat; so as not only to prevent the undue expansion of the gas before it arrives at the point of ignition, but also to maintain heat in the flame.

Sir Frederick Bramwell very ingeniously pointed out, some time since, that the important point in the proper combustion of gas is not so much to keep the gas cool as to keep the flame hot. The distinction is extremely subtle; but, nevertheless, a non-conducting gas-chamber performs both these important functions. If a gas-chamber made of metal or any good conductor of heat is used, then the gas becomes expanded in bulk, and the velocity of the issuing gas is greatly increased; less time being given for chemical combinations necessary to produce a proper amount of light from it. In addition to this, as Sir Frederick Bramwell has pointed out, the heat which should remain in the flame is conducted away from it into the lower fittings of the burner, where it may burn the fingers of the incautious consumer, but it is of no manner of use in the evolution of light.

You will, perhaps, say it is not possible to conceive how the flame can be kept hot without keeping the gas cool at the same time, because if the heat is to be kept in the flame, and not conducted away down the stem of the burner, the gas must be kept cool by the means employed. But, nevertheless, there is a difference in the effects produced, and the ingenious definition of Sir Frederick is scientifically accurate. As an explanation of the expansion theory, it may be stated that if one cubic foot of gas is heated to about 500° Fahr., it will occupy the same bulk that two cubic feet do at the mean temperature of the atmosphere. Now, supposing it is issuing in a cool state from the aperture of the burner at a velocity equal to one and a half miles per hour, it must then issue at the rate of three miles per hour, if the rate of consumption is equal in both cases. Therefore it has only half the time in the heated state to combine with the oxygen of the air that it had in the cooler state, and a loss of illum-

inating power is the result. Again, as regards the maintenance of the heat in the flame; supposing the chamber from which the gas issues is a conductor of heat. In this case the heat from the flame is conducted away from it down the stem of the burner, expanding the gas and leaving the flame so relatively cooled, as to require more gas to raise it to the necessary state of incandescence to allow the oxygen to combine with it in the proper proportions. In this case, as the two operations are simultaneous, it is difficult to apportion the effect produced. But that the effect is produced is proved by the following experiments, made some time since by the late Mr. F. J. Evans.

Two Argand burners were made precisely alike in every respect, except that one had a combustion chamber made in steatite, and the other in brass. The same quantity of gas was consumed through both of these, the result being that the burner with the non-conducting chamber gave more light per five cubic feet of gas consumed than the other, the proportion being as fifteen candles to thirteen. The burner with a non-conducting chamber was quite cool immediately below the chamber, while the other was so hot that it could not be touched without burning the hand. The metal burners which are now used in enormous quantities in London and the provinces become exceedingly hot, so much so as to communicate the heat to a considerable distance down the fittings. The velocity of the issue of five cubic feet per hour of gas from these burners, varies from ten to sixty miles an hour. The worn out burners generally give the lowest velocity. As a rule, the metal burners give the lowest result in light per cubic foot of gas consumed.

For example, a metal burner of the flat-flame type, which has been stated in the columns of the *Journal of Gas Lighting* to be identical with the steatite hollow-top burner, invented in 1868, gives the following results: A large-sized burner, No. 8, burning five cubic feet per hour, gave a result equal to 11.5 candles, whilst the result obtained with a like quantity of gas from a steatite burner of corresponding size, which has a non-conducting gas chamber, was 14.6 candles, a difference in favor of the steatite of 3.1 candles, or nearly 25 per cent. more light. Another metal burner, of a size more generally in use by the public, only gave 6.2 candles for the five cubic feet, or considerably less than half the latent value of the gas, which was 16 candles.

The fact of the difference of illuminating power, with like quantities of gas, clearly shows that the two burners are not the same by any means. The statement gravely made in the columns of the *Journal of Gas Lighting* that they are identically the same, clearly shows that if this is the belief of the gas trade generally with respect to these two burners, it is no wonder that the public, who rely on the recommendations of the trade, continue still to waste their gas in the manner pointed out by the gas referees in 1870 and 1871.

As before stated, careful tests of a collection of burners, bought from different gas-fitters and iron-mongers in various towns in England, and from their recommendation, prove that the knowledge of the proper use of gas possessed by these persons is still most incomplete, and therefore, the general public continue still to burn gas in the same wasteful manner as they did when the gas referees made their report.

The remedy for this enormous waste of gas is in the hands of the public only. Gas producers, whether they are corporate bodies or public companies, are almost powerless to oppose the vested interest which derives large profits from the sale of gas burners constructed with a view to require frequent renewals. It is only fair to say that the producers of gas have always shown the greatest interest in the improved use of gas in every way; but the speculating builder and his colleague, the local plumber and gas fitter, hold a final power for evil over the employment of gas, which, till very lately, has been paramount.

Happily for the gas interest, the general public are beginning to take a deep interest in gas, and are acquiring a great amount of information concerning it, through the numerous gas exhibitions which have been held under the auspices of gas companies and corporate gas committees throughout the kingdom, and it is to be hoped that the forthcoming exhibition at the Crystal Palace, in October next, will very largely aid the good work

of instructing the public how to use gas to the best advantage. Thus we may hope that soon the ring of interest inimical to the progress of gas will be broken through and fresh encouragement given to the inventors of improvements in the use of gas. It will be impossible, in the limits of this paper, to give you a complete idea of all the improvements which have been, and are still being made: but I propose to indicate the direction they are taking, and give you a general idea of what they are.

It has been found that a comparatively large quantity of gas, about four to six times that ordinarily used, will give a much better result from one burner per cubic foot of gas consumed than can be obtained from four or six separate burners consuming the aggregate quantity of gas equal to that consumed by the large one; and this is true of both Argand and flat-flame burners. This is by no means a new idea; it was known to Faraday and others before him. I have in my possession an old burner made many years ago; it has several rings, and a silver top drilled with very fine holes. The quantity of gas used is large—the effect is small—in fact it is more useful for boiling water than for giving light. But this old burner is a type of a large Argand of twenty or thirty years ago. They did not succeed, because, although they produce a great amount of light, it was at the cost of too much gas. Modern Argands will produce just double the amount of light for the same quantity of gas.

There is also incorporated in some of the newest burners now before the public an idea which was originated by Professor Frankland not more than ten years ago, viz., that if the air for combustion supplied to a burner be heated before it arrives at the point of ignition, a much better result per cubic foot of gas consumed can be realized. This, you will perceive, is a mode of carrying out Sir Frederick Bramwell's idea of keeping the flame hot, and undoubtedly a better result can be obtained. This burner, although it did not come much into use, has very lately been repeated, and is now being sold on the Continent. The burner is so much like Dr. Frankland's that there is no difficulty in recognizing it at once.

Of the modern Argands there are now several kinds; one is made with two or three concentric rings of flame and a glass chimney, and is made with non-conducting steatite gas chambers, and apertures permitting the gas to issue under an almost inappreciable pressure at the point of ignition, the velocity per hour being only about $1\frac{1}{2}$ miles. In this kind of burner the gas is kept cool and the flame hot. These were first used in the public lighting of Waterloo Road, in 1879, and in Waterloo Place and Queen Victoria Street.

Another kind, of newer type, is made on the theory of keeping the flame hot by making use of the products of combustion to heat the air supply. This also combines the low velocity of emission theory, and likewise heats the gas itself. It is made by inverting the flame of the burner, the heat generated by the products of combustion, being carried away by a concentric flue, fastened around the burner, through which metallic tubes convey the air necessary to produce combustion, which thus becomes heated.

A third is constructed on the principle of keeping the flame hot and the gas cool, but has besides an arrangement for admitting separate currents of cold air around the flame, for the purpose of keeping the chimney cool. It was important to observe that, although this burner does not warm the air admitted to it for promoting combustion, yet the results per cubic foot of gas consumed are stated to be as high as any of the others, showing clearly that there is no advantage in heating the gas before combustion. As to the advantage obtained by heating the air, the practical effect upon an Argand suitable for use by such ordinary unskilled labor as it usually employed to look after gaslights, has not yet been clearly demonstrated; although it is, without doubt, a great advantage to burners of the flat-flame type, because these have always too much cold air supplied to them.

The Argand, with its more complete regulation of air, and its immunity from the effect of surrounding cold air, is able to evolve from 15 to 30 per cent. more light per cubic foot of gas consumed, than can be obtained from the best flat-flame burner; but, although the general public have no objection to glass chimneys in

paraffin and other oil lamps, they do not appear to look with favor upon the general employment of glass chimneys for gas-burners, no matter how much better the result to be obtained. I need not say that the cordial support of the ring inimical to the true interests of gas is given to the public on this point, and a great deal of very strong literature condemnatory of the Argand burners has been widely disseminated. But it is impossible, in the face of the improvements which are continually being made in gas-burners of the Argand type, to believe otherwise than that they are destined to play an important part in the gaslighting arrangements of the future. The improvements in the flat-flame burners, though not producing such high results as those obtained from large Argands, are great, and concurrently with improved lanterns, have placed the lighting of the public streets on a much improved footing. Here, again, about three times the amount of light per cubic foot of gas is obtained from the use of large burners than with the old-fashioned small ones. The consumption of gas by the large ones is only equal to the aggregate consumption of four or six smaller burners. In addition to this the improvements in the reflecting tops of the lanterns make the new lamps still more effective, and may fairly be said to double the effective power. In internal lighting, the progress of gas has been very considerable of late years. Small burners for rooms have been greatly improved. For large rooms and theatres new kinds of sun-burners are made, to give three times the amount of light obtainable from the old ones, and to ventilate the buildings at the same time.

One of the greatest advantages of gas is that the heat generated by the combustion can, if properly applied, be made to do the work of ventilation, and it is in this direction that the future progress of gas-lighting lies. There are many ways of utilizing this heat; some are extremely easy to put into practice, others require more preparation. Among the simplest is the method of ventilating rooms by the fish-gill ventilator, invented by the late Goldsworthy Gurney. It consists simply in covering an opening made in the wall with strips of calico fastened across the whole by tacks put into the two upper corners of each band. The bands are made just long enough for the lower part of the superior band to cover the top part of the inferior one. When fixed properly they open like the gill of a fish, hence the name; they can be used to let in fresh air, or carry off heated air from the top of a room.

These useful and simple ventilators, if employed in rooms where gas is used, would tend greatly to the comfort of the public who require a good light, but complain of the resulting heat. They work when closed by diffusion, the heated air passing through the porous medium of the calico, and the cooler air from outside the rooms passing in without draught. For the ventilation of ball-rooms it is very easy to put into the windows a frame fitted with muslin or washed calico of half or even the full size of the window. Ventilation will thus take place by diffusion, and the draughts and danger resulting from incautiously-opened windows will be avoided. If the wind blows hard on this opening it may be protected by a loose curtain of muslin or calico hanging in front of it.

You will perceive, therefore, from what I have said, that the progress of invention in gas-lighting is great and continuous, and that in the future, if the public will only interest themselvès just sufficiently to obtain a moderate amount of information on the subject of gas, they will be enabled to use it with great economy and comfort to themselves in every way. The facile manner in which gas can be employed to produce the light of a rushlight or the blaze of a thousand candles by the mere turn of the wrist, joined to the readiness with which it can be conveyed to great distances without any practical loss, will always ensure a large and growing demand for it everywhere.

But it must be remembered that its extreme adaptability renders it capable of being used with the most crude apparatus as well as the most perfect; and when we see in the public streets the blazing pipe and shovel, a rough but powerful burner of from 100 to 600 candles power, rigged up in a few minutes by a navvy, we must not be surprised at the prevalence of crude apparatus of smaller dimensions put into practice by the public, who are not aware that they can do better with a more perfect burner.

LUBRICANTS.*

Proceedings of Society of German Engineers.

Lubricants, as is well known, are used for reducing the friction of the moving parts of machinery to the lowest possible degree, thereby preventing undue wear and tear, and, at the same time, obtaining the greatest possible amount of work from the machinery. Owing to the extensive introduction of mechanical power during the last thirty years, the consumption of lubricants has grown in proportion, until it is now enormous. Lubricants are supplied to us by all the three kingdoms of nature. We derive tallow and train-oil from the animal kingdom; olive oil, rapeseed oil, palm oil, and cocoa-nut oil, from the vegetable kingdoms; resin oil and intermediary oil, so to say, between the vegetable and mineral kingdom; and mineral oil pure and simple, from the mineral kingdom.

Tallow is prepared from the fat of cattle and sheep by being heated with water, sometimes with the addition of diluted sulphluric acid or caustic soda, either by the direct application of heat or by hot steam. By this treatment the cellular tissues are destroyed, and the pure fat is separated, which then settles as a layer on the surface. Tallow thus prepared forms, at an ordinary temperature, a yellowish white, pretty hard mass, which melts at a temperature of + 40° C. It contains, if not freed from acids by alkalies, besides the neutral fats, from 1 to 5 per cent. of sebacic acid. In examining tallow, care should be taken to ascertain whether it is free from cellular tissues, free from mineral acid, as well as extraneous additions (other fats, sebacic acid, mineral substances, &c.)

Train-oil, prepared from the fat of seals, &c., in a manner similar to tallow, forms, at an ordinary temperature, a liquid of a light or dark brown color and of a peculiar odour, solidifying at a temperature of from + 5° to + 15° C. As a rule it contains a large proportion (up to 5 per cent.) of sebacic acid. The same care should be observed in examining train-oil, as is recommended in the case of tallow.

Olive oil is obtained by pressure from the fruit of the olive tree, cultivated largely in the South of France and Italy. It forms a beautifully yellow liquid of a peculiar odor, and a mild taste, which begins to get thick at a temperature of + 2° C. The contents of free sebacic acid is very small in the better descriptions; inferior sorts contain about 0.5 to 1 per cent.

Rapeseed oil or rape oil is procured from the seed of the various kinds of Brassica, the seed being crushed in powerful hydraulic presses. The oil thus obtained is a yellowish-brown to brownish-green liquid, of a peculiar odor and a pungent taste, which precipitates the mucilaginous substances of the seed which have passed in pressing only after being kept in store for some time, and becomes clear by this precipitation. But even perfectly clear oil contains mucilage or albumen in chemical solution, and these can only be removed by treatment with chemicals or by heating up to 200° C. The raw oil only cleared by storing was, and is still, used for lubricating bearings, especially railway axles. The purified oil, prepared by removing from the raw oil the mucilaginous and albuminous constituents by sulphuric acid, chloride of zinc, &c., and the free sebacic acid (on the average 0.4 per cent.) by treating with alkalies, is reserved for lubricating the more delicate portions of machinery and locomotives, steam cylinders, &c. The perfectly purified rape oil, and from which all acid has been eliminated, is of a light yellow color, almost odorless, possesses a specific gravity of from 0.914 to 0.915 at 15° C., solidifies at a few degrees below zero, and melts again at about + 3° C. In testing rapeseed oil, it must be ascertained whether it possesses the correct specific weight and melting-point, and whether it is free from mucilage, sebacic and mineral acids.

Palm oil and cocoa-nut oil are obtained from the fruit of the oil and cocoa-nut palms respectively, imported in large quantities into Europe. The fruit is either crushed in hydraulic presses, or the oil is extracted by means of sulphuret of

* From a paper read by Herr Lux before the Society of German Engineers.

carbon. The two oils or fats (for at an ordinary temperature they resemble butter) are now used very little for lubricating purposes, but extensively in the manufacture of soap.

Resin oil, gained by distilling the common resin obtained from the residue left in the manufacture of oil of turpentine from turpentine, was formerly used only in the preparation of "carriage grease," mixtures of this oil with colophony, fats, &c. Refined resin oil is now procured by freeing the crude oil by means of alkalies, and subsequent bleaching, the product being a clear semi-liquid of yellow color, possessing but a slight odor, and having a high specific gravity (about 0.970). The oils are never perfectly free from organic acids, most of them containing about one per cent.

The class of mineral oils includes a number of lubricants which have nearly all a common origin with "petroleum." Crude petroleum, as it was first won in large quantities in America (since 1859), consists of an intermixture of many organic substances, which, on account of their exterior similarity to real oils, as well as for simplicity's sake, are called "oils." These oils, which may be distilled without decomposing, differ from each other by their various specific weights, as well as by their various boiling-points. For a long time the volatile ingredients only were extracted from crude petroleum, which were used for illuminating purposes, whilst little attention was paid to the heavy oils which remained after distillation. But when those residues grew in quantity, means had to be devised for utilizing them, and it was soon found that they could be used as lubricants. At first the oil was applied in its original form, as left in the alembic, in which it still contains many impurities. But now in most cases, after the more volatile oil used for lighting purposes has been distilled over (at a temperature of from 150° to 350° C.), the lubricating oil is also driven over. The latter is then purified with acids and alkalies ,and a product obtained which, in consequence of its impurity, chemical constancy, and other valuable properties, appears very suitable for use as a lubricant.

As the raw mineral oil consists of a large variety of combinations related to one another, and as those combinations. according to a greater or less degree of distillation, &c., may appear in the most various mixtures, it follows that, independent of their greater or less degree of purity, the mineral oils may possess widely varying properties principally of a physical nature. Thus the color of the different mineral oils varies from light yellow to dark brownish red; their specific weights fluctuate between 0.880 and 0.920; their cohesion,—their "body," as more generally expressed,—shows itself in all grades between the consistency, for instance, of the highly liquid linseed oil, when fresh, and the semi-liquid resin oil, &c. But, however much their physical properties may vary, in their chemical nature mineral oils are closely related to each other. They all consist (of course, only real mineral oils are included) of a mixture of carburetted hydrogen, indifferent organic combinations, which possess neither acid nor basic properties. They do not decompose either at very low temperatures or at degrees of heat which far exceed those prevailing in the steam cylinders, &c., where they are employed. They do not undergo any change either on contact with the air or with water or steam; they do not attack metals, even the most easily oxydisable, such as potassium or sodium, and are as little changed or decomposed by the metals themselves.

This chemical indifference is the principal advantage possessed by mineral oils over all fat oils, whether they are of vegetable or animal origin. All those fat oils decompose in time on exposure to the air, at high temperatures, on contact with metals or their oxides, and thus destroy, sometimes more quickly, sometimes more slowly, the parts of machinery which they are intended to preserve.

Notwithstanding those great excellences which mineral oils have over fat oils, great difficulties were at first experienced in introducing lubrication with the former more generally. Convenience, attachment to custom, and want of intelligence were amongst the obstacles which had to be overcome. Mineral oils were looked upon as entirely unsuitable for lubricating machinery, and fat oils as alone possessing the specific property of a lubricant; apprehensions were raised as to the "easy inflammability" of min-

eral oils, &c. But the introduction of mineral oils for lubricating purposes was also greatly retarded by the want of sense, and partly also by the want of honesty, on the part of individual producers and dealers. Properties were claimed for the crude mineral oil only possessed by carefully purified oil; when finally the price of the lubricating oil exceeded that of the oil used for burning, part of the latter was left in the lubricant, so that, especially if great pressure took place, it was found unsuitable. Notwithstanding all this, lubrication with mineral oil has, within the short space of the last five years, made such progress that it may justly be called, not only the lubricant of the future, but that of the present day. As the mineral oils on account of their chemical properties, are far more valuable than fat oils, and, on the other hand, owing to greater variety physically, they may be adopted more readily for different purposes than fat oils, there is nothing to prevent their general introduction.

It will be gathered from the foregoing that the displacement of fat oils for lubricating machinery by mineral oils is a great technical progress. But the use of mineral oil is a great advantage also from the point of cheapness. The best mineral oils are now only half the price of fat oils. With suitable construction of the parts to be lubricated and a correct choice of the most suitable lubricating materials, the consumption of mineral oils is not greater; on the contrary, it ought to be less, as with mineral oil no hardening or thickening takes place, and thus there is no loss. Finally in lubricating with mineral oil, the parts oiled are not destroyed, but, on the contrary, preserved; whilst the destruction of machinery parts, such as pivots, regulator valve-rods of locomotives, &c., is only too often a consequence of the use of fat oils.

But the ultimate introduction of lubrication with mineral oils is of importance also from an economical point of view. Our population is steadily increasing, and the difficulties of gaining a livelihood are growing with it. On the other hand, there is an abundance of suitable material for lubricating with mineral oils, for, without reckoning the almost inexhaustible stores in America, petroleum is now found in large quantities also in Russia, Galicia, and Germany. Our food supply would be greatly extended, either directly, by appropriating large quantities of fat and oils for the maintenance of the people, or indirectly, by restoring the areas now used for the cultivation of oil-producing seeds for raising cereals. It is highly necessary in the economy of nations that there should be, not only a division of labor, but also a classification of the work to be done. Let the inferior materials, as far as they can, be used for inferior objects, whilst the more valuable products are reserved for a higher purpose.

STEEL VERSUS COMPOUND ARMOR.

From "The Engineer."

We have now a good deal of evidence before us as to the conclusions arrived at in Italy, Russia, and Denmark, on the results of the recent competitive trials made in those countries. We gave a translation of an Italian report on trials made at Spezia, subsequent to those reported by us at the end of last year. The speech of his Excellency the Minister of Marine, Rear-Admiral Acton, in the Chamber of Deputies at Rome, which we have before us, deals more fully with the entire question, and cites conclusions obtained from Russian authorities; and in the *Army and Navy Gazette* of June 16th is an article containing extracts from the Danish Ordnance Select Committee Report. These may well be taken together. Admiral Acton admits that photographs giving the views of the plates tested at Spezia might give an impression very unfavorable to the compound plates to any one who did not know the circumstances connected with the bolting; in fact, who did not know that six bolts were matched against twenty. He explains that subsequent trials on the fragments of plate with

medium guns showed a great advantage on the side of the compound plates. He then dwells on the different natures of the resistance of compound and steel plates. The former has a very hard face, which breaks up projectiles in a remarkable way, and would have a still greater advantage in this if struck obliquely. Its soft foundation plate, however, requires rigid backing, such as it did not get at Muggiano—that is, at Spezia—but such as it would have in the side of any vessel.

The steel armor is actually penetrated and breaks up under repeated blows from projectiles of medium caliber. The Admiral remarks that in preferring compound armor to steel, Italy is in good company with England, Germany, Austria, and Russia; indeed, France herself continues to employ compound as well as steel armor. Recently some magnificent Krupp steel projectiles practically produced no better results against compound armor than the Gregorini chilled iron shot. On service it is held by Admiral Acton that a ship would much more frequently be exposed to the continued fire of medium guns than to even a single shot of a really heavy gun; and compound armor resists this continued fire well, while steel succumbs to it. He would be disposed to test compound and steel plates, respectively, by one or two heavy blows and a number of medium ones. After explaining how the particular tests employed at Muggiano were arrived at, and giving a list of English, Russian, American, and other ships, for which compound armor is to be employed, supplied from the firms of Messrs. Brown and Messrs. Cammell, he quotes from the Italian officer on special service at the St. Petersburg trials of March 29th and 30th last the following words: "The tests executed on the 8th instant upon the Cammell plate—third and fourth shots—are the sequel to those of the 24th of November. The projectiles broke up upon the plate without causing practical damage to the same, so that it would serve for two or three more shots, which will probably be executed upon it;" and, again, "Admiral Schwartz, head of the Russian artillery service, and Admiral Schestakow, Minister of Marine, notwithstanding the Spezia tests in November and those made in Sweden, is strongly of opinion that the compound plates are superior to the Schneider. I thought it well to refer these points to the officials of the marine with whom I spoke at the Polygon, among whom was admiral Stahl, head of the Testing Commission, and all share the opinion of Admiral Schestakow and Admiral Schwartz, that the compound plate is superior to the Schneider." Admiral Acton then finally gives it as his opinion that "there can no longer remain any doubt as to the real superiority of compound armor which we have adopted for Italy, which England, Russia, Germany, and the other Powers continue to prefer, and which even France adopts in competition with Schneider, without arresting its manufacture." The Danish Ordnance Committee, on the other hand, argue as follows from their experiments: "The plates have suffered in proportion to the number of shot kept out. The latter being the main point, it must be acknowledged that next to the steel plates, the greatest resistance has been rendered by the iron plates, which thus have discharged their duty best; whereas the compound plates are inferior to the other sorts in both respects."

It seems a strange thing that such different apparent results should be exhibited in different countries, especially that the Danes should prefer steel plates in the teeth of conclusions arrived at by Italy and Russia after experiments conducted on a much larger scale. The fact is that experiments and results can be considered from different points and with different objects in view. Let us see what features we are sure of in the condition of the question.

Speaking generally, the compound principle appears to have a great advantage in one respect. The body of the shield requires tenacity, while the face should be hard in order to break up the shot before they get their points in, and so receive support from the plate itself. In fact, qualities of different kinds are called for in the front and back of plate. In the case of a homogeneous mass of steel the best combination or compromise of qualities must be employed. In the case of the compound plate the metal suited to each part can be there employed, that is, the shield may have a hard steel face on a tough iron foundation plate. The solid steel plate then must have a

softer face than is desirable to secure the toughness required throughout. Its advocates probably, therefore, depend on the superiority of steel over iron in tenacity, on the advantage of the whole plate being stiff and less likely to bend than the wrought iron, and on the hammering to which it is subjected in making. As we have before stated, we believe that the advantages of the hard face and tough back of compound plates are very great; but we cannot help thinking some day hard and soft steel may take the place of hard steel and soft iron. Our present business, however, is with compound and steel plates as they stand, and we are glad to find that the judgment is strongly in favor of the former in almost all cases. Apart from the actual comparison of results under a fire which is at all a match for the armor, we feel with Admiral Acton, that thick armor is much more likely on service to be fired at by guns far below its power, and with projectiles striking it obliquely; and we know that under these conditions steel gradually shatters, while iron suffers very little, to say nothing of the advantage of the hard steel face. Of course, it must be imperative that the plate cannot be punched or perforated completely. In wrought iron it becomes possible that live shell might be driven through, but this we regard as out of the question with steel-faced or steel armor. We believe, then, we are doing well with compound armor.

With regard to the experiments themselves, we do not think that they are conducted always in a sound way, admitting of comparison with each other. In a recent article on "Hard Armor"—*Engineer*, May 4th last—we pointed out that, even in the most recent experiments against hard armor which cannot be perforated, shot are always matched against plates according to their power of perforation—which is misleading when the shot are employed in work of a different character. Against hard armor, including chilled iron, steel, and the thickest kinds of steel-faced plates, the shot may either drive their points slightly into the surface, or, as in chilled iron, chip a little of the face off, but their further action is that of a hammer breaking the plates across. No hole is made, and it is difficult to conceive that "penetrating power," which is work proportioned to the size of hole which might be made in soft plates, is the proper standard of comparison. Surely the total stored-up work, delivered on the point of impact as far as the shot holds together, is the standard to employ to compare shot fired against the same plate. Probably this is the standard employed by the Danish officers. There is more difficulty in comparing experiments against plates conducted on the different scales, such as those of Spezia and St. Petersburg. We have, in the article referred to, taken the striking energy per ton of metal in the plate. This supposes that the work of cracking plates through is in proportion to their mass, which is hardly likely to be correct. Nevertheless, it is probably as near the mark as anything we can at present suggest. By this it appears that the blows recorded by us in our reports of the Spezia and St. Petersburg trials had the following work in them per ton of metal in the plate struck. At St. Petersburg, taking the plates of 12¼ tons each, the heavier blows fired first had 8704 foot-tons energy, and the lighter ones, which followed, 5228 foot-tons each, or 711 and 427 foot-tons per ton of plate. So that the Schneider which was broken up and a quarter of the plate stripped off after three blows, had received 1564 foot-tons, while the Cammell-Wilson remained good after receiving 1991 foot-tons for each ton in the plate. At Spezia the plates weighed about 31½ tons each. Consequently, the lighter and heavier blows of about 21,000 and 33,800 foot-tons, respectively, gave on the plates about 666 and 1070 foot-tons for each ton of metal in the plate. After the second round, the Cammell and Brown plates were broken and stripped. Schneider's was really broken, but held up by the backing. Each had then about 1732 foot-tons per ton of plate. Schneider received two more blows, making a total of 132,000 foot-tons, or 4,200 foot-tons per ton of plate. Bolted as it was, it behaved excellently, and although fragments were hanging by single bolts at the top, it could not be said that much more than about a quarter of the backing was exposed.

It will thus be seen that the Spezia plates were subjected to a much heavier shock of impact than those at St. Petersburg; that is, to blows of 666 and 1070

instead of those of 427 and 711. Let us compare the best compound and steel plates. The Cammell-Wilson at St. Petersburg has now, however, borne 1991 foot-tons per ton, without any complete through fracture, whereas the Schneider, at Spezia, after 1732 foot-tons, that is, after the second round, was broken through in different directions. We could hardly say how many of the cracks went entirely through it, because it was surrounded by a frame, but we could see along its edge enough to be satisfied some did so. By this mode of comparison the compound Cammell plate has already borne more than the Schneider at Spezia. In favor of the latter, however, we must urge that it is much harder to make a good 19in. plate than one only 12in. thick, and we should like to see more trials on the larger scale.

OBSERVATIONS ON THE MOUTHS OF LAKE TRIBUTARIES.

By WALTER P. RICE.

From the Journal of the Association of Engineering Societies.

While this paper may seem somewhat in the nature of a collection of isolated facts, yet they are all links in the great chain of causes governing the regimen of the mouths of lake affluents, and as such are worthy of attention.

GAUGING THE CUYAHOGA RIVER.

During March last I conducted a series of observations on the volume and currents of the Cuyahoga, under direction of Col. J. M. Wilson. These observations, although limited, afford interesting data. I used the current apparatus adopted by General Ellis. It is made of tin, and consists of two parts, the principal or submerged float being an annular ring 8 inches high and 8 inches extreme diameter, with an air space of ½ inch, making inner diameter 7½ inches; the other, or surface float, is an ellipsoid 6 inches diameter by 1½ inches depth. The two parts are connected by a copper wire .036 of an inch diameter, attached to the center of figure of the submerged position by means of two brass cross-wires 4 inches from the top. Lead is applied to overcome the flotation, and in sufficient excess to bring about 8 ounces tension on the wire.

Observers who have used this apparatus state, that it is apt to assume a hexagonal condition from effects of water pressure in depths varying from 20 to 30 feet.

The instrument which I had constructed must have been of an inferior quality of tin, for it partially collapsed under pressure at a depth of only 9 feet, and started two sides of what would constitute an almost perfect heptagon if completed.

Currents.—The river was gauged a little south of the U. S. Life-Saving Station, the bottom being very uniform, and the greatest care taken to insure accurate results. The result is embodied in the following table:

VOLUME AND CURRENTS OF CUYAHOGA AT MOUTH.

Date.	Maximum current.	Volume of water.	Remarks.
	Feet per min.	Cubic ft. per m.	
February 15, 1882.....	49.5	91,063	Wind S. W., max. vel., 19 miles.
February 20, 1882.....	168.7	365,307*	Wind N. E., max. vel., 9 miles.
March 3, 1882.........	27.1†	56,653	Wind W., max. vel., 13 miles.

* NOTE.—Rainfall of 0.8 inches, February 19.
† On March 3 the maximum velocity is only 5.4 inches per second, a velocity insufficient to hold sand in suspension.

The term mouth, as applied to a river, is most certainly a happy expression. Take any of the rivers emptying into Lake Erie, for instance, we find at the mouth respiration as in man. Irregular breathing, if we may be allowed to use the comparison, a flux and reflux of breath, currents and reverse currents at certain seasons, and, under certain conditions, pulsations as frequent and irregular as those of a sick man. As an illustration. In attempting to obtain a set of observations with regard to the discharge of the Cuyahoga, February 11, 1882, the following was the result of four observations in the central sub-section of the river:

During an interval of forty-five minutes, with a rise .075 of a foot in the water level the mid-depth velocity declined 75 per cent., the loss of velocity being 50 per cent. for the last 15 minutes of the interval. Any rise of water from winds off the lake backs the water up in the river and produces just such results. For this reason, to obtain the best results in gauging such a river, the section selected should be far enough removed to be independent of the movements of the lake.

The experiments on the Cuyahoga developed the fact of the existence of an eddy. The stream suffers a contraction at the Lake Shore & Michigan Southern Railway draw-bridge, with corresponding increase of head or velocity. After passing this point there is a decrease of velocity and consequent loss of head, and the corresponding mechanical effect is employed in working on the elements of the more slowly moving current below, and in the formation of an eddy. The abutment takes up about 25 feet of the waterway. The divergence of the currents takes place about 22 feet from the dock, and the eddy, as determined, agrees with and affords a fine practical confirmation of the laws governing the dynamics of fluids.

The submerged float being set for a depth of 9 feet was released at the mouth of the Cuyahoga, and followed the path marked out, the wind shifting between east and southeast with a velocity of ten miles an hour. After leaving the protection of the pier, the curve of the current is normal to the direction of the wind force. This shows the strong inclination of the river to flow to the eastward, even with a strong opposing force. As an approximation, the average velocity of the current over the path indicated was about 9.1 feet per minute.

DYNAMICS OF LAKE ERIE.

Under this head we shall notice the movements of the lake as all such changes of level necessarily affect all tributaries in the vicinity of their mouths.

Effect of Long-Continued Winds.—The rise and fall of the water from this cause is sometimes very great; winds blowing off shore lowering the water level, and winds blowing off the lake heaping up or raising the level. The action of the water is sometimes several hours in advance of the coming wind, and offers sure data for predictions. At the upper end of Sandusky Bay I have observed the water fall to such an extent that tugs which were in active operation on Friday, November 12th, 1880, had to be propped up on Sunday, the 14th, this being the effect of a three days' blow from the southwest, the water having fallen between four and five feet. I have noticed almost as great a fall in Maumee Bay, which is very similar in its characteristics. I have also noticed a change of level partly due to westerly wind and partly to contraction, between the river and lake at Port Clinton, within 1,000 feet of the end of the pile revetment; the water on the west or river side of the revetment being $3\frac{1}{2}$ inches higher than on the east or lake side.

Annual Fluctuation.—There is an annual fluctuation of the water level of the lake in harmony with the laws of evaporation and rainfall, high water occurring in June or July, and low water in January or February. These months seem to represent the extremes of all atmospheric phenomena, including even earthquakes. The wonderful unity of action of all these forces is shown by the series of annual profiles. The amount and character of the annual fluctuation of the lake is dependent upon the atmospheric condition.

Pulsations, or "Seiches."—Besides the decided change in water level, directly due to the action of winds on the lake, there are certain periodic pulsations or throbbings of the lake and mouths of affluents certainly not due to this cause.

We must search elsewhere for an explanation. The solution which occurred to me is as follows: Conceive Lake Erie to be the reservoir or bulb of a gigantic barometer, of which the affluents for a certain distance from the mouths constitute the stems. Here we have a most sensitive barometer, the readings of which may be taken from the water gauge. The rarity or density of the atmosphere pressing upon the lake's surface being the direct cause of the pulsations. I believe the water gauge to be the true index to this barometer, and if we had sufficient data we could determine the condition of the atmosphere from the reading of the gauge. These pulsations, as a general rule, are small. I suspect from the reason that the storm centers avoid the lake, going north or south of it; but in the case of a rapidly moving storm area over the lake I have no doubt these harmless pulsations would develop into a great change of level, and that it would be as much if not more dependent upon the varying pressure of the atmosphere than upon the attendant storm winds. I offer this as explanatory of the tidal waves sometimes seen on the great lakes. After writing the above I found Col. Whittlesey had noticed these pulsations, and he speaks of them as follows:

"There is a sudden flux and reflux, which is completed in a few seconds, or minutes; sometimes due to storms, but more often cannot be traced to any cause. These oscillations are not yet explained; they occur on all the lakes and upon other bodies of water, causing a rush into the mouths of rivers, generally of a few inches in height, but sometimes several feet."

Upon further search I find that this same phenomenon has been noticed on the Lake of Geneva and the Baltic, under the term "Seiches," and Schulten has demonstrated the direct connection between the "Seiches" of the Baltic and the height of the barometrical column.

"When pressure of air diminishes, the water begins to swell (Seichie). When barometer again rises, the surface of the sea sinks; the movements of the water are always a few minutes earlier than those of the instrument, on account of the greater mobility of the aqueous particles."

I make the above quotations as *apropos* and a confirmation of my views. It seems as if careful observations on the great lakes, with self-registering water barometer and gauge in connection, with the meteorological information obtained by the Weather Bureau, could not fail to be productive of results most satisfactory in the development of the law of storms and atmospheric disturbances.

The disturbances of water level in the lake might be classified as follows:

I. "Seiches," or water swelling.
II. Lunar tide.
III. Fluctuation directly due to wind.
IV. Annual fluctuation.
V. Secular.

Of these disturbances, I., III. and IV. are important factors as affecting the regimen of lake tributaries at their mouths.

Ice Gorge at the Mouth of the Cuyahoga.—In the spring of 1881 the ice gorged at the mouth of the Cuyahoga, flooding the docks in some places and causing great apprehension. The action of the gorge affords an interesting study. Before its formation the water possessed an average depth of 17 feet at the ends of the piers. The river, in obedience to its laws, attempts to sweep to the eastward over its accustomed course; but in this direction it must of necessity pass over its shoal, which, serving as a nucleus for the collection of ice, offers opposition, and throws the path of least resistance to the westward.

The aqueous wanderer therefore makes another turn and passes out to the westward. The latent force of the accumulated waters is dissipated by the work of cutting out a new channel.

Formation of Bars and Beach.—The general tendency of rivers along Erie's southern shore, and entering normal to the coast line, or north and south, is to sweep to the eastward in a curve more or less modified by the winds. This is due to the fact that the westerly wind is the prevailing one and to the downward flow of the lake. In the rivers of this class and of the size of the Cuyahoga, Grand, Ashtabula and others, the greatest shoal will be found in or near the prolongation of the east pier, and at distance dependent, of course, upon the volume and velocity, in figures, from 200 feet to 500 feet from end of pier. The west wind is

therefore quite an ally to the engineer, and probably a disappointment to dredgemen.

In regard to the making or advancing of shore lines, the sand collects on the windward side of the piers when exposed to the prevailing wind, or when the topography of the coast line destroys the latter to the windward, considering the wind having the greatest scope of sea.

Marshes.—Most of the tributaries of Lake Erie along the southern shore are marshy at their mouths. While these marshes at first thought seem to be of no earthly use except as active agents in the dissemination of malaria and gratification of sportsmen, yet they play a very important part in the regulation of these streams in time of flood. The marsh is to the river what the governor is to the steam-engine. They are the natural storage reservoirs for surplus water. It may be well to note the fact that the amount of evaporation is greatly increased by the spreading of this dangerous excess over the vast area of the swamp land. It is evident that the reclamation of these low lands will have the effect of raising the water level and changing the regimen of the adjacent stream.

ON A NEW METHOD OF SINKING SHAFTS IN WATERY, RUNNING GROUND.

By WILLIAM GALLOWAY.

From "Nature."

When an attempt is made to sink a shaft in very watery deposits of gravel, sand and mud in the ordinary way—that is, by digging out the solid matter by hand, and pumping the water to keep the bottom dry—it is found that, after a certain depth has been reached, the current of water which flows up through the bottom brings solid matters along with it as fast as they can be removed, and further downward progress is then completely arrested. Under these circumstances it is necessary to resort to certain special methods of sinking, two of which have been hitherto employed with more or less success. According to one of these methods the shaft-lining consists of an air-tight iron cylinder fitted with an air-tight cover. When the excavation is continued below the natural level of the water, compressed air is forced into the interior of the shaft so as to drive back the water, and leave the bottom dry. The workmen can then stand in the bottom and remove the solid matter by hand as easily as if the ground had been naturally free from water. The lining sinks downward as its lower end is laid bare, and is lengthened at the top as required. The pressure of the air is gradually augmented as the depth increases, but unfortunately this process cannot be carried beyond three atmospheres without prejudicially affecting the health of the workmen. When the depth of the watery running ground surpasses the limit represented by a pressure of three atmospheres, it is necessary to resort to the second method. In this case the water is allowed to stand at its natural level in the shaft, and the solid matters are removed from the bottom by a revolving dredger. The lining or casing consists of a cylinder of masonry or iron provided with an iron shoe or cutting ring, and sinks downwards at first in virtue of its own weight, being lengthened at the top as in the previous case, but after a time it generally becomes necessary to force it down by the pressure of screws, assisted by the blows of an instrument resembling a pile-driver. When it cannot be made to sink deeper, another similar cylinder of smaller diameter is introduced into its interior, the same series of operations are again gone through, and so on until the solid ground is reached.

Simple as the last-described process may appear, its application is sometimes attended with difficulties of almost incredible magnitude. As an example we may mention two shafts which were sunk through about 400 feet of the kind of ground in question at the Colliery Rheinpreussen, near Ruhrort in Ger-

many. One, begun in 1857, was not finished after more than eighteen years' constant perseverance; while the other, begun in February, 1867, was only completed down to the solid ground in June, 1872.

The new method invented by Herr Poetsch is described by Bergassessor G. Kohler in the *Berg und Huttenmannische Zeitung*, No. 38, xlii. *Jahrgang*, September 21, 1883. It consists in freezing the water contained in that portion of the running ground which occupies the position of the intended shaft into a solid mass of ice, and then sinking through it by hand without having to pump any water. To this end a preliminary shaft of larger dimensions than the intended shaft is sunk down to the natural level of the water. A number of vertical bore-holes, about one meter apart, are then put down round about its sides at the bottom, so that they pass through the ground just outside the lining of the intended shaft. Others are put down within the area of the intended shaft, and one is put down in its center. All of these bores are continued down to the bottom of the running ground. They are made by means of the sand-pump, and are lined with sheet-iron tubes in the usual way. A circular-distributing pipe with small copper tubes branching from it is placed at the bottom of the preliminary shaft. One copper tube extends to the bottom of each bore-hole, and each tube is provided with a stopcock. At the surface are several ice-making machines of the Carré type. The liquid intended to circulate through the bore-holes and effect the operation of freezing consists of a solution of the chlorides of magnesium and calcium, whose freezing point lies between — 35° C. and — 40° C.

By means of a small force-pump it is made to circulate at such a rate that it leaves the cooling-trough with a temperature of about — 25° C. It descends into the distributing pipe, passes through the copper tubes to the bottom of the bore-holes, ascends outside the copper tubes to the top of the bore-holes, finds its way into the collecting-tube, reascends to the surface, passes through the cooling-trough, and then commences the downward journey again.

Herr Poetsch estimates that, under ordinary conditions—that is, when the outer ring of bore-holes can be made in the ground outside the lining of the intended shaft—the freezing process will occupy from ten to fourteen days.

When it has been ascertained by means of bore-holes that the wall of ice round about the intended shaft is thick enough, the operation of sinking is commenced. The ice is cut out by hand, and a descending cylinder of masonry or iron is carried down at the same time. The lining prevents the surrounding ice-wall from breaking inwards, and the bottom cannot burst upwards.

Herr Kohler made a personal inspection of this process at the shaft Archibald, now being sunk to the lignite beds, at Schneidlingen, in Germany. The shaft passes through a bed of running sand four meters thick. Twenty-three bore-holes were employed in two rows near its sides. The freezing process was completed on August 10th last, when the running sand had become a compact mass of such great hardness that no impression could be made on it by the finger-nail, and it was with considerable difficulty that a flake 15 mm. thick could be broken from it.

Sufficient data do not yet exist for estimating the cost of this process as compared with those already known, but we are of opinion that if the operation of freezing can be effected in two or three weeks, or even months, it will compare favorably with them in this respect under almost any circumstances. We believe also that it is capable of application under a variety of circumstances not mentioned in Herr Kohler's article, such as damming back an excessive flow of water on solid ground, driving horizontal drifts or tunnels through mud and sand, and so on.

We would, therefore, recommend the inventor rather to turn his attention in this direction than to think of condensing the intake air of mines by the application of cold, with the view of dispensing with ventilating furnaces, and enabling winding operations to be carried on in upcast as well as in downcast shafts. The former field, if we mistake not, will be a large one; the latter, we can safely promise him, will be a very small one.

THE POLE COMPASS AS A GUIDE.

From the "Nautical Magazine."

In the great number of British steamers which have been lost during the last two years—one might even say *ten* years without being very far from the truth—twenty-seven per cent. have betrayed the fact most clearly that something was wrong with the courses steered. No *currents* have, in their cases, existed—unfortunately—whereon to fall back; no gales of wind to take the responsibility of their loss; no undiscovered rocks or shoals to help the bewildered out of bewilderments; nothing, in fact, but defective compasses. Well, there no doubt will be defective compasses, as long as compasses exist, just as there are defective machines everywhere; but in respect to the mariner's compass particularly, there are *degrees* of defection, which mean simple errors at one end, easily corrected, but death and destruction at the other.

If compasses are defective, and are really to blame for that large percentage already mentioned, let us see where the causes come in. A few simple inquiries into the method and system of their treatment will lead to the discovery that in the great mass of freight steamers—the great mail packet ships are better looked after in this respect—what is called the standard compass is placed upon the extreme end of a long vertical pole, somewhere in the vicinity of the bridge, and in such position, standing some twelve or fifteen feet above the level of the deck, it is supposed to be beyond all attractive influence. To be in fact beyond the attraction of beams, masts, funnel, &c., &c., of which the ship is composed. When our ships and steamers were in their infancy, when the *Great Britain*, for example, some five and thirty years ago, ran ashore on the coast of Ireland, owing, it was said afterwards to defective compasses, it was generally supposed that a compass, whose various errors had been determined by competent persons, would continue to have such errors, for a voyage or two at least, much in the same way that a fairly good watch, after being regulated for error, may be depended on to keep in that corrected condition for a month or two; or as the watchmakers say, "guaranteed for two years." But the stranding of the *Great Britain*, and other such accidents, opened the eyes of nautical experts to the fact that an error of the very best compass could not be depended on for as many days as some had given it months. A change in the course of the ship, that is to say, in the direction of her head—a heavy list to starboard or port; heavy rolling; or even a storm of thunder, with lightning—was quite enough, separately, to put the compass all wrong. And such changes were frequently sudden.

Finally, after years of trial and experiment, the experts in compass management determined to have the standard compass in such a position that it should be clear of all attraction of ship-fittings, and yet be readily got at for purposes of daily observation. In a large steamer, with spacious decks and bridges, this was not at all a difficult matter, although in smaller ships it was, yet by no means insurmountable; for, if there is room for a bridge, there is invariably also room for an azimuth compass.

Those masters of vessels who ignore the pole compass, who prefer what is called a pure and simple "azimuth compass," placed upon a pedestal on the bridge, or in some part of the ship where it is clear of attractive influence, and where observations upon it can be readily obtained, as a rule, *make* their courses shaped per standard; they make such courses because their officers are able carefully to examine the compass daily, or hourly, by sun, or stellar, or even lunar observations, at any time of day or night, as the case may be.

In the pole compass, fitted as it generally is on board freight steamers, such a system is not carried out; indeed, in very many cases, it might even be said that it is not understood. The ship when new, or on the commencement of a voyage, is swung in or near the docks by a person who makes a living by such work.

He does it correctly enough, no doubt,

and supplies the captain with a series of cards, showing what is the deviation of every point and half point of each compass on board the ship, but especially that of the pole compass. When the ship gets to sea, speaking generally, these deviations, obtained near the docks, or indeed anywhere, it matters not—change, and sometimes to such an extent as to cause much bewilderment. Yet the captain and officers, in numerous instances, are led to suppose that such deviations will hold good throughout the voyage; and in far too many cases they believe this, and act accordingly.

As an example of this system a case may be cited of a new steamer leaving the north-east coast of England for Calcutta. She was swung, on being ready for sea, in order to ascertain the various errors of the compasses. And this service was performed by a fully competent person, no doubt.

First, she was swung in the docks, where a number of iron steamers were moored, but on her captain expressing some doubt as to the value of the errors so obtained, she was taken outside the harbor some three miles from the land, and there put through another series of observations. She then returned to the docks, remained there three days, and afterwards proceeded on her voyage. The three days in the docks had been occupied in taking in a cargo in which iron, or metal of any kind formed no part. This ship made her courses good, until her head was suddenly brought to the northward, after rounding Ceylon, when in the short run from the Basses to Madras she was *forty-five miles* out of her reckoning. On leaving India and proceeding homeward, the courses made were so bad, that she was, on two occasions, within a hair's breadth of being run ashore; first, on the reefs outlying some of the islands in the Red Sea; and secondly in the English Channel. On a subsequent voyage this vessel ran ashore on the coast of Africa, and knocked her bottom out.

It must be assumed that her master and officers were as ignorant of the proper treatment of a compass, or rather of the great errors to which it is liable, as were the badly-advised owners and builders who placed such a contrivance as a pole compass on board their valuable ship. Otherwise she could never have made such extraordinary courses, which were invariably blamed to currents.

It is quite true that in the examination which masters and officers have to pass they are instructed in this branch of navigation, but like many other things, after that inquisition is ended, there is also an end to the exercise; except in mail steamers, when there is certainly more time at hand and a greater number of officers to attend to such duties. There is, however, no attempt made here to excuse officers of such vessels from carrying out such simple duties, and indeed there would be no necessity to excuse them. The fault is not so much theirs as that of the builders, or the owners, in not stipulating for an azimuth compass, on a short pedestal, in place of the long wriggling pole, whose tremors alone, when a ship is steaming, are quite enough to disorganize any compass. It is because the means are not at hand, that ships' officers and masters are negligent of such precautions. Norie, in his excellent Treatise on Navigation, published, probably, more than fifty years ago, gives the simplest and clearest instructions to officers for finding the daily, or hourly error of the compass by celestial observations. Such methods were, however, so little in practice at sea in the wooden ships, when the present masters were apprentices, that along with a great deal more in his valuable epitome, they were well nigh being consigned to oblivion.

In the various circumstances under which officers and masters are now engaged for their appointments, it is perhaps better that the ship should be thoroughly tested for compass error by some competent authority on shore before proceeding to sea; but there are no duties at sea which have a greater call upon the captain than those which demand his daily, or even hourly attention to such friendly guides; and probably there are no duties he would so readily attend to—if the means existed.

Seamen now-a-days are allowed to say so little in regard to the fitting out of ships, that it is not wonderful such a make-believe as a pole continues to exist; it has certainly a rather impressive appearance, which perhaps has something to do with its general favor amongst inexperienced builders and owners

The best place to fit a standard compass is as near the bridge as possible, if the bridge be amidships, for in the center of the ship there is less of that tremulous motion which is common to all steamers, and which has a destructive effect upon a compass's constitution. It should be at least twelve feet from all ironwork, and particularly vertical ironwork such as masts or funnels. If there is not room on or near the bridge, there are many other places, even in the smallest ships, where one could be placed with advantage. It is the fashion in some steamers, which have been built lately to stick to the pole, and to have a platform upon it, for an observer it is presumed, but such standards are not reliable, chiefly because there is too much jolting connected with them. Before concluding this paper it would be interesting to take note of the difference between the successful navigation of a ship fitted with a pole and supplied with cards of deviation by the operator who swung her, and another fitted with an ordinary azimuth compass placed upon a five-foot pedestal. In the first case the cards are examined by the captain, previous to his shaping his courses, and with a belief, firm or otherwise, in their accuracy, he steers for his intended port. Probably a good look-out by a vigilant officer discovers a light or a headland in time to avoid dangers, as was done *frequently* in the case of the steamer already cited, but when fogs and mist surround this unfortunate ship, as happened to her in her following voyage, where is she? Now, turn to one of many examples from those vessels on which daily observations are taken, and where a book is kept in which all observations are duly recorded. We shall find that in upwards of sixty per cent. of such ships in navigating either of the two channels, they make their courses, it may be truly said, to a mile, while nearly all manage it successfully and in most cases at great speed.

GERMAN RAILWAY TESTS FOR IRON AND STEEL.

From "The Engineer."

A CONTROVERSY between the Association of Ironfounders and the Union of German Railway Administrations has brought forward the test question with a certain degree of prominence in the technical press of Germany. One of the most comprehensive articles published on the subject is that which appeared in a recent number of the *Zeitung des Vereins Deutscher Eisenbahn Verwaltungen*, in which Herr Wöhler has reviewed the past history of the controversy, and has endeavored to refute the attacks on his system of classification which were made at the recent Dusseldorf meeting of the iron and steel industry. The action of the railway companies with respect to the question of classification was, it would seem, occasioned by the want of due observance by manufacturers of the needful measures for arriving at excellence in quality, and in their memorial to the Government in 1877, the companies alluded to the opposition which would probably be made by the iron and steel industry to the introduction of the new regulations then under discussion, by reason of the trouble and expense involved in making the trials and researches which would under the circumstances be rendered necessary. Since then the principles of these tests have been criticised in a hostile spirit at various assemblies of the industry affected by them, and ineffectual attempts were made at one time to get the objectionable conditions modified by the Minister of Public Works. At the meeting held at Dusseldorf, on December 10th, 1882, the principles of Herr Wohler's system were again attacked by several manufacturers, and in his brief reply, published in the *Cologne Gazette* shortly afterwards, that gentleman defended his method. It consists in measuring the resistance to fracture, and the tenacity of the metal by the contraction in the cross section after fracture. He alludes in his explanatory remarks to the difficulty of accurately measuring the extension of length at the

point of fracture, while there is no difficulty in arriving at it by means of the easily-measured contraction or diminution of the cross section. In his more detailed remarks in the technical journal already referred to, Herr Wohler alludes to the fact that if an iron or steel bar is bent by external force, it is first subjected to an elastic extension, and if the force is removed it again takes its original shape. If by the application of greater force the limits of elasticity are passed, there is then a permanent extension, the amount of which gives the measure of its tenacity, while its strength is indicated by the amount of force necessary for its fracture. These qualities are independent of each other, but if two similar bars which possess equal tenacity, but different strengths, are subjected to the same exertion, the weaker of the two will, it is considered, extend more than the stronger one, in the same way as with two bars of the same material but of different thickness, the thinner is more extended than the thicker one under an equal burden. The volume of a body is not changed by extension, and therefore a contraction is normally allied with it. Thus the extension in a longitudinal direction of a round bar causes a diminution of its cross section in the same proportion as its length increases. If the extension takes place equally through its whole length, the contraction—or diminution of cross section—is throughout alike, and the measure of extension is simply given by the difference between the original and the subsequent length, in reference to the former. This can also be given correctly by the difference between the original and the subsequent cross section, in reference to the former, and both systems of measurement must give uniform results. This is, however, not the case if the material of the bar is not equal throughout, in which case there will be an inequality in the extension corresponding to the difference which may exist in the strength of the various parts. The weaker portions extend more than the stronger, and therefore Herr Wohler argues that the extension for any particular part of the bar can only be found by measuring the contraction at each place, and not from the difference in length.

From these facts he infers that every iron or steel bar which is extended acquires in the direction of the extension a greater degree of strength. In order to extend it further the burden must be increased, and then the extension is increased until the strength has again been sufficiently augmented to allow the bar to support the increased weight. With an equal increase of the burden the corresponding increase of extension is not the same, but gradually increases, while the cross section diminishes. If the extension has reached a point where the diminution of the cross section surpasses the increase of strength arising from the greater extension, then, provided the tenacity is not exhausted, the extension can, it is true, be continued; yet the burden cannot be increased, but further extends the bar in a more rapid manner until it is broken. Even an unavoidable difference in the strength of a material suffices to produce this effect at one part of the bar somewhat earlier than in other portions. If the same trial is then made on one of the broken pieces, the effect referred to manifests itself in another portion of the bar. These facts are considered as indicating the advantages of estimating the regularity and equality of the material by the relation between the extension of the bar and the contraction. In good tough steel it is said that this proportion is approximately 1 : 2—only an approximate estimate of the tenacity can be deduced from the extension, and this is less likely to be exact according to the greater irregularity of the material.

While thus advocating the principle of taking the contraction in the cross section of fracture as indicating the tenacity of the material, Herr Wohler admits that there may be some exceptional cases in which this method is not applicable, and on which objections have been founded by the opponents of this system. He considers that the method in question displays every fault of the bar which is being tested, and thus facilitates the task of the officials in charge, of the examination of the material submitted. Though the test may present some inconvenience to one or other of the houses interested, it is argued that it is not in any way injurious to the interests of the manufacturers in general. In drawing up the conditions in question the railway companies were influenced by the desire of assuring themselves that the material

they received was of itself fit to be used for the various purposes for which it might be intended, and they based their action in the matter on the principle of keeping strictly in view, in their tests, the conditions and mechanical laws which become operative when the materials are in actual use.

In his comprehensive article, Herr Wohler enumerates the various kinds of injury and wear to which axles, tires, and rails are subject, and remarks that under normal circumstances railway material is not forcibly torn, bent, or broken, and that when such violent force is brought to bear on it, then the limit of human precaution has already been passed. On the other hand, he considers that scientific tests have to be arranged in view of those small and sometimes almost imperceptible movements which, by their frequent repetition, affect the durability of the material subjected to their influence.

The test applied by the imperial railways of Alsace-Lorraine, of which Herr Wohler is manager, in the acceptance of axles, involves the sample bar being subjected to a load of 34.92 tons per square inch of the cross section during ten minutes, without any further extension taking place during that time. If this test is withstood, the bar is subjected to a further weight until it is broken. After being broken, the cross section of fracture must not exceed 65 per cent. of the original cross section. Various facts are quoted from the records of the Alsace-Lorraine Railway Direction with a view of proving that the working of the new regulations has been in no lasting way onerous to the manufacturers interested, inasmuch as there has been since they came into force a gradual diminution in the quantity of rejected material. The following table explains this assertion more fully:

	Accepted.	Rejected.	Percentage Rejected.
Tough steel tires, 1880....	4763	196	$4\frac{1}{7}$
Tough steel tires, 1881....	4461	30	$\frac{2}{3}$
Tough steel axles, 1880 ..	2089	99	$4\frac{3}{4}$
Tough steel axles, 1881 ..	3259	11	$\frac{1}{3}$

In the instances of the rejections made in 1881, there were circumstances indicating the accidental nature of the defects by which they were occasioned. The deliveries of rails seem also to prove that the new regulations have not presented any serious difficulties to manufacturers. In 1879 a contract was made for about 100 miles of rails, which was divided between two establishments. The quality figure arrived at by the sum of the figures of strength and contraction reckoned by the German standards, and subject to certain limits in their respective proportions, was fixed at eighty-five, in accordance with the recommendations of the Salzburg Congress; but it was also stipulated that if the works delivered at least three-fourths of their respective quantities in a superior make with the quality number 90—while still maintaining a strength of at least 38.1 tons per square inch—the price for the proportion of superior rails would be increased by 3 per cent. It resulted that each establishment delivered about four-fifths of its quantity in the better quality and in accordance with the required strength. The remainder of the deliveries were even higher than 90 in quality, being above 100 in one case; but being about 2 tons under the strength fixed, were consequently not reckoned as superior to the standard. It is, however, remarked that the Salzburg Congress had fixed the standard of strength at about 32 tons. It is supposed that this increase of the quality number beyond 85 did not augment the cost of production by 3 per cent., because the works would not have delivered the better quality if there had not been some advantage for them in doing so. In his address on the subject delivered by Herr Wohler before the Verein fur Eisenbahnkunde some twelve months ago, he expressed his opinion that the deliveries made during 1881 manifested a surprising uniformity, proving that homogeneousness is increased in proportion to the improvement in quality. The manufacturers deny, however, that this improvement has been brought about by the new regulations, taking to themselves the credit of having thus raised the standard of their productions by their own independent exertions in that direction.

Herr Wohler, in his most recent com-

munication to the technical press of Germany, disputes at considerable length the assertions made at the Dusseldorf Congress last December by his opponents, and the results which were there described as having been obtained from experiments made by certain manufacturers. Further experiments would seem to be contemplated by them with a view of elucidating the different effects produced by sudden and gradual imposition of burdens for the purpose of testing. He refutes in a categorical manner the arguments deduced from experiments made as to the influence of the reduction of thickness by hammering on the effective properties of steel. He maintains that railway engineers have many difficult problems to solve, for which perfection of material is indispensable, and expresses his surprise that manufacturers should oppose such a requirement if railway companies are willing to pay for it.

The manufacturers' organization has published a letter in the *Cologne Gazette* stating that the cause of the quality of the rails alluded to being above the standard was that the application of the tests is sometimes made in a stringent manner, and under circumstances which treat insignificant defects in such a way that, as a measure of precaution, the quality is made above the standard. The part of Herr Wohler's remarks dealing with the question of the business profit which must have resulted to the manufacturers does not seem, however, to have been dealt with.

ON THE DISCHARGE AND REGULATION OF THE TIBER.

By T. MONTANARI.

Translated from "Il Politecnico" for the Institution of Civil Engineers.

In the foregoing abstract professor Nazzani describes the measurement of the Tiber by means of a current meter. The present article, published at the same time as Professor Nazzina's Paper, but independently, deals with the subject from an entirely different standpoint. The author gives no new measurements, but examines those taken by other inquirers, at different times, on the Tiber, with a view to determining a new Tibrimetrical scale (*i.e.*, a table for giving the discharge of the river when its height on the Ripetta gauge is known). There were three sets of measurements available; 1st, Benetti's, made by means of rods on June 19th, 1821, when the height on the Ripetta gauge was said to be 6.24 * meters; 2d, Canevari's, on May 16th and August 17th, 1871, the heights being 9.661 and 5.80 respectively; 3d, measurements of the maximum surface velocity only, taken by Vescovali on the 13th, 14th, 15th, and 17th of December, 1871, the heights being 12.90, 11.69, 9.08, and 8.08 respectively Upon the first of these measurements a scale was founded by Venturoli; Canevari and Vescovali each founded a scale independently upon his own measurement. The author proposes to examine them all side by side, and obtain a new and accurate scale from them. The importance of the question is shown by the fact that the flood of 1870 was variously estimated by seven different engineers at 2,500, 2,800, 3,058 (Vescovali), 3,128 (Canevari), 3,765, 4,000, and 4,575 cubic meters per second.

The author discusses the method of ascertaining the mean velocity of the river by means of the velocities of the rods, applies corrections to the results obtained by Benetti and Vescovali, and estimates the discharge, from the experiments of the former, at 232.44 cubic meters, instead of 244.06 as given by the observer; and from those of the latter at 160 cubic meters, instead of 174.50, for the discharge at the height of 5.80 meters, and 794.25 instead of 997.50, at the height of 9.665 meters.

Vescovali, having measured only the surface velocity, had to make use of an equation for finding the mean velocity, and of four given by different hydraulicians:

* This is not quite certain; it may have been 6.39, but the author adopts 6.24 as the height.

$\frac{v+0.059}{v+0.15}$ or even 0.924 given by Turrazza,

$\frac{v+2.37187}{v+3.15312}$ . . . " Prony,

0.85 . . . . . " Brunings,

$0.80\times\frac{v+2.37187}{v+3.15312}$. " Baumgarten

he preferred the last. The author points out, however, that Baumgarten's formula was derived from a very dissimilar river (the Garonne), and also from measurements taken with a current meter; whereas those of Vescovali were taken by means of floats, and he shows that, whereas the *mean* velocity of a river should be the same, whatever instruments are used, the maximum velocity will not; current meters giving higher results than rods or floats. He then gives the coefficients to be applied to the formula of Prony, derived from the observations of various experimenters. Baumgarten gives 0.80, as above, with maximum velocities varying from 0.987 meters to 3.12 meters, the mean of seven velocities less than 1.585 giving a coefficient 0.797; the maximum velocity in Prony's experiments being 1.300. Eleven experiments with floats on the Seine at Paris give the ratio of the mean to the maximum velocity from 0.837 to 0.903, the mean velocity being 0.8557; and other experiments with floats on the same river, at Poissy, Triel and Meulan give from 0.836 to 0.892 as the ratio, or a mean of 0.8433. On the other hand, in ten experiments on the Saone, near Verdun, with a meter, giving the maximum velocity (instead of the maximum surface velocity), the ratio varied from 0.803 to 0.721, the mean being 0.758. So that, in the case of the Seine and the Soane, it appears that the ratio between the maximum velocity measured by the current meter and that measured by floats is as 1 : 0.895, and it is probable that a similar ratio would hold in other similar rivers. The author concludes that Baumgarten's coefficient should have been, not 0.80, but $\frac{0.80}{0.895}$ = to nearly 0.895. No general formula can be given which will give the mean velocity of a section when the maximum is known, as this ratio depends upon a variety of causes, the cross section of the river, its depth, inclination, &c. He compares, at some length, the formulas given by different authorities for this ratio, and for that between the mean velocity in any vertical and the maximum in the same. This investigation leads him to adopt Prony's formula with coefficient 0.874, which gives $k=0.74$ when the velocity is 1.933 meter, as was the case when Canevari's measurement was taken at the height of 9.665 on the Ripetta gauge. Applying this coefficient to the velocities and areas of Vescovali's measurements, the discharges are:

	Cubic Meters.
When the height of the Ripetta gauge is 8.08 the discharge is	535.560
When the height of the Ripetta gauge is 9.08 the discharge is	717.195
When the height of the Ripetta gauge is 11.69 the discharge is	1253.664
When the height of the Ripetta gauge is 12 90 the discharge is	1668.073

from which to derive the constants of the formula of discharge,

$$Q=aH_2+bH+c.$$

(H being the height on the gauge), which give as the nearest approximation,

$$Q=23.0291\ H^2-253.3086\ H+1091.5.$$

This equation applies to heights above 8.08, but below that it is found necessary to make use of another; and the author finds that the entire curve of the Tibrimetrical scale is composed of three arcs of reverse curves. The first, beginning at the upper part, is a parabola of the second grade, and refers to heights between 14 and 8.70 meters; it is also probably approximately correct if continued above the height of 14 meters. Below 8.70 is a curve of the third grade, which descends to 5.80, below which, again, is a short branch of a parabola of the second grade, with axis not vertical, which descends to 3.30 (the point corresponding to the bottom of the river). This curve gives, in the author's opinion, the maximum limits of discharge when the river is low, the minimum limits when it is high. He also determines another curve giving minimum limits when the water is low, maximum when it is high. These two curves are shown upon a diagram, which also gives the curves of discharge obtained by Vescovali, Canevari, and Venturoli, and also a correction of Vescovali's curve by the author, and one of Venturoli's by Possenti. The paper

goes on at great length into the discussion and justification of the author's methods of calculation.

The next point considered is the relation between rainfall and the discharge of the river. A curve is constructed which shows the duration of each different amount of discharge throughout the year, the abscissas representing hours, the ordinates the height of the river; then, by multiplying each ordinate by the discharge of the river at the corresponding height (obtained from the curve of discharge previously constructed), the mean discharge which the river sends down annually for each level is found, and a curve representing these amounts is drawn, the area of which represents the total annual discharge, which the author finds to be 9,954 millions of cubic meters, and this gives as the mean discharge per second, or *module*, 315 cubic meters. This amount distributed over the 16,132 square kilometers of the river basin above Rome is equivalent to a height of 0.617 meters of water; while the rainfall, as taken at many points in the basin since 1870, is 1 meter, or from 1822 to 1871, 0.92235 meter. Hence the mean discharge of the river is two-thirds of the rainfall.

A remarkable feature of the Tiber is the constancy of the flow, which is greater than that of any other river not fed by extensive lakes or perpetual snow, and hardly inferior to some of these latter. The cause is that the river is supplied from extensive areas of permeable oolitic and cretaceous strata, which, overlying impermeable strata, form vast subterranean reservoirs. The question of the extent to which the river is supplied from these subterranean sources is gone into at some length, and the author then proceeds to discuss the ratio $\frac{D}{P}$, in which D is the mean annual discharge of the river, and P the quantity which would have been discharged if the whole of the rainfall had passed direct into the river; that is to say, $\frac{D}{P}$ is the ratio of the discharge to the rainfall. This ratio varies greatly in different years. A table is given, showing its value as calculated by different observers, from 1822 to 1861. This table the author considers incorrect owing to various causes, and he gives another worked out by himself, and using his own Tibrimetrical scale to obtain the values of D. According to this table $\frac{D}{P}$ varies from 0.876 to 0.422. In comparison with these results he gives similar calculations made in the basin of the Garonne, which show that in the case of that river the value $\frac{D}{P}$ varied during the years 1838 to 1846 from 0.829 to 0.515. From further tables he deduces the following laws: 1st, that the ratio $\frac{D}{P}$ does not vary sensibly with the rainfall; 2d, that it increases with the discharge; 3d, that these two laws seem to apply equally to the Tiber and the Garonne. The fact that evaporation is pretty constant, whatever the rainfall may be, has an important influence on this ratio. The author considers that, for the purpose of these calculations, the year should be taken as commencing on the 1st of September, so that the discharge should, as far as possible, correspond with the rainfall of the year, which is not the case reckoning from the 1st of January, as the rain and snow of the end of one year form a material part of the discharge of the next.

In an appendix the author refers to Nazzani's measurements, which give the discharge considerably smaller than his calculations. This he attributes partly to the current meter (and particularly the kind of meter used by Nazzani) being unsuitable for measuring velocities in so turbid a river as the Tiber, partly to the effect of wind not having been allowed for, and partly to other causes. He seems to admit, however, that the problem of constructing a Tibrimetrical scale is not yet solved, but must wait for further experiments. His calculations of the ratio of discharge to rainfall, however, would not be materially modified should Nazzani's measurements prove correct.

Blasting paper is made by J. Petry, Vienna, consisting of unsized paper coated with a hot mixture of 17 parts yellow prussiate of potash, 17 of charcoal, 35 refined saltpetre, 70 of potassium chlorate, 10 of wheat starch, and 1500 of water. After drying it is cut into strips, which are rolled into cartridges.

THE REFORESTING OF MOUNTAINS.

By P. DEMONTZEY.

From "La Nature," for Abstracts of the Institution of Civil Engineers.

The state of sterility of the mountain slopes of the Alps, the Pyrenees, and the Cevennes, is traced to the removal of the forests which used to cover these mountains. The unrestrained torrents have gradually stripped the soil from the slopes, which, in the absence of trees, is deprived of its source of renewal, so that each year the area of vegetation becomes more restricted, and the rapid descent of the floods spreads devastation in the fertile valleys below. The first tentative measures for remedying this disastrous condition were undertaken in 1860, by encouraging the growth of forests in various mountain districts in the south of France, and the experiments have resulted in converting desolate regions into tracts covered with young forests, and in changing formidable torrents into harmless streams. In order to promote the growth of trees, it is necessary both to cover the denuded rocky slopes with a covering of softer ground, into which the roots can penetrate, and also to protect the young vegetation from being washed away by the torrent. These objects are accomplished by erecting numerous dams at suitable spots across the bed of the torrent, some constructed of masonry, designed to be permanent, and others merely temporary constructions formed with hurdles and fascines, and by protecting and regulating the sides and bed of the torrent by lines of hurdles and brushwood. The dams and cross lines of hurdles reduce the slope and increase the width of the bed of the torrent, and consequently diminish the velocity of flow, and cause an accumulation of detritus; whilst the hurdles along the banks protect the side slopes from scour, and the accretions diminish their declivity. The growth of forests is thereby rendered feasible, which in their turn protect the slopes from denudation, check the descent of the small streams into the torrent, and, by the fall of their leaves, furnish a supply of soil for promoting vegetation. The kind of trees that should be planted depends upon the site, the exposure, and the altitude; but whereas, under the recent unfavorable conditions, the limit of altitude of the growth of forest trees was placed at about 6,500 feet, pine firs and larches, of from ten to fifteen years' growth, may now be seen covering large tracts of ground at an altitude of about 9,000 feet, where the old forests do not extend beyond the original limit assigned of 6,500 feet. The result of the above kind of work, carried out in the valley of St. Bernard, near Barcelonnette, has been to retain all the detritus in the valley, so that nothing but pure water passes away. The slopes are, in consequence, becoming flatter, and it only remains to facilitate the growth of trees higher up the banks by promoting the extension of the covering of soil, which is rapidly effected by layers of willows along the banks, and even occasionally across the bed of the stream, producing a network of roots and layers, increasing with the growth of the plants. In the case of some strata traversed by numerous ravines, the accumulation of detritus would be too slow to yield satisfactory results, and it becomes necessary, after constructing a dam at the base of the ravine, to remove all the projecting portions of the slopes, and throw them into the bed of the stream, so as to raise the bed at least 3 to 6 feet. A series of lines of fascines are then placed across the new bed to maintain it, and the upper layer of rocky debris is converted, by exposure to the atmosphere, into soil suitable for some kinds of vegetation. A law was passed in France, in 1882, for replanting forests and preserving them on the mountains, and the State has undertaken the execution of the work in the most important districts. It is anticipated that the results of these works will be, security of the population and land from inundation, by the regulation of the torrents; prevention of detritus from filling up the beds of the rivers below; extension of land suitable for cultivation; increase in the discharge of rivers, and consequent possibility of extending irrigation; and a large development in the supply of wood.

THE WINDING OF ELECTRO-MAGNETS.

From "English Mechanic and World of Science."

IN the current number of the *Philosophical Magazine,* Profs. Ayrton and Perry have a paper in which they describe the results of some experiments to determine which mode of winding a given length of wire on an iron bar gave the strongest electro-magnet for the same current. Four bars of iron, each 12 in. long, were cut from the same rod ⅝ in. thick, and an exactly equal length of wire was wound on the four bars respectively, in the following way: 1. Wire wound equally over the whole length. 2. Wire coned towards each end. 3. Wire wound equally over half the iron bar, leaving the other end bare. 4. Wire wound on one half but coned towards the end. Electro-magnet No. 1 was put on so that its axis was at right angles to the axis of a small magnetic needle and passed through the point of suspension of the needle, which was suspended so as to move freely in a horizontal plane, and far enough away that the magnetic field due to the electro-magnet No. 1, when magnetized by passing a current through it, was nearly constant over that portion of the field in which the little suspended needle moved when deflected. A constant current was now passed through the coil on No. 1, and the deflection of the little needle observed when No. 1 was placed at different distances from the center of the test needle, the axis of No. 1, however, always remaining in the same line. Under these circumstances it is well known that the strength of the field produced by No. 1 at the center of the test-needle is approximately proportional to the tangent of its deflection. Experiments were now made in a similar way with electro-magnet No. 2, and with each end of No. 3 and of No. 4 the same current as was employed with electro-magnet No. 1 being used in all cases, and which was much below the saturating current. It was found that the effect of coning the wire was to produce a strong field very near the pole, but that the force fell off very rapidly as the distance from the pole increased. The uniformly-coiled magnet No. 1 was found to produce the most powerful field at considerable distances from the end of the electro-magnet, while for points nearer the magnet, but still at a distance of about 3 in. from it, the covered end of No. 3 magnet produced the strongest field, the next strongest being produced by magnet No. 2 with the wire coned towards each end. To ascertain the force which each magnet would exert on an armature, experiments were made, and the following results obtained, the current flowing through the coil in each case being exactly the same, as well as the armature employed:

Magnet.	Weight required to detach the armature from the covered end of the magnet.
No. 1	45 ounces.
" 2	57 "
" 3	57 "
" 4	77 "

These results confirmed the previously-ascertained fact that the field produced by the covered ends of the electro-magnets numbers 2 or 3 at distances near the pole was much stronger than that produced by No. 1. They also showed that for very small distances the covered end of No. 4 produced the strongest field. From these experiments, Messrs. Ayrton and Perry conclude that with a definite iron core, a definite length of wire to be coiled on it, and to be traversed by a definite current, the mode of coiling to produce the largest field depends entirely on the distance from the end of the electro-magnet at which the field is to be produced. With the particular magnet employed by them it is seen that, at distances from the end of the magnet—very small compared with the length of the core, the wire should be coiled up at the near end of the core; to create a field at a distance from the end of the magnet equal to about a third of the length of the magnet, it is better to coil the wire uniformly over one-half of the core than to cone it up at the near end; while for distances from the end of the magnet, equal to, or greater than, about ⅘ of the length of the core, the uniform mode of winding is the best.

THE ELECTRO-MOTIVE FORCE AND RESISTANCE OF "BUNSEN" CELLS.

By GEO. G. GROWER.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

It is frequently required to know the current which can be obtained from "Bunsen" cells, on circuits of various resistances. This can easily be calculated from Ohm's law, when the electromotive force and resistance are known. Of course the external resistance must also be known.

The electromotive force of cells, as given in various books, seems very irregular; different figures being given by different authorities. This may be partly due to the fact that the electromotive force is constantly changing while the cell is working, being highest at first, and falling more or less rapidly according to the work the cell is doing.

The density of the acids or solutions used also affects the electromotive force, and as experimenters use different densities, this may also account for part of the irregularities mentioned.

The resistance of the cells keeps changing also, and very few books give any figures in regard to it. The electromotive force of cells is usually given in volts, and although the volt has an absolute value theoretically, it is practically a very uncertain unit.

The "Daniel" cell, charged with a solution of zinc sulphate, is frequently used as a standard of electromotive force; it is taken as 1.079 volt by many experimenters, but some regard it as .955 volt, and still others as one volt. This shows how uncertain the volt is as a practical unit of electromotive force.

The "Bunsen" cell is charged in a number of different ways, each of which has its advantages and disadvantages. The two commonest ways of charging is to fill the porous cup with nitric acid, or a solution of bichromate of potash in sulphuric acid. The zinc is put in dilute sulphuric acid.

To compare the merits of the two ways, one cell was set up charged each way, and they were left to run down on a resistance of about 1 ohm. The electromotive force, resistance and current were taken at intervals. The method of obtaining the electromotive force, etc., was as follows:

The cells were left short-circuited on a resistance of 1.05 ohms, except when deflections were taken. A key was inserted in line, which kept the circuit closed, except when depressed, when it threw the cell direct on the galvanometer. By means of a switch the galvanometer could be connected to the terminals of 1 ohm in the line.

The galvanometer used was a high resistance reflector shunted so that a suitable portion of the current would go through it. A high resistance was also inserted in the galvanometer line.

The steady deflections are proportional to the current, but as it takes time to bring the spot to rest, the battery may have changed considerably when the deflection is obtained. The electromotive force of the cells begins to increase the moment the current is stopped.

The various swings or impulses are also proportional to the current, so to make the observations rapidly, before changes can occur in the cell, the first swings were taken.

For a standard cell I used a "Daniel," containing a saturated solution of copper-sulphate and a solution of zinc sulphate, prepared by dissolving 1 part of zinc sulphate in 10 of water. Pure chemicals were used in this cell. The zinc was not amalgamated. The galvanometer was set so that the standard cell gave a first swing of 100, therefore each deflection meant .01 of the standard cell.

It will be seen from the above that the deflection of the cell equals the electromotive force, that off the ohm equals the current. The electromotive force divided by the current equals the total resistance; subtracting from this the external resistance 1.05, we have left the resistance of the cell in ohms.

The dimensions of the cells were:

glass jar, 10 cm. diameter, 12.5 cm. high; porous cups, 5 cm. diameter, and zincs, 6.5 cm. diameter. The carbons were 2x1.5 cm.

Each jar contained 400 cu. cm. of dilute sulphuric acid (1 to 12 by volume). No. 1 contained 100 cu. cm. of nitric acid (sp. gr., 1.35) in the porous cup; No. 2, 100 cu. cm. of chromic solution, prepared by dissolving 30 g. of bichromate of potash in 240 cu. cm. of water, and then adding 50 cu. cm. of sulphuric acid. The zincs were amalgamated the day before using; they were also weighed, so that the amount of local action could be ascertained. The cells were left charged $1\frac{3}{4}$ hours before using. External resistance was 1.05 ohm.

The electromotive force is given in terms of the "Daniel" cell; the resistance in ohms. The current answers the formula $\frac{E}{R}$.

After Working.	No.1, Nitric Acid. E. M. F.	Current.	Resist.	No. 2, Chromic Solution. E. M. F.	Current.	Resist.
	1.71	1.21	.35	1.83	1.22	.45
5 minutes.	1.70	1.21	.34	1.40	.90	.50
10 " .	1.69	1.21	.33	1.32	.86	.48
15 " .	1.69	1.20	.34	1.29	.86	.45
30 " .	1 69	1.21	.33	1.24	.82	.46
45 " .	1.68	1.21	.33	1.20	.79	.47
1 hour...	1.69	1.23	.32	1.17	.79	.43
$1\frac{1}{2}$ " ...	1.68	1.24	.29	1.13	.76	.44
2 " ...	1.69	1.25	.29	1.10	.74	.43
3 " ...	1.69	1.26	.28	.99	.67	.42
4 " ...	1 68	1.26	.27	.90	.60	.45
5 " ...	1.65	1.23	.28	.76	.49	.50
6 " ...	1.64	1.22	.28	.61	.37	.60
7 " ...	1.64	1.21	.29	.48	.29	.60
8 " ...	1.64	1.21	.29	.43	.25	.67
9 " ...	1.63	1.20	.29	.41	.23	.73
11 " ...	1.60	1.17	.30	.35	.20	.70
22 " ...	1.45	.91	.53	.31	.17	.77
23 " ...	1.36	.83	.46	.31	.17	.77
24 " ...	1.17	.73	.54	.31	.17	.77
25 " ...	1.02	.63	.56	.31	.17	.77
26 " ...	.90	.55	.57	.31	.17	.77
29 " ...	.43	.25	.66	.31	.17	.77
30 " ...	.42	.24	.69	.31	.17	.77
31 " ...	.41	.23	.72	.31	.17	.77

The current here taken will dissolve in 1 hour 1.32 grains of zinc.

Zinc dissolved in cell No. 1 = 48.241 g.
" " by current = 37.026 g.
" " by local action = 11.215 g.

Zinc wasted is about 30 per cent. as much as utilized.

Zinc dissolved in cell No. 2 = 13.226 g.
" " by current = 11.128 g.
" " by local action = 2.098 g.

Zinc wasted is about 19 per cent. as much as utilized.

The above figures show that although the electromotive force of the cell with the chromic solution is at first higher than with nitric acid, it immediately falls far below it.

The nitric acid cell holds its electromotive force well until nearly exhausted. With nitric acid the resistance is less and the current stronger and steadier than with chromic solution.

The waste of zinc due to local action appears to be more with nitric acid than with chromic solution.

The nitric acid cell is far superior to the chromic as far as the work is concerned, and if it were not for the disagreeable and unhealthy fumes it gives off, would be used in nearly every case.

CENTRIFUGAL FORCE.—Professors Ayrton and Perry exhibited at the last meeting of the Physical Society an ingenious lecture apparatus for demonstrating the laws of centrifugal force. As was properly pointed out by Professor Ayrton, the ordinary lecture apparatus of this kind do not really demonstrate the laws of the subject, but simply show the effect; and a new and more scientific class of apparatus is demanded by the extension of scientific teaching. Professor Perry and he have been engaged in designing new apparatus to meet the wants of their City Guilds students, and the apparatus shown was one of the instruments in question. It consists of a rotating vertical axis carrying an aneroid chamber filled with mercury, which also rises in a graduated capillary tube projecting from its middle. A metal arm projects at right angles from the aneroid or diaphragm side of this chamber, and carries a sliding weight which can be shifted to different distances on the graduated arm. On rotating the axis the centrifugal force of the projecting arm pulls on the elastic diaphragm of the mercury chamber, and the mercury within it having more room sinks in the capillary tube by a corresponding number of degrees. The apparatus is capable of demonstrating the law of centrifugal force with accuracy, according to experiments which have been made; and as Professor Guthrie remarked it could be used for indicating the speed of wheels and shafts. We may add that there is already a mercury counter in existence, in which a closed mercury chamber is rotated, and the parabolic concavity given to the mercury by the centrifugal force is employed to measure the speed.—*Engineering.*

ON TELPHERAGE.*

By Prof. FLEEMING JENKIN, LL.D., F.R.S.

From "The Electrician."

"The transmission of vehicles by electricity to a distance, independently of any control exercised from the vehicle, I will call Telpherage." These words are quoted from my first patent relating to this subject. The word should, by the ordinary rules of derivation, be telephorage, but as this word sounds badly to my ear, I ventured to adopt such a modified form as constant usage in England for a few centuries might have produced, and I was the more ready to trust to my ear in the matter, because the word telpher relieves us from the confusion which might arise between telephore and telephone when written.

I have been encouraged to choose telpherage as the subject of my address by the fact that a public exhibition of a telpher line, with trains running on it, will be made this afternoon for the first time.

You are, of course, all aware that electrical railways have been run, and are running with success in several places. Their introduction has been chiefly due to the energy and invention of Messrs. Siemens. I do not doubt of their success and great extension in the future—but when considering the earliest examples of these railways in the spring of last year, it occurred to me that in simply adapting electric motors to the old form of railway and rolling stock, inventors had not gone far enough back. George Stephenson said that the railway and locomotive were two parts of one machine, and the inference seemed to follow that when electric motors were to be employed a new form of road and a new type of train would be desirable.

When using steam we can produce the power most economical in large engines, and we can control the power most effectually and most cheaply when so produced. A separate steam engine to each carriage, with its own stoker and driver, could not compete with the large locomotive and heavy train; but these imply a strong and costly road and permanent way. No mechanical method of distributing power, so as to pull trains along at a distance from a stationary engine, has been successful on our railways; but now that electricity has given us new and unrivalled means for the distribution of power, the problem requires reconsideration.

With the help of an electric current as the transmitter of power, we can draw off, as it were, one, two, or three horse-power from a hundred different points of a conductor many miles long, with as much ease as we can obtain 100 or 200 horse-power at any one point. We can cut off the power from any single motor by the mere break of contact between two pieces of metal; we can restore the power by merely letting the two pieces of metal touch; we can make these changes by electro-magnets with the rapidity of thought, and we can deal as we please with each of one hundred motors without sensibly affecting the others. These considerations led me to conclude, in the first place, that when using electricity we might with advantage subdivide the weight to be carried, distributing the load among many light vehicles following each other in an almost continuous stream, instead of concentrating the load in heavy trains widely spaced, as in our actual railways. The change in the distribution of the load would allow us to adopt a cheap, light form of road. The wide distribution of weight entails many small trains in substitution for a single heavy train; these small trains could not be economically run if a separate driver were required for each. But, as I have already pointed out, electricity not only facilitates the distribution of power, but gives a ready means of controlling that power. Our light, continuous stream of trains can, therefore, be worked automatically, or managed independently of any guard or driver accompanying the train—in other words, I could arrange a self-acting block, preventing collisions. Next came the ques-

* Introductory address delivered to the Class of Engineering, University of Edinburgh.

tion, What would be the best form of substructure for the new mode of conveyance? Suspended rods or ropes, at a considerable height, appeared to me to have great advantages over any road on the level of the ground; the suspended rods also seemed superior to any stiff form of rail or girder supported at a height. The insulation of ropes with few supports would be easy; they could cross the country with no bridges or earthworks; they could remove the electrical conductor to a safe distance from men and cattle; cheap small rods employed as so many light suspension bridges would support in the aggregate a large weight. Moreover, I considered that a single rod or rail would present great advantages over any double rail system, provided any suitable means could be devised for driving a train along a single track. [Up to that time two conductors had invariably been used.] It also seemed desirable that the metal rod bearing the train should also convey the current driving it. Lines such as I contemplated would not impede cultivation nor interfere with fencing. Ground need not be purchased for their erection. Mere way-leaves would be sufficient, as in the case of telegraphs. My ideas had reached this point in the spring of 1882, and I had devised some means for carrying them into effect when I read the account of the electrical railway exhibited by Professors Ayrton and Perry. In connection with this railway they had contrived means rendering the control of the vehicles independent of the action of the guard or driver; and this absolute block, as they called their system, seemed to me all that was required to enable me at once to carry out my idea of a continuous stream of light, evenly-spaced trains, with no drivers or guards. I saw, moreover, that the development of the system I had in view would be a severe tax on my time and energy; also that in Edinburgh I was not well placed for pushing such a scheme, and I had formed a high opinion of the value of the assistance which Professors Ayrton and Perry could give in designs and inventions.

Moved by these considerations, I wrote asking Professor Ayrton to co-operate in the development of my scheme, and suggesting that he should join with me in taking out my first Telpher patent. It has been found more convenient to keep our several patents distinct, but my letter ultimately led to the formation of the Telpherage Company (Limited), in which Professor Ayrton, Professor Perry, and I have equal interests. This company owns all our inventions in respect of electric locomotion, and the line shown in action to-day has been erected by this company on the estate of the chairman—Mr. Marlborough R. Pryor, of Weston. Since the summer of last year, and more especially since the formation of the company this spring, much time and thought has been spent in elaborating details. We are still far from the end of our work, and it is highly probable what has been done will change rapidly by a natural process of evolution. Nevertheless, the actual line now working does in all its main features accurately reproduce my first conception, and the general principles I have just laid down will, I think, remain true, however great the change in details may be.

The line at Weston consists of a series of posts, 60ft. apart, with two lines of rods or ropes, supported by crossheads on the posts. Each of these lines carries a train; one in fact is the up line, and the other the down line. Square steel rods, round steel rods, and steel wire ropes are all in course of trial. The round steel rod is my favorite road at present. The line is divided into sections of 120ft. or two spans, and each section is insulated from its neighbor. The rod or rope is at the posts supported by cast-iron saddles, curved in a vertical plane, so as to facilitate the passage of the wheels over the point of support. Each alternate section is insulated from the ground; all the insulated sections are in electrical connection with one another—so are all the uninsulated sections. The train is 120ft. long—the same length as that of a section. It consists of a series of seven buckets and a locomotive, evenly spaced with ash distance pieces—each bucket will convey, as a useful load, about $2\frac{1}{2}$ cwts., and the bucket or skep, as it has come to be called, weighs, with its load, about 3 cwts. The locomotive also weighs about 3 cwts. The skeps hang below the line from one or from two V wheels, supported by arms which project out sideways so as to clear the supports at the posts; the motor or dynamo on

the locomotive is also below the line. It is supported on two broad flat wheels, and is driven by two horizontal gripping wheels; the connection of these with the motor is made by a new kind of frictional gear which I have called nest gear, but which I cannot describe to-day. The motor on the locomotive will give as a maximum 1½ horse-power when so much is needed. A wire connects one pole of the motor with the leading wheel of the train, and a second wire connects the other pole with the trailing wheel; the other wheels are insulated from each other. Thus the train, wherever it stands, bridges a gap separating the insulated from the uninsulated section. The insulated sections are supplied with electricity from a dynamo driven by a stationary engine, and the current passing from the insulated section to the uninsulated section through the motor drives the locomotive. The actual line is quite short, and can only show two trains, one on the up and one on the down line; but with sufficient power at the station any number of trains could be driven in a continuous stream on each line. The appearance is that of a line of buckets running along a single telegraph wire of large size. A block system is devised and partly made, but is not yet erected. It differs from the earlier proposals in having no working parts on the line. This system of propulsion is called by us the Cross Over Parallel Arc. Other systems of supplying the currents, devised both by Professors Ayrton and Perry and myself, will be tried on lines now being erected; but that just described gives good results. The motors employed in the locomotives were invented by Messrs. Ayrton and Perry. They are believed to have the special advantage of giving a larger power for a given weight than any others. One weighing 99lb. gave 1½ h.-p. in some tests lately made. One weighing 36lb. gave 0.41 h.-p.

No scientific experiments have yet been made on the working of the line, and matters are not yet ripe for this—but we know that we can erect a cheap and simple permanent way, which will convey a useful load of say 15 cwts. on every alternate span of 120ft. This corresponds to 16½ tons per mile, which, running at five miles per hour, would convey 92½ tons of goods per hour. Thus, if we work for twenty hours, the line will convey 1850 tons of goods each way per diem, which seems a very fair performance for an inch rope. The arrangement of the line with only one rod instead of two rails diminishes friction very greatly. The carriages run as light as bicycles. The same peculiarity allows very sharp curves to be taken, but I am without experimental tests as yet of the limit in this respect. Further, we now know that we can insulate the line satisfactorily, even if very high potentials came to be employed. The grip of the locomotive is admirable and almost frictionless, the gear is silent and runs very easily. It is suited for the highest speeds, and this is very necessary, as the motors may, with advantage, run at 2,000 revolutions per minute.

The suspended rods are not suitable for high speeds. Centrifugal force would use great strains on them, and the vehicles would be shot up into the air at the points of support. Very high speeds might be attained for light trains with a stiff road, but we are for the present less interested in this application of our ideas. A smaller type of line with ⅝ rods and smaller spans is in course of construction.

This will probably soon be extended for a mile or so, now that we have gained some experience on the few spans of this heavier line.

At present we do not contemplate working lines extending more than five miles from a station, so that in a long continuous line we should require stations at intervals of ten miles. A single station could work, either simultaneously or in succession, a large number of lines radiating from it in many directions.

I cannot yet enter into questions of cost, and the company is hardly ready to undertake contracts, except perhaps for very simple cases. We have still to elaborate designs for sidings; for loading and marshalling the trains; and we have still to test the arrangements for governing and blocking. We have also to compare different systems of electric propulsion and blocking, and improve the design of many details in construction. All this will take time, but time and thought are all that is required. No new discovery is wanted; no unforeseen difficulty has been met with.

I am almost afraid to speak of the probable uses to which telpherage may be put. If I said all I thought, I should be told I was describing an electrical Utopia. The first and most obvious use of a telpher line is that to which existing wire tramways are already put—namely, the conveyance of minerals or ore from mines to canals, railways, or the sea. The suspended wire rope is specially suited for rocky uneven ground, and very heavy gradients could be worked. The telpher line has the following advantages over the present system: It can go round sharp curves, change the gradient as often as is desired, and be made of any length; any train can be stopped and shunted without stopping the others. If made with no working parts, as in the present example, the permanent way may be left idle for part of the year with no sensible deterioration.

Mineral traffic of this kind is, however, in my opinion, only one small part of the work which these lines can do. Where railways and canals do not exist, telpher lines will provide the cheapest mode of inland conveyance for all goods—such as corn, coal, root crops, herrings, salt, bricks, hides, and so forth—which can be conveniently subdivided into parcels of one, two or three hundredweight. In new colonies the lines will often be cheaper to make than roads, and will convey goods far more cheaply. In war they will give a ready means of sending supplies to the front. Moreover, wherever a telpher line exists, power is thereby laid on, and this power may be used for other purposes than locomotion—a flexible wire attached to the line will serve to drive a one, two, or three horse-engine, which may be used for any imaginable purpose, such as digging, mowing, threshing, or sawing. It is true that in the transmission of the power more than half may be wasted; but the proportion wasted is diminishing yearly, monthly, almost daily, with the growth of our electrical knowledge; and when we remember that by stationary engines in London power can be produced at the rate of about one halfpenny per hour for each effective horse, we shall not be alarmed at the prospect of doubling this cost when the power is delivered on a rough hillside, especially when we remember that the engine receiving that horse-power need weigh no more than one hundred pounds. Surely I am not too sanguine in expecting that great changes will be produced in agriculture by these new facilities for transport, coupled with the delivery of power at will from any point of the telpher road. It must not be supposed that I look on the new telpher lines as likely to compete with railways or injure their traffic. On the contrary, my feeling is that they will act as feeders of great value to the railways, extending into districts which could not support the cost even of the lightest railway. It is idle to endeavor to foretell the future of any new idea, but this much is certain—a novel mode of transport offering some exceptional advantages will be publicly shown on a practical scale to-day.

REGENERATIVE FURNACES AT THE MUNICH GASWORKS.

By Dr. SCHILLING and Dr. H. BUNTE.

Translated from "Journal fur Gasbeleuchtung" for Institution of Civil Engineers.

These furnaces have been described from time to time in their different phases of development, and the present description of them is consequent upon the adoption of a definite and complete form which is the result of scientific examination and practical experience during several years.

The peculiarity of these furnaces lies in the manner of working the generator, and in the systematic utilization of the waste heat of the oven in the regenerator. The generator is worked on the so-called "damp" system, i.e., the gasification of the coke is carried on by a mixture of air and steam. The advantages of this method have been made known by the experiments of Dr. Bunte on the capabilities of coke-generators, which were carried out at the Munich gas works, at the instigation of the German Association of Gas- and Water Engineers. These

advantages consist chiefly in the fact that the formation of clinker in the generator is prevented, whatever the kind of coke used; the earthy constituents remaining in the form of loose ashes in the grate, through which the air can easily pass. The experience of several years has shown that under this system the most various kinds of coke can be equally well used. When the generators have been fired first with coke from Saar coal, then with that from Bohemian coal, or with both together, mixed with that from Bohemian Plattel coal, there has not heen the least trouble in working them. It is just these kinds of coke that differ so much in their characteristics; for while the earthy constituents of Saar coal are easily melted, those of Bohemian coal are remarkably refractory. It is easy, simply by using a greater or less quantity of steam, to obtain the desired constitution of the ashes, and so prevent the formation of clinker. The porousness of the ashes, permitting the passage of air and steam, admits of an unusually uniform production of gas in the generator, and the furnace can be worked with only a slight draught. A stratum of ash several decimeters thick may be permitted to accumulate upon the grate without disturbing the regular working of the furnace, and the removal of the ashes need not be performed oftener than from every twenty-four to thirty-six hours. In order to obtain a uniform production of steam without loss of heat, the waste-gases, after leaving the regenerator, are made, before reaching the flue, to pass under and heat a water-tank situated below the fire-grate. The air and steam are mixed before entering the generator, and superheated by passing through channels adjacent to the flues conducting the waste-gases away. The tank containing the water for evaporation is covered, so that the steam cannot ascend direct through the fire-grate, but must pass out at the back of the tank, where it mixes with the air-supply, and the mixed air and steam, after passing through the canals above referred to, return to the fire grate, and enter the furnace above the covered tank. The generator-gases are introduced into the oven, a retort-setting, between the bottom retorts, where they mix with the heated air-supply for the secondary combustion. After heating the retorts, the products of combustion pass through flues in the regenerator situated below the oven. The air traverses channels situated alternately between the flues, the air always passing in a contrary direction to that of the waste-gases. Afterwards the gases heat the air and steam for the generator; and, lastly, they heat the water in the tank for the steam production.

The heating-surface of the tank is so designed that about 1.0 to 1.3 kilogrammes of steam will be produced per hour, which quantity is amply sufficient, under all circumstances, to prevent the formation of clinker. To regulate the quantity of steam, if required, a valve can be opened, which permits cold air to mix with the waste gases, and so reduce the temperature to the required degree. When it is required to remove the ashes from the generator, bars of flat iron are inserted through the sides of the generator, at a certain height above the grate, which support the superincumbent mass of fuel. The ashes are then withdrawn from a sealed door in the front. This operation is performed in ten minutes, and need not be repeated for thirty-six or even forty-eight hours. To replace the water evaporated in the tank, a funnel and siphon-tube are affixed to the side of the tank, into which a small stream of water continually flows, the surplus running off by an overflow. In starting a generator the waste gases are not allowed to pass through the regenerator at first, but are conducted direct to the chimney-flue, the change being made by dampers or valves after the oven has become thoroughly heated.

The working results taken from a bench of six ovens of this description are given on page 513.

The regenerators do not suffer deterioration, and require no repairs for years, as a four-years' experience with the older type of oven has proved; also the generator-shafts remain perfectly intact.

The following is an average of many analyses of the generator gases:

Carbonic acid	8.6	per cent.
" oxide	20.6	"
Hydrogen	15.0	"
Nitrogen	55.8	"
	100.0	

Gas produced per oven in	24 hours	= 2,300 c. m.	= 71,650 cubic ft. per ton.	
Coals carbonized in retorts in	24 "	= 7,350 kilos.	= 7 tons 3 cwt.	
Coke used for furnace in	24 "	= 800 "	= $15\frac{3}{4}$ cwt.	
Gas produced per retort in	24 "	= 287 c. m.	= 10,135 cubic ft. per ton.	
" " " 100 kilos. of coal		= 31 "	= 10,234 "	
Coal carbonized per retort per diem		= 919 kilos.	= $18\frac{1}{4}$ cwt.	
" charged per retort every 4 hours		= 153 "	= 334 lbs.	
Coke used per 100 kilos. of coal		= 10.9 "	= 10.9 per cent.	

The coke contained 14 per cent. of ash.

The generator possesses a temperature of 1,150° Centigrade on entering the oven, and the products of combustion leave it at 1,400°. After passing through the regenerator, and heating the secondary air-supply, they are reduced to 900°, and the air is heated to 1,000° or 1,100°. The waste-gases heat the air and steam for the generator to 350°, and they leave the water-tank and pass into the chimney-fire at a temperature of 550° Centigrade.

If the products passed direct from the oven to the chimney at a temperature of 1,400° the loss of heat would equal 64 per cent. of the total heating value of the coke; but by regeneration and the utilization of the heat to produce steam for the generator, this loss is reduced to 25 per cent. Of the total quantity of heat (= 39 per cent.) withdrawn from the waste-gases, 20 per cent. is communicated to the secondary air-supply, 6 per cent. to the primary air-supply, and 5 per cent. goes to the production of steam, while 8 per cent. is lost in radiation and conduction from the surface of the brick-work.

RELATIONS OF STATIC AND DYNAMIC ELECTRICITY.

By JOHN T. SPRAGUE.

From the "English Mechanic."*

Some time since I promised to explain a subject which has puzzled a good many, and as to which questions have often been put—viz., the often-quoted statement of Faraday as to the electricity contained in water and in a flash of lightning. That is the text of the present article.

Faraday said that the electricity contained in a grain of water is equal to that in a powerful flash of lightning: some quote him as saying it is equal to that of a severe thunderstorm. Now, we know from chemistry that the potential energy of a grain of water—that due to the combination of hydrogen and oxygen, which combination is broken up by electricity—is ft.-lb. $6{,}841 \div 9 = 760$. It seems absurd, therefore, to compare this with the enormous forces exerted by lightning, seeing that it is hardly equal to raising the heat of a pound of water one degree Fahrenheit.

We must remember that when Faraday made this statement he was only beginning the study of electric quantities and relations, our present knowledge of which is largely due to his work. At that time nothing but vague generalities had been attained to, and little was known, or even imagined, as to the nature of electricity. Our present exact system of measurement was undreamed of, and Faraday was therefore unable to perceive that he was comparing together things having no relation such as he was working at; but it is strange that his statements should be constantly quoted as if they contained real truths, and that their true interpretation has not yet been clearly shown. Before attempting this it will be well to bring together the actual statements of Faraday and other workers on the same lines.

Faraday found that to decompose a grain of water required a current for three and three-quarter minutes, capable of keeping red hot a platinum wire $\frac{1}{104}$ of an inch in diameter. The very vagueness of such a measure at once shows the difficulties under which he labored. We can express this value definitely as a current, for the time given, of 3.13 ampères; or, better still, as a "quantity"

of 704.37 coulombs, the coulomb and ampère representing chemical action equal to .000158 grm. of hydrogen, or .00142 of water. He found that to effect the same decomposition by the machine required 800,000 charges of a Leyden battery of fifteen jars, each of 184 square inches of coated surface. Each of these charges, produced by 30 turns of a 50 in. plate machine, was capable of killing a rat, and was equivalent, chemically, to the action of a platinum and zinc wire, each of $\frac{1}{18}$ of an inch in diameter, dipping $\frac{5}{8}$ of an inch into 4 oz. of water containing one drop of acid, and developing current for about three seconds. Weber has also calculated that this charge from one grain of water, if placed on a cloud 1,000 meters (3,281 ft.) above the earth would exert an attractive force equal to 1,497 tons. So we have this enormous force and the potential slaughter of nearly a million of rats, originating in water, which we know is only possessed of less than 800 foot-pounds of energy, just about the amount of work a man ought to do in every five minutes of his day's labor.

De la Rive says: "Becquerel succeeded in finding that in order to decompose a gramme (15.43 grains) of water a quantity of electricity was required, equivalent to what would be furnished by 51,586,400 discharges of a battery having one square meter (1.9609 yds.) of surface, reduced to 20,063,456 when the charge of the battery is at its maximum. On reducing Faraday's result to the same conditions we find 21,850,451. The difference is very inconsiderable for experiments of this nature." This remark shows how very imperfect was the then existing knowledge. The value of the comparison depends wholly upon the potential given to the charge, not merely on the coated surfaces; and in the actual figures this shows a variation between 51 and 20 millions. He goes on to say: "Adopting the round number of 20,000,000 for one gramme, in order to decompose a milligramme (.0154 grain) of water, there are required 20,000 discharges of a battery of 1 square meter surface, or an electric pane having a surface of about 5 acres (Faraday's figures give 352 acres). Now, it is this same quantity of electricity that must be produced by the decomposition of this same milligramme of water by chemical means; and if it were accumulated so as to discharge itself instantaneously, it would be capable of producing the effects of a flash of lightning."

Weber established the relation between the statical and dynamical effects of the charge of a Leyden jar by measuring the first with the torsion balance, and the other by means of a galvanometer, to which the charge passed as a current by means of a long column of water which it traversed. His results were as follows:

"Through each section of a conductor, traversed by a current whose force is equal to the electro-magnetic unit, there passes, in a second of time, a quantity of electricity equal to $155,370 \times 10^6$ static units, an equal quantity of negative electricity traveling in the opposite direction.

"The quantity of electricity required to decompose 1 milligramme of water amounts to $106\frac{2}{3}$ times this quantity (this being the ratio of the electro-chemical to the electro-magnetic units) or $16,573 \times 10^9$ units of electricity of each kind; to decompose 9 milligrammes, or 1 equivalent, requires, of course, 9 times this quantity. This quantity of + electricity $(9 \times 16,573 \times 10^9)$ accumulated on a cloud 1,000 meters above the surface of the earth, would exert an attractive force equal to 226,800 kilogrammes, or 208 tons."

It is further calculated that if all the atoms of hydrogen in one milligramme of water in the form of a column of one millimeter long were attached to a thread, and all the atoms of oxygen to another, then to effect the decomposition of the water in one second, the two threads would require to be drawn apart with a force of 147,380 kilogrammes, or 145 tons. The thread would need to be tolerably strong, seeing that this immeasurably exceeds the tensile strength of any known material; that is to say, this imaginary force, mathematically demonstrated, is greater than any cohesive force known to exist.

Now, let us translate this into modern expressions. We have seen that the dynamic value of one grain of water is 704.37 coulombs. What is the corresponding static expression which will replace the 800,000 charges and 325 acres of coated pane? We require a condenser

of capacity capable of receiving a charge of 704.37 coulombs, and the mere dimensions of this will depend upon the potential of the charge; starting with the unit 1 volt, and taking the capacity of the usual cable as 3.5 micro-farads per mile, we find that a force of 1 volt (say a Daniell cell) would require 2,465 million miles of cable to receive it, or speaking roughly, enough to wind 1,200 times round the earth and moon, or go twice round the orbit of the earth; that is to say, the earth would take two years in "paying" it out.

Let us put it another way: the (imaginary) capacity of the earth is 630 millions of electro-static units, or 700 micro-farads; this quantity (throwing in the odd 4.37 millions) would charge to 1 volt a million such worlds as ours! or, allowing the potential which Messrs. Ayrton and Perry calculated as necessary for their theory of the earth's magnetism—viz., 54 million volts, it would only require 54 grains of water to supply the required electricity, which, however, Prof. Rowland also calculated would result in bursting the earth to pieces.

Leaving all these fantastic unrealities in which some people delight, let us examine the common sense of the matter. Electricity is nothing but a word in which certain natural phenomena are embodied, and it involves two wholly distinct functions, both of which are commonly called "electricity;" for instance, people talk of the torrents of electricity poured forth in a flash of lightning, and the torrents of electricity streaming out of a dynamo-machine, and the idea of "quantity" in these two is wholly different; each belongs to one of the two functions which together constitute electricity. Those two functions are: (1) The molecular construction of *matter*—that to which the idea of the "grain of water" is collated, and which we may symbolize as Q, for quantity; (2) The stress put upon those molecules, the "potential" symbolized by E for electro-motive force. Static electricity has no existence as a "quantity;" it is simply the evidence of the stress existing in that class of matter which is called di-electric, or which resists the passage of electric current. All the ideas we have been considering are based upon the impossible isolation of Q; the real phenomena always depend upon $Q \times E$. Matter and energy together constitute electricity.

Now in the case of water the E is a potential of volt 1.5, and what we have to take into account in the case of the grain of water is, therefore, $704 \times 1.5 = 1,056$, which multiplied by .7373 gives us a little over the 760ft.-pounds, which we know is the total energy of the grain of water. Let us now consider the case of the lightning flash, the Q of which is this same 704 coulombs, or the grain of water. The EMF of the lightning flash is very uncertain; it is calculated, from experiments with De la Rue's 14,000-cell battery, that the EMF of a flash one mile in length is 3,604,000 volts, and this by the same process gives us 1,872 millions of foot-pounds, and enables us to realize why this "grain of water" can produce such great effects when concentrating this amount of energy in some point of high resistance which appropriates great part of it as "work" in possibly one millionth of a second.

But the *current* of such a flash is not, as some say, a very small one, because current is $Q \div T$, related to time, and the ampere means 1 coulomb per second. We have here, then, 704 coulombs, passing in a time variously estimated as one 20,000th to 1 millionth of a second; taking the lowest estimate, we have a current of 14 million amperes, greater than all the dynamos on earth could generate. From this it is pretty clear that no lightning-conductor is ever called upon to carry such a current; either it lowers the potential so gradually that the actual flash when it occurs is reduced to a very small one, or, on the other hand, it only carries a small part of the energy of the discharge, which is largely distributed over surrounding space.

It should be clearly realized that the distinction of static and dynamic electricity is artificial and misleading as usually conceived. *Frictional* electricity differs from that of the battery only in being produced in a circuit of highly resisting substances, and therefore under high potential. But so-called *static* electricity has no existence at all; it is a name which includes either *stresses* set up in dielectrics —that is, actions of energy in an *inductive circuit*, or else of very small currents in a *conductive* circuit of great resistance,

all dielectrics having some small conductive capacity.

The static phenomena are exactly parallel with the dynamic. It may be said that there is this difference: that an electric current requires a continual expenditure of energy to maintain it, while the static charge, once produced, may be (theoretically) maintained indefinitely without the expenditure of energy. The theory which says so is a mathematical one, commencing with the favorite formula, "Let us assume." That a charge can be so maintained is an assumption contradicted by every known fact. The fact that no such charge has ever been produced is explained, in order to fit the theory, by the invention of "leakage"; that is to say, there exists no such perfect dielectric as the theory requires for its foundation; all dielectrics are partial conductors, and this means *current.* But even if we imagine such a dielectric, it would involve, not static *electricity*, but energy, stored "potential," as strains upon the molecules of the dielectric; just as we have in the second battery, not electricity, but energy, stored potential as chemical forces. Further, no such charge could prove its existence as a charge.

When it moves an indicator of any kind, that involves energy expended and current passing. The simple fact is, that the current is infinitesimal and the energy inappreciable. But we have just such currents in dynamic electricity. Experiments have been made as to deposit of minerals, in which the resistance of the depositing cell is so great, that the current would need a century to deposit one grain of metal, and yet that current can indicate itself upon a galvanometer.

In such experiments the depositing cell is a static field, a condenser, in which only a small force exists. Its electrodes indicate a static charge, exactly as do the static conductors, and the electrolytic cell and static condenser are exact analogues of each other.

I think these relations are more clearly shown in a tabular form, which contrasts the two circuits and principles, than in any other manner, and therefore I append such a table, which I have prepared for my book.

Static	ELECTRICITY	*Dynamic*
Energy *potential*: stored in a field of force as stress in a dielectric.	means	Energy *kinetic*: acting in a conducting path, along molecular chains.
Inductive	CAPACITY	*Conductive*
Area of dielectric.	varies as	Area of conductor.
Spec. ind. capacity of dielectric.	"	Specific conductivity of material.
Thickness	inversely as	Length.
Units of Charge stored under unit Potential.	is measured by	Units of current transmitted under unit EMF.
The Potential in volts required for unit charge.	or by	The EMF in volts required for unit current.
Inductive	RESISTANCE	*Conductive*
Reciprocal of inductive capacity.	is the	Reciprocal of conducting capacity.
CHARGE	ELECTRIC	CURRENT
Potential, in volts.	is as	Electromotive force, in volts.
Resistance	inversely as	Resistance
Lines of force under stress.	is related to and dependent on	Lines of equivalent molecules forming chains.
Inversely as the several resistances.	divides among several circuits	Inversely as the several resistances.
	ENERGY, OR WORK.	
Square of charge in unit or equal resistance.	is as	Square of current in unit or equal resistance.

FORMULÆ.,

Charge	$\frac{E}{R}=Q$	Current	$\frac{E}{R}=C$
Potential	$Q\times R=E$	Electromotive force	$C\times R=E$
Resistance	$\frac{E}{Q}=R$	Resistance	$\frac{E}{C}=R$

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—NOVEMBER 7th, 1883.—A paper by Mr. Edwin Thacher, M. Am. Soc. C. E., was, in the absence of the author, read by the Secretary, the title being: "Description of a Combined Triangular and Suspension Bridge Truss, and Comparison of its Cost with that of the Warren, Pratt, Whipple, and Howe Trusses." The writer presented drawings and descriptions of a truss formed by a combination of the triangular and suspension systems, the primary system being composed of top and bottom chords, and a web of struts and ties arranged in the form of triangles, free to change figure from the effects of temperature. The center ties extend each over not less than two panels, and over not more than the number in half span, less one. A careful analysis was presented of the strains in these bridges; the paper also having tables giving the uniform live loads, which may be substituted for the wheel loads, of the leading types of 10-wheel and consolidation engines, in spans ranging from 10 to 500 feet. The writer discussed the defects of various forms of bridge truss, com-

pared with what were considered the advantages of the particular truss described in the paper. He also presented estimates of the cost of bridges built upon this principle as compared with the cost of the Warren, Pratt, Whipple and Howe trusses, deducing a lower result for all spans built by this method. He presented comparative estimates as to the economy of bridges built entirely of iron as compared with those built with the combination of wood and iron, with the result that the ultimate cost of the combination bridge was considerably lower than the other.

Several bridges built upon this plan, at various points in the United States, were described.

Engineers' Club of Philadelphia—Regular Meeting, October 6th.—Mr. Edward Thiange presented an illustrated description of a method of earthwork computation by means of diagrams, constructed from the proposition: "The areas of similar figures are to each other as the squares of their homologous sides." An idea may be had of their nature and uses by the following directions: To get the average volume in cubic yards of a station (in embankment), to the center fill at each end add the constant height of the "grade triangle" (which is formed by the road bed and the side slopes produced); at the resultant heights on the diagram, measure, with the scale of cubic yards, the lengths of the ordinates terminated by the slope lines at each station respectively; their sum, diminished by the "grade prism," is the average quantity for the station of 100 feet.

A paper upon "Economy in Highway Bridges," by Prof. J. A. L. Waddell, was read. Its objects are to determine the most economical depth and number of panels for spans from 40 to 200 feet; the lengths at which it is better to change from pony truss to thorough bridge, and from single to double intersection; the exact dead loads, and the amounts of lumber and iron for each case.

The Secretary presented a photograph and description of the Lift Bridge for double track railway over Oswego Canal, N. Y. W. S. & B. R. R., kindly contributed to the Club by Mr. Jno. A. Partridge (Member Am. S. C. E.), Div. Engr. Length of truss, 94 feet; height, 23 feet; width, 30 feet 4 inches; angle with center line, 38°; weight of bridge with machinery, 146 tons; weight of counter-weights, 138 tons; height of lift from bridge seat, $10\frac{1}{2}$ feet; time required for lift, 30 seconds.

Regular Meeting, November 3d.—"Notes upon Roads, Streets and Pavements," by Mr. Chas. H. Haswell, were presented. Classification, grade and construction are first treated, and then special notes made with regard to Macadamized, Telford, gravel or earth, corduroy and plank roads, and asphalt, wood, block stone, rubble and concrete pavements. Miscellaneous notes upon road making, complete a very compact collection of data.

Mr. E. A. Geiseler read an illustrated paper upon "Tides and Newton's Theory of Them."

Mr. Allen J. Fuller presented notes upon the "Effect of Frost upon Fire-plug Castings." He referred to a general impression that the freezing of the earth around fire hydrants has a tendency to grip fast to the frost jacket, and lift it with the expanding or heaving earth, which he denied for the following reasons:

1st. The frozen earth slides on the surface of the frost jacket, because its expansion is greater than that of iron.

2d. As the expansion of the earth must be in proportion to the intensity of the cold, so will it be greater above than below a given point, therefore, the first foot of frozen ground will have a greater upward movement than that which is below it, and the second foot greater than the third, etc. Thus it will be seen that the earth below a given point rises more slowly than that above, and its friction is opposed to the one above.

3d. If this is true of feet it is true of inches, and of portions of an inch; therefore, there is a retardation movement throughout.

4th. The upward movement of the ground, the freezing being greatest towards the surface, and such movement involving a more complete fracture of the earth surrounding the frost jacket, it follows that the friction is less at this point than that below it, and in consequence there is less power to move upward than downward.

Of course, the above does not appiy to any construction that the frost can get beneath.

Mr. Frederic Graff noted and described the form ot wooden casing which had been successfully used in the early practice of the Philadelphia Water Department.

In response to the theory advanced in regard to the action of frost in raising the casings of fire-plugs, and to the statement that if the base of a structure extended below the frost line it would not be lifted, Prof. Haupt remarked that he thought the theory was in part sustained by the fact observed by some of the district surveyors, and verified by the accurate measurements they were obliged to make, that fences moved bodily to the South and East in consequence of the action of the sun and frost upon the ground on opposite sides of them. He thought, also, that the deductions concerning the immobility of structures resting below the frost line was not fully sustained by the facts, as, in the Northwest where ice forms rapidly, he had heard of numerous instances of piles, driven for bridges and extending some distance below the frost line, having been raised as much as five to six inches in a single night, and he conceived the action in this case to be similar in kind to that of piles driven entirely through solid ground, the only difference being in the amount of the resistance offered by friction and weight of pile. The water in freezing around the pile acts upon it as a gripper or vise, and the expansion of the various strata or laminæ of water as they become converted into ice act as levers to force up the pile.

The Secretary did not consider the case cited by Prof. Haupt as parallel, as the so-called piles being driven through water and soft mud, were probably columns resting upon their

bases and depending but little upon the frictional resistance of the material through which they passed. Therefore, the expansive force upward of the freezing water would be opposed by little more than the weight of the pile, whereas in a fire hydrant casing or other deeply planted post, the presumably well-rammed material around the whole length under ground would offer such proportional frictional resistance, as to cause the freezing earth to slide up the post rather than to lift it. If the ice could be supposed to act downwards upon the piles in question it is hardly likely that it would have forced them further home.

Prof. Haupt also exhibited a "History of the Manual Arts, or the Inventions of Human Wit," published by Mr. Herringman, London, 1661, and read extracts therefrom describing the clepsydrae or ancient water clocks.

The Secretary read the following account from the *Mexican National* of Laredo, Texas, of a bridge construction by Mr. C. A. Merriam, Member of the Club, Asst. Supt. and Engr. F. Div. M. N. Ry. "On the 6th day of September (the anniversary of loss of bridge last year) the Mexican National Railroad bridge was carried away by high water. On Monday the 16th the first pile was driven for the new structure, which was completed on the 23d ult., and trains are now running regularly. This is pretty quick work, the erection of a bridge 600 feet long in seven days."

The Secretary narrated his experience on behalf of the Club, and read extracts from correspondence, etc., with the Custom House, through the stupendous inscrutability of the management of which the Transactions of the Society of Engineers of London, and of the Institution of C. E.'s of Ireland, are charged with duty, and all the other foreign societies upon the exchange list of the club are admitted free.

ENGINEERING NOTES.

The Lower Thames Valley Sewage.—We last week referred to the circumstance that there was at lengh a probability of the question of the disposal of the sewage of the Lower Thames Valley being solved by a scheme propounded by Messrs. Mansergh and Melliss, C. E., of Westminster. At that time the question of site was under the consideration of the local authorities, three places having been referred to in Messrs. Mansergh & Melliss' report as appropriate for collecting and dealing with the sewage. These were, respectively, Barnes, Mortlake, and Ham Fields, and the Mortlake site has now been selected for that purpose. We now append particulars of the method of utilization, which it is proposed to adopt, and which method is already in operation at Chiswick, on the opposite side of the river, with satisfactory results. This being so, and as the details of the works have not yet been decided upon, it will make the matter clearer if we describe the existing works at Chiswick, the principle remaining the same whatever the locality of the works. The sewage is brought by gravitation to the pump well in the sewage works, and is then pumped into tanks, the total lift being 19 feet. The chemicals used are crude sulphate of alumina and chalk lime, to which is added a little yellow clay. The milk of lime and the alumina and clay are mixed together by machinery and run into the pump well, where the action of the pumps secures a complete incorporation of the chemicals with the crude sewage. The sewage in the well has a dark brown tint, and is without any smell, as the rapidity with which it is delivered to the well prevents decomposition setting in. The whole of the solid portions of the sewage are broken in their passage down to the sewers, so that they reach the works in solution. A large strainer holds back the solid matter not in solution, and which might damage the pumps on its passage to the tank. The engines are in duplicate, so that in case of accident or during repair the work may be proceeded with. The sewage, mixed with the chemicals, passes into the distributing channel, from which it is run into any one or any number of the eight tanks which receive the sewage. Four of these tanks hold 50,000 gallons each, and four 150,000 gallons each. When a tank is filled it is allowed to rest, and precipitation immediately commences. In about three hours the process is completed. At one end of the tanks a floating valve, which has its mouth one inch below the level of the water and which rises and falls with it, is placed, by means of which the surface water is drawn off without disturbing the precipitated matters at the bottom. When this valve is opened the clarified water rushes into a lower channel, passes through a large filter bed filled with coke, which is afterwards used as fuel, into a third channel, and then by way of the effluent pipe, into the river. The water is now clear, bright, and inodorous. It is only claimed for this process that it purifies the sewage so that the effluent water can be turned into the river without causing any nuisance or detriment. It is not, however, claimed for it that it rescues all the valuable manurial properties from the sewage, as the free ammonia is not precipitated by it. The bottom of the tank from which the top water has been run off is covered with a thick, black sludge, which, when treated immediately, does not give off any offensive smell. The tank bottom inclines towards the engine-room, and to this end the sludge gravitates. From here it is lifted by a pump into an underground carrier and deposited in earth tanks, wherein the water mixed with it evaporates and percolates away until the sludge becomes portable. It is then given away to market gardeners and others, as the local authority have not hitherto sought to realize any profit from it, and is applied to land with good effect.

The Hydromotor.—We have a report of a trial which took place on October 11th, on the river Elbe, near Dresden, of a vessel, the name of which, "Hydromotor," explains the principle of propulsion. The writer of the report states that the invention "promises to effect an important change in the propulsion of vessels of all classes." He adds: "The inventor, Dr. Emil Fleischer, has applied the principle of hydraulic reaction to the propulsion of ships in a manner which, according to the testi-

mony of officers of the German Navy, is as simple as it is effective. It does completely away with all risks from accidents to rudder or machinery, for there need be no rudder, and there is scarcely any machinery; solves the problem of avoiding loss of power through transmission, for there is hardly any transmission; and, finally, reduces the perils of the sea to a minimum, so far as injuries to the machinery are concerned. For, the same immense force that propels the vessel can, at a moment's notice, be used to pump her so effectively that she would remain afloat with a considerable leak in her bottom; while in case of fire there would be no diffiulty in quenching it." The writer then gives an account of the trial of the vessel, which is 197 feet long by 23 feet beam, and draws 2 feet of water and is constructed of iron. "The navigation of the Elbe near Dresden," he says, "presents many obstacles in the shallows and rapids which occur at frequent intervals. Noiselessly, and without any oscillation, did the large vessel—large as compared with the steam-craft plying on that part of the river—after the simple turning of a lever by the captain on the bridge, commence its trial trip, stemming the current, and keeping an even course under the right bank of the river. The only noise audible was that of the rushing of the water from the tubes fixed a little above the level of the river, and nearly amidships, on both sides of the vessel. Another turn of the lever and the action was reversed. The vessel comes to a dead stop in less than her own length. By the alternate use of the levers she may be turned around on an almost stationary pivot. The captain handles the levers on his bridge independent of all communication with the engine-room." We are finally told that arrangements are now being made to apply the invention on a very large scale. Lacking a detailed description of the vessel tried in Germany, it is impossible to pronounce an opinion upon its merits; but, from what has appeared, the "invention" seems to be a resuscitation of the principle first applied on a large scale by Ruthven, over seventeen years ago, to the Nautilus, and later to the well-known Waterwitch, and one or two other vessels. Mr. Ruthven worked out the principle as early as 1839, and applied it to a small vessel 9 feet long, and later on, in 1844, to a larger one, 40 feet in length. Notwithstanding all his efforts, however, to induce shipowners to adopt his mode of propulsion, he failed, for the simple reason that, as compared with screw propulsion, power for power, the hydraulic propeller lagged behind the screw by about half a knot per hour. Unless Dr. Fleischer, therefore, has succeeded in meeting that objection, he will taste of the fruits of failure and disappointment just as Ruthven, and many an enthusiastic inventor before him, did.

IRON AND STEEL NOTES.

THE POSITION OF THE STEEL RAIL TRADE.—The outlook in the immediate future before steel rail manufacturers is grave in the extreme. Prices which have fallen almost uninterruptedly since the beginning of 1882, are now at a lower figure than they have touched since the midsummer of 1879. According to report, one of our southern railways has given out within the last fortnight a contract for about 4,000 tons of steel rails at a price under £5 per ton delivered in the Thames; and it is difficult to see how the makers can realize from this more than £4 10s. per ton at their works, if that amount will be left to them after paying freight and other charges. This is bad enough, but it is by no means such a desperate state of matters as was described in the sensational story to which a contemporary gave prominence about two months since. It shows, however, that prices are still tending downwards and we are afraid that they have not yet reached the bottom. So far as can be judged at the moment, balancing the probable demands of consumption against the power of production, the next year or two will witness a sharp struggle for existence amongst steel rail makers. Persons well versed in the iron trade look upon the steel rail branch of it as the one before which the most gloomy prospect lies. It behoves manufacturers, therefore, to study the situation carefully, and not, ostrich-like, to blind themselves to the facts that surround them, and drive on in a happy-go-lucky fashion, trusting that things will improve.

The cause of the existing state of things ought to be well known to every one connected with the iron trade, yet it would appear from the policy pursued by many manufacturers that they are oblivious to it. Over-production lies at the root of the evil. During the nine months ending September 30 this country exported, according to the Board of Trade statistics, 579,421 tons of steel rails, as against 552,555 tons during the corresponding period of last year. In the face of these figures it cannot be said that the demand has fallen off, for the quantity going into home consumption, from all appearances, will be very much the same in both years. This increase in the quantity of steel rails shipped is the more remarkable when the falling off which has taken place in our total iron exports is remembered. Given a growing consumption, accompanied by receding prices, and there is nothing left but to come to the conclusion that the power of production has been increased too rapidly; otherwise, manufacturers must not object to the charge of over-production, so long as they continue to complain of the low level to which prices have come. The enormous strides which have been made of late years in the production of Bessemer steel form one of the most remarkable features of modern industrial progress. The output has been more than quadrupled during the last decade, for, from 410,000 tons in 1872 the production has risen by leaps and bounds to 1,673,649 tons in 1882, and the increase since 1880 alone has been more than 50 per cent. That this astonishing rate of expansion can be maintained appears incredible, but we believe that the production this year will prove to have been still larger than it was in 1882. At the end of last year ten converters were in course of construction, some of which have come into operation already; while the policy of existing works, down to the present, has been to increase the output as far

as possible, so as to make up for a diminishing margin of profit by a larger turnover. But supposing that last year's production is simply maintained this year, and leaving the question of any increase on the one side, let us see what it means. Of course the whole of the Bessemer steel made is not turned into rails, but the greater proportion of it is. In 1882, out of the 1,673, 649 tons of ingots produced, about 1,325,000 tons were rolled into rails, of which the output that year was 1,235,785 tons. This year the proportion which the make of steel rails will bear to the production of ingots is likely to be considerably greater, because the export demand for blooms having almost entirely ceased, the manfacturer will have more steel to roll into rails, unless an outlet can be found for the metal in some other form. It will be sufficient, however, to base our examination upon the production of last year. Taking as an average section of rail one weighing 56 lb. per yard, we find that the production of this country in 1882 was sufficient to cover 14,044 miles of single track, that is to say, that in 18 years, without any further increase in its power of making steel rails, Great Britain alone could double the existing railways possessed by the world. In less than two years it could relay the lines in operation in India, Canada, and the whole of our Australian colonies, and within another year could equip all the railways of the United Kingdom. But it is not only in this country that the manufacture of steel has been brought to such a pitch; the production in the United States has been even more remarkable, the quantity turned out by the rolling mills of that country in 1882 having been 50,000 tons more than our own, while on the Continent rapid advances have been made likewise. In 1882 the world's production of Bessemer steel is estimated to have exceeded 5,000,000 tons, and from the information we can obtain the quantity of steel rails rolled was probably not less than 3,750,000 tons. Some idea of the meaning of this enormous quantity will be gained when we state that it is sufficient to relay in six years all the railways built in the world. The capability of the world to continue absorbing rails at this rate may well be doubted. Of course the total mileage of railways already in existence is comparatively trifling, being estimated about 265,000 miles, and there is still a vast field for its extension—Asia, Africa, and South America being as yet but very sparsely furnished with iron roads —but in the very nature of things, the growth must be gradual. It is seldom that the construction of a line of railway is undertaken until the necessity for it has been demonstrated, and it has become a matter of importance to join two centers of trade or to open up a rich district. It is only in America that they build a road running from nowhere to nowhere. Moreover, the making of a railway is a costly undertaking, and even where governments take the work in hand the sinews of war are the chief consideration. Hitherto the building of railways has proceeded by fits and starts, and after every period of activity there has been a time of reaction and comparative cessation of work. It is only the operation of the natural law that all exertion must be followed by repose. Such a period of activity has been experienced during the last three years, and if the unprecedented rate of railway construction which has prevailed throughout the years 1881–3 has not been sufficient to raise the price of steel rails even to a moderate level, what is to be expected when stagnation sets in? As we have seen, the power of production of the works in this country alone is already more than 50 per cent. greater than it was previous to the burst of activity in railroad making. Any further increase is nothing short of suicidal.

But to every cloud there is a silver lining. In considering the prospects we have confined our remarks so far to the immediate future, say the next three or four years. Those works that can manage to survive the struggle will, in all probability, find plenty of work to do. The low range of prices which may be said to be inevitable will stimulate consumption, and as our colonies and dependencies increase in population and wealth the present mileage of railways will not satisfy their wants. In fact the existing mileage is as nothing to what well-populated countries can maintain. The number of miles of railroad open in the United States is about 116,000. Messrs. Poor, in their *Manual* for 1883, say: "Included in the available area of the United States are 3,000,000 square miles. A ratio of one mile of railroad to ten square miles of area will give 300,000 miles of line. Construction will proceed uninterruptedly until such an extent of mileage is reached." The President of the Iron and Steel Institute, in addressing the May meeting of that body, remarked that India, with a population of 250,000,000 has less than 10,000 miles of railway, whilst the United States, with only 50,000, possesses more than 100,000 miles. Such figures show the scope there is for new railways, but their construction is a question of time.—*Iron.*

RAILWAY NOTES.

From data just published it appears that the number of locomotives running on German railways at the end of 1881 was 10,869, only 530 of which were supplied by foreign manufacturers. Borsig, of Berlin, supplied 2,345 engines; next came the Maffei Company, with 966; Hannoversche Locomotivfabrik, 939; Henschel and Sohn, 871; Sachische Maschinenfabrik, 768; the Vulcan Company, Stettin, 755; the Karlsruher Maschinenbau-Gesellschaft, 638; the Esslingen Company, 634; Wohlert, 629; Schwartzkopff, 574; Krauss, 278. The above firms represent the principal German locomotive builders. The railway companies constructed in their own shops only 241 engines. Of the locomotives imported, 286 came from Austria, 193 from England, and 51 were built in Belgium.

The directors of the Midland Railway Company has just completed the purchase of all the Pullman drawing-room cars running on its line from London to Liverpool, Manchester, Glasgow, and Edinburgh. It is understood that the Midland Company takes possession on the first of November, and the special car conductors have all had notice to leave the service

of the Pullman Car Company. The Midland intends to utilize these cars as first-class carriages without any extra charge. It is expected that all first-class passengers will travel in the cars, and that this will enable a number of first-class carriages which at present run half empty to be tsken off the trains. Each Pullman car weighs about 21 tons, and as two are attached to the Scotch express and the 5 p. m. express from London to Manchester and Liverpool, a great loss is incurred in drawing them when they are frequently nearly empty. The change will secure a reduction in the dead weight of the trains, and the use of two engines on many trains will be avoided. As a second engine costs about 1s. extra per mile run, there will be a large saving in working expenses. The Pullman Car Company still retains the sleeping cars.

ORDNANCE AND NAVAL.

The change that is taking place in the tonnage of the vessels that do the carrying over sea is very well exemplified by a statement of the tonnage that left the port of Sunderland during the first half of the present year. In that period the average tonnage of the vessels frequenting the port had increased by about 14 per cent. In the number of vessels there is a considerable variation. Comparing the half year with the first half of the past year, it is seen that of vessels frequenting the port of less than 150 tons register there has been a decrease of 224, and of the vessels between 150 and 250 tons, the decrease is 53 in number; but all above that show an increase. Between 250 and 350 tons the increase is 36; between 350 tons and 500 tons it is 4; between 500 and 750 tons it is 104; between 750 tons and 1,000, it is 46; and above 1,000 tons the increase is 29. It is thus evident that small vessels are less frequenting the great port of North Durham, and that the work is done by fewer and larger vessels; and this experience will probably be found to be general, for the tendency is marked towards the use of steam-propelled vessels, whilst the small sailing craft that a few years ago were the chief support of the ports are dying out. The result is that there is a tendency towards the use of the ports that can offer the fullest facilities for vessels of large tonnage, and as Sunderland has very considerably increased its facilities of late, it finds the benefit of the change, and some of the smaller ports lose. And probably that change will go on, for it is not likely that the small vessels that now exist will be renewed when they are removed from the register; so that the ports that prepare themselves with the fullest facilities for the loading of large vessels will reap the benefit.

The 48-Ton Gun.—The most important trial of naval gunnery which has ever taken place off the port of Plymouth took place recently, when the *Agamemnon*, double screw turret-ship, was taken into the offing for experiments with her four 38-ton guns. The *Agamemnon*, which was built at Chatham from designs of Mr. Nathaniel Barnaby, may fairly be regarded as a small *Inflexible*. Her style is the same, and her fittings are identical. The objects of the trials were not so much to test the accuracy of the gunners as to indicate the resisting power of the ship, when it became necessary to discharge any or all of the powerful weapons with which she is armed. On leaving Plymouth Sound, a course was at once set for the eastward of the Eddystone, where it was intended to commence experiments. It was 10.30 before the guns of the fore turret commenced to fire, and the first two rounds were of a very unimportant character. Scaling charges of blank powder had been introduced in order to test the efficiency of the guns. These were fired in quick succession, and then commenced the real work of the day—the firing of the right and left gun on the port beam horizontally directed with a half charge of 105 ℔. The effect on the ship by the firing of these guns was in no way felt. Every precaution had been observed in connection with the experiments, and inasmuch as fire might have been left in the guns, owing to their being "chambered," it was resolved to inject water into them after they had been searched by means of the sponge. In this way there was no fear of fire remaining and igniting the charge which might be soon after placed in the piece of artillery. The guns in the fore turret were then fired in rapid succession with full and battering charges, the former representing the discharge of 157 ℔. of powder with 800 ℔. of shot, and the latter 210 ℔. of powder with similar projectiles. The guns were also fired simultaneously with battering charges. Similar experiments were then made with the guns in the after turret, and some delay here occurred by the giving out of one of the packing presses, which was struck with great force by the recoil of the gun from the full charge. The most severe test of the day took place at about 4.30, when all four guns were fired simultaneously. The guns were charged with 840 ℔. of powder and 3,200 ℔. weight of shot. The effect of the combined explosion on shore has been reported to have been very great. On board, however, but a slight concussion was felt. As the result of the day's experiments, the *Agamemnon* is regarded as one of the most powerful additions to the British fleet. With the minor discharges of cannon the greatest damage done was the destruction of glass and the carrying away of boats' covering. When the combined firing of the four 38-ton guns took place, the temporary bulkhead underneath the superstructure was driven out and the angle irons were twisted. This bulkhead had only been erected at Devonport as a protection to the boilers for the new engines put into that ship to drive the hydraulic machinery. The weakness was at once detected, and will be remedied. The automatic sights and the loading and elevating gear worked in the most successful manner.

The New Russian Artillery.—The *Moskoya Sbornik* publishes particulars of the new artillery adopted for the Russian Navy, from which we take the following information. Prolonged and careful experiments have been made during a considerable time, for the pur-

pose of ascertaining what weapons, powder, and projectiles could with most advantage be substituted for the clumsy and inefficacious material hitherto in use. The experiments of the Krupp firm were taken as a basis, and the result of several trials has been the adoption of models upon which the new guns are being manufactured at the Obukow foundry. In the four-pounders (9 cm.) and eleven-pounders (11 cm.), the rifling has an increasing twist, and its angular inclination divides it into three sections; at the commencement it is rectilinear, it next follows an arc of a circle, and afterwards becomes again rectilinear, both rectilinear portions being tangential to the arc. The charge is fired from above, and the vent is inclined at an angle of 41 deg. to the axis of the bore. The following Table gives the principal details:

Items.	4-pounder. (9 cm.)		11-pounder. (11 cm.)	
	ft.	in.	ft.	in.
Total length of gun.	6	10	6	11
Length of rifled tube............	5	2.25	5	1.2
Length of chamber.	0	10.75	0	10.5
	cwt.	lb.	cwt.	lb.
Weight of cannon without breech-block............	8	100.5	12	29.5
Weight of breech-block............	0	103.5	1	40
Weight of projecile.	0	16.75	0	30.5
Weight of charge...	0	3.1	0	5
	ft.	in.	ft.	in.
Initial velocity......	1476	10	1355	0
Number of grooves.	24		24	

The new 9 inch guns have not yet been definitely determined on. The 28 cm. guns of the new type have also been constructed at Obukow, and are to be used for arming some of the ships of the fleet, especially the gun-boat Tuscha. In the comparative trials of accuracy in shooting between the old and new guns, the deviation of the former was vertically 5 ft. 3.38 in., and horizontally 5 ft. 5 in.; while that of the latter was only 12.6 in., and 3 ft. 2.58 in., respectively. The following are the principal dimensions, &c.:

	28 cm. canon.		
	ft.	in.	
Total length of gun............	19	10	
Length of rifled tube............	13	0.5	
" " chamber............	4	0.75	
Number of grooves............	64		
	tns.	cwt.	lb.
Weight of gun with breech-block	30	19	14
" " projectile............	0	4	72.25
	0	5	48.5
" " charge............	0	1	33.5
	ft.	in.	
Initial velocity............ ...	1,640	5	
	1,312	4.25	

The experiments with this gun and others of still larger calibre proved the necessity of deciding upon the species of prismatic powder most suitable for heavy charges with large projectiles, *i.e.*, upon the density and dimensions of the grains. The powder at present in use with heavy projectiles has a density of 175, and produces so great a pressure against the sides of the barrel that it is considered impossible to increase the charge further with a view to obtain the desired initial velocity. Experiments are therefore still being carried on, both by the Messrs. Krupp and the Russian powder manufacturers, and some more satisfactory results have already been obtained, though no final decision has yet been arrived at.—*Engineering.*

BOOK NOTICES

Publications Received.

Annual Report of the Chief of Engineers of the United States Army.

Report of the United States Commissioner of Education.

To Mr. James Forrest, Secretary of the Institution of Civil Engineers, we are indebted for the following papers:

Continuous Girder Bridges. By Thomas Claxton Fidler, M. Inst. C. E.

Water Supply and Irrigation of Canterbury Plains, N. Y. By George Frederick Ritso, A. M. I. C. E.

Blasting a Channel in the River Yarra. By Joseph Brady, M. Inst. C. E.

The Preservation of Iron. By Benjamin Howarth Thwaite. A. M. Inst. C. E.

A Deep Boring at Northampton. By Henry John Eunson, Student Inst. C. E.

Treatment of Complex Ores. By James Warne Chenhell, A. M. Inst. C. E.

Raising the Steamship Austral. By John Standfield, M. Inst. C. E.

Graphic Methods of Computing Stresses. by Charles Ormsby Burge, M. Inst. C. E.

The Waterworks of Edinburgh, Port Elizabeth, S. A., and Peterborough. By Alexander Leslie, M. Inst. C. E.; John George Gamble, M. Inst. C. E., and John Addy, M. Inst. C. E.

Signal Service Notes. Prepared under direction of Bvt. Maj.-Gen. W. B. Hazen.

No. 1. Report of the Michigan Forest Fires. By Wm. O. Bailey.

No. 2. The Use of Homing Pigeons for Military Purposes. By Wm. E. Birkhimer.

No. 3. To Foretell Frost. By James Allen.

No. 4. Use of the Spectroscope in Meteorological Observations. By Winslow Upton, A. M.

No. 5. Work of the Signal Service in Arctic Regions. By Bvt. Maj.-Gen. W. B. Hazen.

Monckton's Practical Geometry. By James H. Monckton. New York: William T. Comstock.

This treatise is not unlike several others which have been constructed for the same general object.

The learner is afforded a series of lessons beginning with the simplest problems in geometry, and carefully graded to embrace many of the problems of projection.

The plates are made to face the descriptive text. There are many inaccuracies in the text, but perhaps no more than usually results from attempts to simplify scientific rules so as to render them acceptable to learners who are not conversant with standard text books.

The book will be found useful by mechanics who wish to begin mechanical drawing by themselves, but in its present form it can hardly be recommended for schools.

DANGERS TO HEALTH. A PICTORIAL GUIDE TO DOMESTIC SANITARY DEFECTS. By T. PRIDGIN TEALE, M. A. New York: D. Appleton & Co.

The pictorial portion of this book is so elaborate that the text is hardly needed. Seventy full-page plates are introduced, and are devoted mostly to the illustration of defects in plumbing arrangements. In order to exhibit plainly the objectionable air and water currents, they are printed in blue.

The book is designed to be of service to householders, landlords, physicians, architects, and health officers.

It is safe to say that all ordinary sanitary defects are so plainly shown by this work, that the ordinary householder may learn how to make a satisfactory examination of the sanitary arrangements of a dwelling.

ELECTRICITY IN THEORY AND PRACTICE, OR THE ELEMENTS OF ELECTRICAL ENGINEERING. By LIEUT. BRADLEY A. FISKE, U. S. N.

The preparation of this book has resulted from a well-founded conviction, that practical men and students have found great difficulty in seeing the relation between the theory of electricity and its practical application, because they have had to study the theory of electricity from books solely devoted to abstruse theory and the applications from books wholly devoted to practical applications. The author has endeavored to supply a want felt by both classes, by preparing a treatise explaining the principles upon which practice depends.

To accomplish this purpose, a brief outline of the theories of magnetism and electricity is first given; then follows an exposition of the principles of electrical measurement, after which, in order, telegraphy, electric lighting, electric machines, motors, electric distribution of power, meters and electric railways.

The book, doubtless, occupies a position not heretofore filled in scientific literature, and we believe will satisfy the want of a large class who earnestly desire to know the relation between the electrical science of an academic course and the practice of electrical engineers.

THE CHEMISTS' MANUAL. By HENRY A. MOTT, JR., Ph. D., F. C. S.—Second Revised Edition.—New York: D. Van Nostrand.

The estimate of the value of this work to the student of practical chemistry, as expressed by Dr. Chandler, and printed as a preface to the first edition, has been fully justified by the exhaustion of the first supply and a demand for the second.

The revision has included such changes in atomic weights, and the addition of such new methods of analysis as the progress of science demanded.

The work includes treatises on both quantitative and qualitative analysis by both wet and dry methods; also chapters on mineralogy, stoichiometry, and physiological chemistry, together with a miscellaneous collection of tables and formulas.

It is a complete ready reference book for the laboratory worker.

MISCELLANEOUS.

FOR the preparation of cathedral glass, flashed or wholly colored, blown or cast glass plates are, under German patent 22,306, coated with a mixture of equal parts pulverized basalt, potash, saltpetre, and calcined borax made into a paste with water and subjected to a red heat after drying. The temperature must be high enough to fuse the coating and soften the glass simultaneously. The cooling is effected in the usual manner.

THE report of Colonel Frank Bolton, the official metropolis water examiner, for September, again draws attention to the way in which the water purified and filtered with every care by the water companies is polluted all over London by the receptacles which are employed under the intermittent supply system. Dr. Frankland's report shows and states the water supply to be good, and "again unusually free from organic matter." It always is "unusually free" now, we are glad to see.

ON the power required to shear hot steel blooms some figures have been given in *Stahl und Eison* by Mr. R. Lauenstein. The shears with which the experiments were made are driven by a 10 by 16 horizontal engine geared one to four and a-half, the stroke of the shears being 9 in., and the dimensions of the bloom $6\frac{3}{4}$ in. square. When the engine is running at a speed of 45 revolutions the power is just sufficient to cut the blooms, the speed of the fly wheel being sensibly affected. When the blooms were not quite hot enough the engine stopped without cutting entirely through the whole bloom. This, therefore, proved to be the minimum limit of speed. From this Mr. Lauenstein calculates that the entire pressure upon the cutting tool of the shears was 125,120 lbs. or 2,746 lbs per square inch of the bloom to be cut.

LIGHT STANDARDS.—M. Monnier, directory of the laboratory installed at Paris (by the gas companies) for the study of electricity, has undertaken a comparison of the existing standards of light. He finds the value of the French carcel to be in English standard candles 8.33, in German candles 7.5, in Munich candles 6.5. The standard equivalent values of those in vogue are for the English 9.5, and German 7.6; therefore some change in this respect would seem to be advisable. Schilling's equivalents to the carcel are English 9.6, German 9.8, and Munich 8.7. On the other hand Schilling's measurements give the corresponding values as 7.77, 7.4, 8.7 There is thus room for a correct determination, and on the face of this disagreement between other observers we are the less inclined to trust to M. Monnier's results. It is high time some definite system of light standards was adopted. What is the Committee of the International Electrical Congress appointed to consider this question doing?

Colonel Bolton says in his September report on the London water: "It has been suggested that the question of a practical 'standard of quality'—including both the organic and inorganic matter contained in water—should be considered and determined by the highest authorities connected with the medical and chemical professions, and when such standard has been settled by these authorities, it will then become the duties of engineers connected with the water companies to work up to such standard, so that the sources of supply of water to the metropolis may be thereby regulated."

Alloys of metals are often difficult to make, and very small quantities greatly affect alloys. The presence of $\frac{1}{30000}$ of a pound of antimony in a pound of melted lead increases the rapidity with which the lead oxidises and burns. Lead which contains more than $\frac{1}{14000}$ of its weight of copper is unfit for the manufacture of white lead. *Der Techniker* says gold with an alloy of $\frac{1}{2000}$ of lead is extremely brittle. Copper with $\frac{1}{2}$ per cent. of iron has only 40 per cent. of the electric conductivity of pure copper. Nickel was regarded as a metal which could be neither rolled, hammered, nor welded, until it was found that the addition of $\frac{1}{1000}$ of magnesium, or of $\frac{3}{1000}$ of phosphorus, makes it malleable. Some varieties of cast steel are exceedingly brittle, but the addition of $\frac{1}{12}$ of 1 per cent. of magnesium makes them malleable. At the Paris Exposition of 1878 a great difference was found in the toughness of sheets which were made of Swedish puddled iron.

In Dr. Frankland's report to the Registrar-General, on the quality of the water supplied to the metropolis during the month of August, it is stated that the so-called "organic impurity" of the river-derived water was from $2\frac{1}{2}$ to $2\frac{3}{4}$ times as great as that of a particular well-water adopted by himself as a standard. As usual, however, Messrs. William Crookes, William Odling, and C. Meymott Tidy, say it is not pointed out, *per contra*, that the so-called "previous sewage contamination" of the standard well-water is more than twice as great as that of the Thames and Lea derived waters. Neither is it pointed out, though shown by Dr. Frankland's own figures, that the so-called "organic impurity" of the river supply of London is appreciably below that of the Corporation of Birmingham's supply, and also, as we have on several occasions shown, of the highly reputed Loch Katrine water supplied by the Corporation of Glasgow. It is not for us to furnish an explanation of the omissions from an official and presumedly impartial report.

Dr. Joule has been experimenting, with a view to counteracting the bad effects produced by the sulphuric acid, which the combustion of ordinary illuminating gas produces in sufficient quantities to destroy the binding of books and to tarnishing the lettering on their backs, besides, of course, vitiating the atmosphere so much that the health of the person breathing it is injured. He suspended two plates of finely perforated zinc, one 3 in. and the other 12 in. above the burner. At the end of three months the lower plate showed an accumulation of the ordinary brownish-black deposit, and a furring of sulphate of zinc, but the upper plate was only slightly affected. "The inference," *Knowledge* says, "from this examination is that a single plate of perforated zinc, about a foot square, placed over a gas jet is sufficient to retain most of the noxious emanations." Such a thing would be a nice-looking ornament, and it might be suggested that the difficulty could also be overcome by using tallow candles, or, perhaps, an electric light.

Protection of Iron from Rust.—The problem of protecting iron from rust is one of perennial interest, and new systems of painting or otherwise treating iron for this purpose are continually being proposed. It has been observed that iron lying still and exposed to the air, as in railways not in actual use, rusts more quickly than when the metal is strongly vibrated by constant traffic. From this it has been inferred that the vibration is attended with an electrical action that decreases the affinity of the iron for oxygen. In tearing down old masonry, iron clamps and bonds are sometimes met with which, when completely bedded in mortar, are as free from rust as when they left the blacksmith's hands.

A French engineer, says the *Polytech Notizblatt*, observed this remarkable effect when uncovering the anchor plates of several chain bridges which had been built for about thirty years. Where the anchors had been covered with the fat lime mortar of the masonry, they showed no sign of rust; but the parts of plates that had been prolonged into empty space were so rusted that two-thirds of their substance had gone. It has been repeatedly observed that iron does not rust in water in which are dissolved small quantities of caustic alkalies, or alkaline earths, which neutralize every possible trace of acid.

These experiences are apparently the bases of a theory propounded by Herr Riegelmann, of Hanau. The paint that he uses contains caustic alkaline earth (baryta, strontia, &c.), so that the iron is in a condition analogous to that of the anchors of the chain bridges already mentioned. Although a thin coat of paint cannot contain so much alkali as a thick bed of mortar, the alkaline action will nevertheless have effect so long as the coating has a certain consistence. Under any circumstances, these new paints will be free from active acids. Riegelmann's paint, moreover, is said to contain a rust-preventing composition which does not require the aid of any alkali in order to effect its purpose. Perhaps this is the same mixture described in the *Neueste Erfindung*, where it is stated that if 10 per cent. of burnt magnesia, or even baryta or strontia, is mixed cold with ordinary linseed-oil paint, and then enough mineral oil to envelope the alkaline earth, the free acid of the paint will be neutralized, while the iron will be protected by the permanent alkaline action of the paint. Iron to be buried in damp earth may be painted with a mixture of 100 parts of resin (colophony), 25 parts of guttapercha, and 50 parts of paraffin, to which 20 parts of magnesia and some mineral oil have been added.